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– and beyond

*Revised edition*

Gideon Toury

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## Descriptive Translation Studies – and beyond

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#### **Volume 100**

Descriptive Translation Studies – and beyond. Revised edition  
by Gideon Toury

# Descriptive Translation Studies – and beyond

Revised edition

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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 corpora, or sets of problems, constitute the best means of testing, refuting, and especially modifying and amending the *theory* itself in whose terms the 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 that 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 adopted in and for the solution of particular problems, what constitutes the subject matter of the discipline of Translation Studies is (whether actually observable or at least reconstructable) facts of real life rather than 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 continuing attempts in recent decades to elevate it to a truly scientific status, as the empirical science it deserves to become, Translation Studies is still only 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 programme: namely, as a vital link between successive phases of its own evolution as well as between the discipline itself and its extensions into the world of our 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, not even those regarded as "Voraussetzungswissenschaften für die Übersetzungswissenschaft" (Kühlwein et al. 1981: 15).

One of the main reasons 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 at least since the nineteen 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, or even as the very reason for its existence. 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, practitioners and their products, the more so if such 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 this behaviour (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 writing that is more oriented towards the applied, which is often unjustly presented as ‘theoretical’, whereas writings of other kinds were subjected to severe criticism on account of the scarcity of examples – as if a handful of quotes torn out of both co-text and context could attest to anything at all. And, in fact, the main consideration underlying the selection, if not the 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; at least by implication, it was also marked as downright unsuitable, and in need of change.

All this is not to say that no attempts have been made to account for actual translational behaviour and its results. However, most descriptive studies have been performed within disciplines other than Translation Studies, such as Contrastive Linguistics, Contrastive Textology, Comparative Literature, *stylistique comparée*, or – in more recent times – Textlinguistics, 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 they were not sufficiently interested in fully accounting 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 becomes more 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 in 1980 and out of print almost from the start. In fact, I have long resisted the temptation to have that book published in a second edition, a temptation that 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 means that such reports are soon out of date. Be that as it may, no particular acquaintance with *In Search of...* is presupposed here. 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. Three of the chapters are offered as excurses: 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 and shading mark shorter digressions within the chapters themselves.

Work on the book has taken quite a while. Over the years, many of the ideas comprising it were discussed 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 gamut 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 some changes, often resulting in complete rewriting. Finally, rethinking and rewriting were also prompted by some of the more serious criticisms levelled against my work, for which I am grateful to dozens of colleagues around 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 firstly to Prof. Miriam Shlesinger, a former student, a fellow translation scholar and a fine editor, and secondly to Prof. Andrew Chesterman, who have given a final touch to my manuscript. I will no doubt come to regret all those (quite rare) instances where I showed obstinacy and refrained from adopting their editorial suggestions!

\*

The book is divided into four parts. Part One 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 my initial decision to devote a book neither to a purely theoretical presentation 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 varying scopes and levels, from a whole historical shift through the translation of single texts to the translational treatment of lower-level entities. Each chapter is self-contained, and can therefore be read in and for itself. However, the framework in which all the studies were carried out and written down lends them a high degree of methodological unity, which links back to the Rationale. The guiding principle here was to tackle each issue within higher-level contexts: texts and modes of behaviour are situated in the appropriate cultural setting, and textual components are contextualized in their 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 theoretical-methodological 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 that have contributed the last part of the book's title.

### A remark on the new edition

The introduction to the first edition was signed in February 1994. Its last paragraph makes the following claim:

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.

The debate I hoped for started right away. Indeed, it is still very much alive today, sixteen years later. I am proud to say that *Descriptive Translation Studies and beyond* played quite a role in this evolution. Indeed, it has not only been reprinted several times in an unchanged version, but it had a special edition made (in English) for the Chinese market as well as a Spanish translation.

It is probably this lively debate, and the fact that it is still ongoing, that motivated the publishers to come up with the suggestion that being unable to produce the next stage of my thinking and study in a new book, I might at least expand the original version and update it a little. I was glad to comply, taking it (perhaps in an exaggerated way) as a vote of confidence of sorts. After all, this is the best most of us will achieve in the profession we have selected for ourselves.

What you are looking at now (and will hopefully go on reading) is just another interim report of a project which has been going on for years. The theoretical aspect has been strengthened with a new chapter on the notion of 'translation problem' (Chapter 2) and the old Chapter 2 has been expanded and divided into two: Chapter 3 on "Norm-governed Activities and Translation" and Chapter 4 on "Translational Norms and Their Study". In addition, throughout the book, many passages have been revised and others have been changed. Finally, some effort has been invested in making the reading smoother without oversimplifying the argumentation. It is therefore basically the same book, but another one.

Tel Aviv, July 2011





## PART ONE

# The pivotal position of Descriptive Studies and DTS

It has been almost forty 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 he envisioned a semi-autonomous discipline which would cater for the whole “complex of problems clustered round the phenomenon of translating and translations” (Holmes 1988:67), which – for very sound reasons (pp. 68–70) – he purported to call Translation Studies.<sup>1</sup> He then went on to outline the structure of the discipline, a compelling admixture of observations on what it was like, in the 1970s, with a penetrating vision as to the form it should, and would eventually, assume.

Until that time, very little effort had been invested in “meta-reflection on the nature of translation studies” (p. 71). It can thus be assumed that Holmes’ piece would have played a much more substantial role in channelling the development of the discipline, had it not been for the unfortunate fact that, for many years, it remained virtually unknown. Actually, for the first fifteen years, the paper’s full text existed only as a mimeographed pre-publication (Holmes 1972b) – a pamphlet which, paradoxically enough, was only available to the initiated, those who were already interested enough in the suggested approach to take the initiative and contact the author 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, most certainly not on an international scale. A personal wish to bring these ideas

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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) or Science of Translation (Nida 1969; Bausch, Klegraf and Wilss 1970, 1972; Harris 1977b; Wilss 1982), let alone Translatology (Harris 1988a). Unfortunately, the confusion which was thus cleared up has been superseded by a new complication: several universities in Europe have renamed their translation departments ‘Departments of Translation *Studies*’, despite the fact that most of the time and energy is invested in the teaching and exercising of translation (including interpreting) as a *skill*, rather than in research or training for research.

to the attention of a wider audience underlay my own decision to reprint Holmes' original text in a special issue of the *Indian Journal of Applied Linguistics*, devoted to translation, which I was invited to guest-edit at the end of the eighties (Toury 1987: 9–24). This action was however not much help either, given the peripherality of the periodical and the scarcity of the book edition of that special issue, although it did enjoy a second edition (1998).

Be that as it may, the sad fact is that, for more than twenty years, Holmes' visionary paper was hardly ever mentioned by other authors, especially in books and articles which became standard works in the field,<sup>2</sup> nor could any direct signs of its influence on other scholars be traced. Thus, despite the incontestable unfolding of Translation Studies over the past decades, including the (re)discovery of the original paper when it was included in *Translated!*, the posthumous collection of Holmes' papers so meticulously edited by Raymond van den Broeck (Holmes 1988: 67–80), a complete realization of his vision is still a long way off. What was achieved, especially after the James S. Holmes Symposium on Translation Studies, held in Amsterdam in 1990,<sup>3</sup> was its adoption as a kind of *Orientierungskarte*, a grid on which individual approaches to the study of translation, of which there are quite a number, could be situated and their (inter)relations noted. However, the 'map' itself still represents more of a desideratum than a reality. Lately, some other maps have also been suggested, most notably the 'conceptual map' which was put together during the planning phase of the online *Translation Studies Bibliography* (<http://www.Benjamins.com/online/tsb>; e.g., van Doorslaer 2005, 2007). A number of overt or covert criticisms (e.g., Pym 1989: 2–3; Snell-Hornby 1991; Chesterman 2009) notwithstanding, Holmes' conceptualization is still the best, most fruitful one. Let me therefore present it once again in some detail.

---

2. 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 a one-page abstract thereof (Holmes 1972a), and does not really tackle any of Holmes' ideas.

3. Several speakers in that conference, which was held a short while after the publication of *Translated!*, indeed 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. In the years that have elapsed, recourse to Holmes has become much more common.

## 1. Holmes' 'map' of the discipline

For me, the main merit of Holmes' programmatic presentation lies in its notion of **division**: not as a mere necessary evil, that is, but as a **basic principle of the very organization of the discipline**, implying as it clearly does a proper division of labour between various kinds of scholarly activity having different foci. The division itself in the form it was suggested takes after adjacent disciplines, most notably Linguistics (e.g., Fowler 1974: 33–37), and is in full keeping with Holmes' conviction (1988: 71) that Translation Studies was on its way to becoming an **empirical science**:<sup>4</sup> 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' overview of Translation Studies and its divisions.

Of course, Holmes was scientifically-minded enough to realize that a 'flat' presentation such as his 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)

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

---

4. To cite one famous formulation of the objectives of empirical sciences, the opening passage of Carl Hempel's 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" (Hempel 1952: 20).

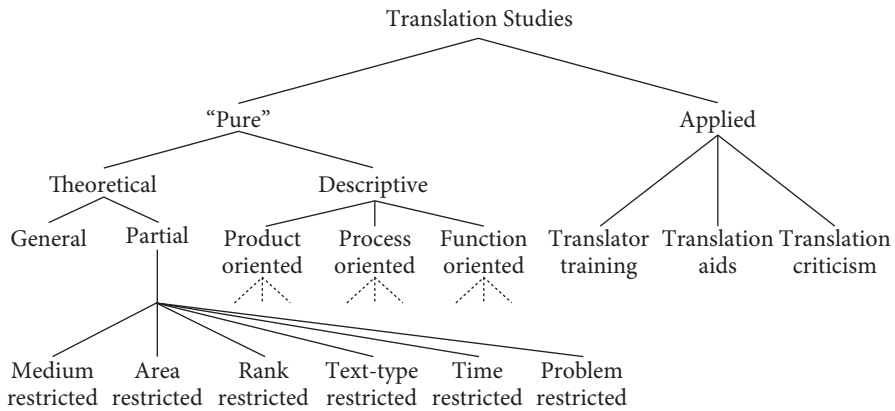


Figure 1. Holmes' basic 'map' of Translation Studies

The rest of this chapter will address itself to precisely this aspect, which is of utmost importance for scholars wishing to locate themselves in the middle ground 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 branch – in full keeping with Holmes' reasoning, I would presume. The basic question I shall be pursuing will thus be as follows:

**It is very clear that individual studies into translation are bound to yield isolated descriptions, an obvious result being a gradual accumulation of discrete pieces of knowledge. But what kind of contribution could descriptive studies carried out within DTS be expected to make to the discipline at large?**

## 2. The organization of DTS<sup>5</sup>

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

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5. 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 (which may or may not be within DTS). Needless to say, my main concern throughout will be with research procedures pertaining to DTS.

Thus, it is certainly true that three approaches – function-, process- and product-oriented – are not just *possible*, but *justified* too, so that each one of them delimits a legitimate field of study of its own. However, to regard each of the three as a field in itself is a sure recipe for reducing the studies to superficial **descriptions** – whether of a translation's position in the culture in which it is or will be embedded; of the process through which a translation is derived from a so-called source text; or of the textual-linguistic make-up of a translation (or aspects of/phenomena within it), along with the relationships that tie it to its source and/or the 'shifts' which are manifested by the one vis-à-vis the other. Once **explanations** are also sought – and no study deserving of the name can afford to dispense with attempting them, at the very least – the picture is bound to change considerably. After all, no hypothesis which is even remotely explanatory can be formulated unless all three aspects have been brought to bear on each other.

In fact, to the extent that the descriptive branch, DTS, aspires to offer a framework for individual studies of all kinds, at all levels, there is no escape from proceeding 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 except for methodical (and, yes, convenience) purposes**. Consequently, whether an individual study is process-, product-, or function-oriented, when it comes to the global level, that of the discipline as a whole, the programme 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 framework.

Seen as such, individual studies of whatever denomination emerge as a two-fold enterprise: each one is a local activity, pertinent to a particular corpus, problem, historical period, or the like,<sup>6</sup> 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.

Attempts to apply experimental methods to the study of translation (see Chapter 14) have certainly shed new light on the need to account for those tripartite interdependencies, as no significant conclusion can be drawn from an experiment unless all parameters which are deemed relevant have been established, along with their interrelationships. Moreover, control over as many parameters as possible is a precondition for the execution of a proper experiment, not to mention the (relative) replicability of one, which renders the need to account for the variables and their interrelations all the more pressing. And the fact that very

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6. One cannot but wonder why Holmes neglected to duplicate his division of the *theoretical* branch into 'partial theories' in DTS. This lack is corrected by adding optional sub-branching to each descriptive branch in Figure 1.

often this need is not taken seriously has been, to my mind, a major impediment to the development of this promising brand of descriptive-explanatory research.

Thus, the [prospective] position (also called ‘function’<sup>7</sup>) of a translation within a 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 do not come into being in a vacuum. Not only is the act performed in a particular cultural environment, but it is designed to meet certain needs there, and/or occupy a certain ‘slot’ within it. **Translators may therefore be said to operate first and foremost in the interest of the culture into which they are translating**, whichever way that interest is conceived of. In fact, the very extent to which features of a source text are retained in a particular translation thereof, or even regarded as requiring retention in the first place (which may at first sight seem to suggest operation in the interest of the *source* culture, if not of the source *text* itself), is also determined on the target side, and according to its concerns. Features are retained, and recast in TL material, not because they are ‘important’ in any inherent sense, but because they have been *assigned* importance: namely, 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 an SL (source language) text ‘invariant under transformation’ is considered a necessary condition for a translation to fulfil the function allotted to it in the target system.

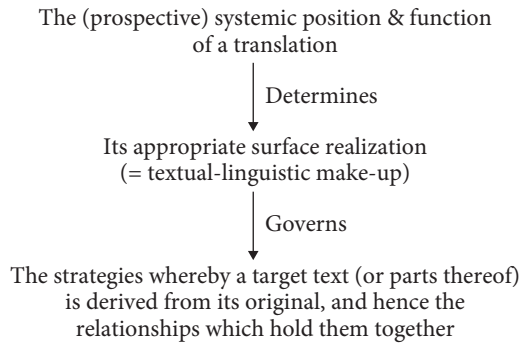
It is the prospective function of the translation, via its required textual-linguistic make-up and/or the relationships which would tie it to the original, which yields and governs the strategies which are resorted to during the production of the TL (target language) text in question, and hence the translation act as a whole.<sup>8</sup> This logic, summarized in Figure 2, has important implications for any research which is carried out within DTS. Thus, there is no real point in a product-oriented study if questions pertaining to the determining force of its intended function, and to the strategies governed by the norms of establishing a ‘proper’

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7. I use the term ‘function’ 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, with other constituents as well as the system as a whole (see, e.g., Even-Zohar 1990: 10). As such, it is not tantamount to the mere ‘use’ made of the end product, as seems to be the case with *Skopostheorie* (e.g., Vermeer 1986) or *Handlungstheorie* (e.g., Holz-Mänttari 1984), let alone more naïve approaches (e.g., Roberts 1992). The different uses of ‘function’ may well correlate, but this particular aspect still awaits scholarly processing.

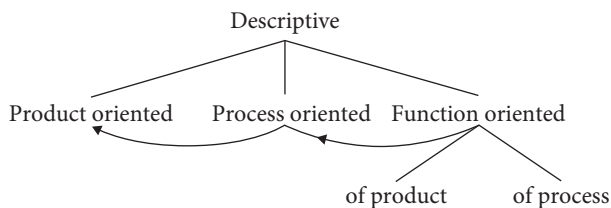
8. Even with respect to the gradual emergence of a translator under ‘natural’ circumstances – that is, outside any schooling system – environmental feedback greatly influences the strategies resorted to in the act of translation, thus giving them a considerable degree of uniformity across a societal group. This issue will be taken up towards the end of the book (see Excursus C).

product, are not taken heed of. Similarly, there is little point in a process-oriented study of whatever type, unless it incorporates the cultural-semiotic conditions under which it occurs.



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

Finally, it is not translations alone (as textual-linguistic entities) whose position in the hosting culture is changeable. Translating itself, as an institutionalized mode of text generation, may also assume varying positions, in terms of e.g. centrality vs. peripherality, prevalence vs. rarity, or high vs. low prestige. This variability with 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 respective sources.



**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 seen as affecting the position of the end-product in the recipient system. However, under the semiotic perspective we have adopted throughout (see fn. 7 above), functions



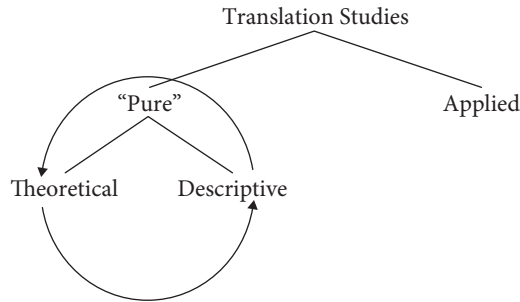
should be regarded as always having at least *logical* priority over their surface realizations (also called ‘carriers’, or ‘functors’). This idea has been a cornerstone of Dynamic Functionalism (Even-Zohar 1990; Sheffy 1992) ever since the late 1920s and early 1930s, when it was formulated by scholars such as Jurij Tynjanov, Roman Jakobson and Pëtr Bogatyřev. Once such a perspective has been opted for, the reversal of roles is no longer viable. Since translating, like any other communicational activity, is *teleological* by its very nature, its systemic position, and that of its future products, should be taken as forming constraints of the highest order.

This principle does not lose any of its validity when the position actually occupied by a translation in the target culture, or its ensuing functions, is found to differ from the ones it was initially designed to have; for instance, when the translation of a literary work, intended to function as a literary text too and translated in a way which is deemed suitable for attaining that purpose, is nevertheless rejected by the target culture, or relegated to a position which it was not designed to occupy (e.g. a children’s book or a piece of journalism). One of the objectives of descriptive studies is precisely **to confront the position a certain translation (or group of translations) has actually assumed in the host culture with the position it was intended to have**, and offer explanations for the perceived differences.

### 3. Between DTS and Translation Theory

One point should be made very clear: the programme I wish to present pertains to Translation Studies *as a discipline* rather than to the immediate concerns of any *individual* researcher, let alone every single attempt to tackle translation in a scholarly way. Needless to say, such a programme would cut across Holmes’ distinctions. Not only will it not be located within any single sub-branch of DTS, but it won’t be confined to DTS to begin with, given that one of the aims of the discipline is to bring the results of studies executed within DTS to bear on the *theoretical* branch.

This is not to say that every single study is, or even should aspire to be, performed with a view to revising the theory in whose terms it is executed. Still, inasmuch as a study is well-performed, its findings will always bear on the underlying theory. Whether the theoretical implications will be drawn by the researcher her/himself or by some other agent, most notably an empirically-minded theoretician, they will inevitably 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 in Figure 4.



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

To be sure, the status of both the principle of interdependency and the priority of functions over carriers is *theoretical*. However, as soon as *concrete* relations between function, process and product are laid bare by an individual study and brought into the game, the course taken by the discipline is bound to be affected. After all, Translation Studies as a whole is called to tackle fully and systematically three types of issues which, although related to each other, differ in scope and level:

1. all that translation CAN, in principle, involve;
2. what it DOES involve, under particular 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.

Although level (1) yields a truly theoretical entity, in terms of a theory of *translation* it is most elementary – a mere co-ordinate system which makes it possible to account for anything connected with translating and translation. Level (2) is, of course, tantamount to DTS's programme. Yet the significance of studies of this kind lies not only in the possibility of supplying exhaustive descriptions and tentative explanations of instances of actual behaviour. No less important are their implications for the discipline at large. Thus, when the initial potentials subsumed under (1) have been modified by diversified factual knowledge accumulated in actual studies under (2), only then will ample grounds have been furnished for making some 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.

In the long run, 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 that will have been found relevant for translation. Lying as it does beyond descriptive studies as a scholarly activity and beyond DTS

as a sub-discipline, the formulation of laws of this kind may be taken to constitute the ultimate goal of the discipline in its theoretical facet.

The laws as we envisage them are anything but absolute, designed as they are to state the *likelihood* that a certain kind of behaviour, or surface realization, would occur under a particular set of conditions. Needless to say, not even one conditioned law can be formulated unless the conditioning factors have been identified and specified. What the formulation of laws thus presupposes is the establishment of *regularities of behaviour*, along with maximal controllability of the parameters of function, process and product.

Looked at from a slightly different angle, such an evolutionary process would entail a gradual transition from Holmes' *partial* theories of translation, which show large areas of overlap anyway, to a *general* theory thereof. This kind of 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, relationship, etc.** Among other things, a theory thus refined will make possible the performance of yet more elaborate descriptive-explanatory studies, which will in turn bear back on the theory, making it even more intricate; and so on and so forth.

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 and its results that hypotheses can be not just formulated, but actually be put to the test. In fact, even if a study doesn't have such testing as an explicit goal – which it most certainly doesn't have to have – this will inevitably wind up being an important concomitant of the study; the more so if constant heed is paid to the underlying theoretical assumptions and to how the methods used actually derive from those assumptions and are answerable to them.

#### 4. Between Translation Studies and its applied extensions

As was to be expected, the bulk of this chapter has been weighted towards the internal structure of DTS, on the one hand, and 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, which we adopted as our starting point (Figure 1), was between the 'Pure' and the 'Applied', so that an introductory chapter which sets out to impose better order on the field cannot afford to ignore the relationships between those two altogether.

Thus, it is precisely one of the advantages of the kind of laws envisioned in the previous Section that they may be projected onto the applied extensions of the discipline with relative ease, and may make possible the elaboration of these extensions in a way which is much closer to reality, hence enhancing their chances of being successfully implemented.

It has always been my conviction that **it is no concern of a scientific discipline, not even within the ‘human sciences’, to effect changes in the world of our experience.** Thus, as should have become clear, I would hardly subscribe to the view 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 doesn’t preclude the possibility of drawing conclusions from theoretical reasoning, or scientific findings, to actual behaviour, be its orientation retrospective (e.g., translation criticism) or prospective (e.g., translator training or policy making). This possibility does exist, of course. However, drawing such conclusions is up to the *practitioner*, not the scholar. It is up to these practitioners to bear the consequences too, and they might just as well be ready to take full responsibility rather than blame theorists (or the ‘theory’ as such) for their own blunders in the ‘practice’, as is all too often the case.

Mentioning practitioners, in this context, those I have in mind are first and foremost those who indulge in *applied activities*, e.g., critics, teachers and policy-makers rather than practising translators, unless they wish to train for the profession in a fully conscious way. Translation is, of course, the object of Translation Studies in all its branches, and not an ‘application’ or ‘extension’ 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, even though it is certainly connected to them.<sup>9</sup>

Needless to say, there is no direct transition from Translation Studies proper to any of its extensions into the world. Even less can the derivation of one from the other be regarded as automatic. Rather, any such transition would necessitate the application of some *bridging rules* – a fact which renders the relations between Translation Studies and its applied extensions slightly different from all the relations previously discussed, as indicated by the use of a different type of arrow in Figure 5. Furthermore, the bridging rules are bound to be different for different types of application, and, at any rate, none of them will 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 bridging rules

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9. 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*.

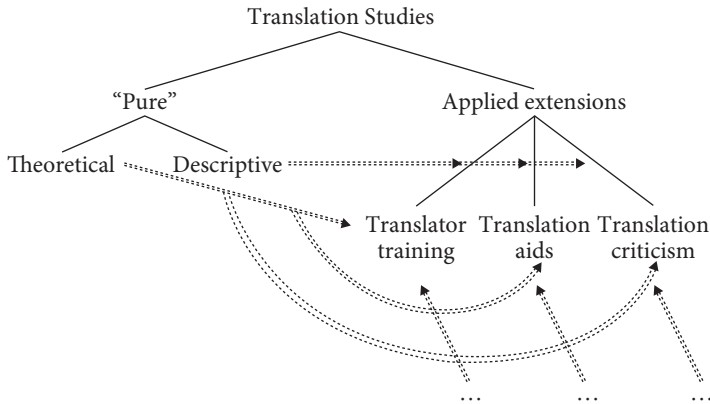


Figure 5. The relations between Translation Studies and its applied extensions

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 concepts would probably be of very little relevance to an extension such as policy-making, 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, the applied extensions cannot be anything but *prescriptive*. This is so 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 actual facts, but rather to set norms in a more or less conscious way. In brief, to tell others what they should have done or should be doing, if they accept these norms (or, very often, acknowledge 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 kind of verbs that are typically used in them. Thus, each branch is characterized by verbs of different categories, and their actual use in 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 relationship 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 typical verbs

Type of relationship	Criterion (or type of condition)	Typical verbs	Branch of Translation Studies
possible	theoretical	<i>can be</i>	translation theory, basic
probable	conditional	<i>is likely to be</i>	translation theory, modified
existing	empirical	<i>is</i>	DTS
required	postulated	<i>should [not] be</i>	applied extensions of Translation Studies

We will return to the implications of the structure of the discipline from a different angle, that of the notion of ‘problem’ and its position in expert discourse on translation, after we have elaborated on the status of translation and translations as facts of so-called target cultures.



## 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 case. 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 Chapter 1, the theoretical question of what would constitute an object of study within a target-oriented approach to translation will be submitted to detailed scrutiny. A separate discussion (Chapter 2) will then be devoted to the presentation of three senses in which the notion of ‘problem’ may feature in professional discourse about translation, a logical expansion of the distinction between different levels (or types) of object in the field. This chapter, in turn, will be followed by a methodological overview of discovery vs. justification procedures in descriptive studies of translation. As a kind of *intermezzo*, we will take a brief excursion into so-called pseudotranslations and their possible relevancy for Translation Studies (Excursus A). Finally, a couple of chapters (3–4) will be devoted to the key notion of ‘norms’ and its relevance to translational behaviour, and to how norms may be reconstructed and studied.

At this point, we will make a small U-turn and go back to the research method. Here (Chapter 5), an attempt will be made to apply a first layer of flesh to the bones presented 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. We will then zoom in on one aspect of the method: the type of unit that the comparative part of a study would be applied to, along with some of its justifications (Chapter 6).

To wind up the preliminary discussion, a step-by-step presentation of an exemplary case study will be offered (Chapter 7), proceeding from a linguistic phenomenon of one basic type – conjoint phrases of [near-]synonymous lexemes – in its recurring use as a translational replacement in one particular tradition, and progressing toward generalizations of an ever higher order. The intention here will not be to unfold the study itself, in all its ramifications, but to highlight the ordered movement from one stage to the next, as an illustration of the research method; hence its modest characterization as a ‘study in descriptive studies’.





## Translations as facts of a ‘target’ culture

### An assumption and its methodological implications

The first question that suggests itself concerns **the range of objects of study in the framework of DTS**: where would the line be drawn between what is and what is not ‘in’? How are we to determine what would be taken up and what would be left out? The current state of research in Translation Studies makes these questions difficult to tackle, let alone answer. On the one hand, today’s discipline is a remarkably heterogeneous series of loosely connected paradigms<sup>1</sup> while, on the other, there is an overriding tendency to regard different paradigms as mere alternative ways of handling ‘the same thing’. Which they are not, nor can they be expected to be.

As is well known, establishing an object of study is never a neutral procedure. Rather, it is a function of the *theory* in whose terms it is constituted, which is always geared to cater for particular needs. Its establishment and justification are therefore intimately connected with the *questions* one wishes to pose, the possible *methods* of dealing with the selected objects with an eye to exploring those questions – and, indeed, the kind of *answers* which would count as admissible. Thus, the question we face is not really what the object of translation studies *is* (in itself, so to speak), but rather what would be *taken to constitute* such an object, in pursuit of a certain set of goals. Evidently, any change of approach will entail a change of object, and the other way around. This is so even if various approaches superficially fall under the same heading: it is not the *label* that counts, but the *concept* it covers; and concepts can only be established within conceptual *networks*.

#### 1. Approaching translation within a target-oriented framework

Indeed, translation scholars of different denominations all use the words ‘translation’ and ‘translating’. Many also make use of labels such as ‘transfer’ and ‘translational relationships’, ‘equivalence’ and ‘adequacy’, ‘translation problem’

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1. And see, in this connection, Chesterman’s 1997 account of the conceptual heterogeneity of the discipline and its history in terms of changing ‘memes’.

and ‘solution’, and many more. However, not only are the contents of recurring words in their use as terms given different characterizations in their respective terminological systems, but there are also considerable differences between approaches as to how the object of study would be delimited, to begin with, and what objects would therefore count as legitimate. Unfortunately, the fallacious rejection of somebody else’s concepts on the grounds that they are untenable within one’s own frame of reference just because they seemingly bear the same name is still common practice. (The most striking case is probably the case of the label ‘equivalence’.)

It will be recalled that the mainspring of the present endeavour was the conviction that the position and functions that translations (as entities) and translating (as an activity) are designed to have in a prospective target culture, the form a translation would have (and hence the relationships that would tie it to its original), and the strategies resorted to during its production constitute an ordered set rather than a mere congeries of disconnected facts. Having accepted this as an axiom, it is *interdependencies* that will be the focus of our attention, the main intention being to lay bare the regularities marking the relationships assumed to obtain between function, product and process.

The crucial step taken in pursuit of this goal is the suggestion that **translations be regarded as facts of the culture that would host them**, with the concomitant assumption that whatever their function and systemic status, these are constituted within the target culture and reflect its own systemic constellation. It was by virtue of its starting point that this approach was described as ‘target-oriented’.

When it was first put forward, the target-oriented frame of reference for the study of translating and translations in their immediate contexts was considered somewhat unorthodox, and its initiator something of an *enfant terrible* (Katharina Reiß, personal communication). At that time, back in the 1970s, Translation Studies was still strongly marked by *source* orientedness, and the different scholarly paradigms were basically *application*-ridden. Whether concerned with training or quality assessment, they were mainly preoccupied with the proclaimed protection of the SL text rights. Thus, translations were approached first and foremost as representations of previously existing texts that were in a language/culture other than that of the target. To be sure, constraints originating in the target culture were never totally disregarded. They have, however, been seen as *subsidiary*; especially those constraints which did not fall within Linguistics in its narrower sense. Many of the factors that affect translational behaviour in real-life situations, along with the fact that these factors have engendered different

translation traditions, were resented, or, at best, relegated to the realm of history, which was regarded by many as a marginal field of study.<sup>2</sup>

In the years to follow, most translation scholars, while not abandoning the seemingly safe haven of the source text, have at least come to integrate more and more target-bound considerations into their reasoning. In addition, a second paradigm, which was heavily target-oriented, was introduced into the field. This paradigm, which has become known as *Skopostheorie*, has gained considerable footing and has left its mark on the discipline, albeit almost exclusively in German-speaking circles, first and foremost in former West Germany, since its writings were mostly confined to this area. As a result, target-orientedness as such no longer arouses the same antagonism as it did just a few decades earlier.

Interestingly, the first formulations of *Skopostheorie* by Hans J. Vermeer (e.g., 1978) almost coincided with the beginnings of my own realization that a switch to target-orientedness was imminent (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, moving as we did in different academic and institutional circles.<sup>3</sup>

Finally, our roads crossed, and the two of us identified some common grounds right away. The shared elements notwithstanding, there remained at least one major difference between the interests of the two target-oriented paradigms, which also accounts for the different assumptions each of them has chosen to proceed from: whereas mainstream *Skopos*-theorists still see the ultimate justification of the frame of reference they are busy establishing in developing a more true-to-life way of dealing with problems of an applied

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2. In my view, no translation can be fully (or accurately) accounted for outside of its position in history. Thus, it is impossible for two translations whose textual-linguistic make-up is identical but which were produced in different socio-cultural and historical environments to ever count as a single translation. By the same token, any account of an instance of translation that is wrongly located in space or time (a common error of students, among others) is bound to be misleading and result in shaky or wrong accounts. I therefore fail to understand criticisms such as Pym's (1998), who has argued that target-oriented studies of translation have "neglected" history and its study. By contrast, I fully endorse Delabastita's 1991 claim that the opposition between theoretical and historical approaches to translation is utterly false.

3. An interesting attempt to associate target-oriented thinking on translation, especially of my brand, and some of the basic ideas of another contemporary approach to translation, Deconstruction, was made by van den Broeck (1990). While I would not endorse all his claims, it is certainly an intriguing article for anybody interested in the way scholarly paradigms change. In this connection, see also the attempt to compare my approach to those of Antoine Berman (Brownlie 2003) and Paul Ricœur (Weissbrod 2009).

nature, the main object being to make ‘improvements’ (i.e., changes!) in 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 presented/regarded as translational within a target culture, on the way to making some generalizations regarding translational behaviour. Recent attempts to conduct historical studies 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 didactics (e.g., Toury 1980b, 1984b, and especially 1992), on the other, indicate that the gap may have been narrowing.

This tendency is also manifest in the recent work of some second-generation *Skopos*-theorists, most notably Christiane Nord (e.g., 1991), who has made an interesting attempt to integrate a version of the notion of ‘translational norms’, so central to my own reasoning, into an account that is basically Vermeerian. Unfortunately (from the point of view of DTS), while doing so, Nord (re)introduced the concept of ‘loyalty’, and as an a priori *moral* principle at that, which lends privileged status to what we would call ‘adequacy’. This may well be opening a new gap between the two approaches as the old one seems to have been closing. Although it may be too early to say for sure, the appearance in the last few years of third-generation *Skopos* theorists (see, e.g., Dizdar’s review of the first edition of the present book [Dizdar 2000] and her comparative presentation of the two target-oriented paradigms in Dizdar 2006: 282–330) seems promising again.

My own programme has not fared too well either, the prevailing tendency having been to read its claims through the glasses of other approaches rather than in its own terms. As a result, many of my arguments were grossly misperceived. In view of the misunderstandings that have emerged, I find it advisable to dwell a little longer on the target orientedness of the approach in view of the type of studies envisaged within it and their ultimate goals.

## 2. Translations as cultural facts

Strange as it may sound to the uninitiated, there is nothing perverse in claiming that a text’s position and functions, including those that go with a text’s being regarded as a translation, are determined first and foremost by considerations originating in the culture that would host it. For one thing, 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 why it is so easy for fictitious translations (which will be singled out in Excursus A) to pass for genuine ones. By

contrast, when a text is presented as having been originally composed in a language/culture, reasons will often reveal themselves – including certain features of textual make-up and verbal formulation, which persons-in-the-culture have come to associate with translations – to suspect, correctly or not, that the said text has in fact been translated.

It follows that 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, has the advantage of not imposing on its object any distinctions that may prove alien to that culture. It thus allows one to proceed with very few assumptions that could be difficult to maintain in the face of real-world evidence.

It seems clear that there is no way for a translation to inhabit the same space as its source, not even when the two are physically presented alongside each other, as in bilingual editions. This is not to say that, having been severed from its source, a translation will never be in a position to bear on the source culture again, or even on the source text itself. After all, culture contacts may operate in both directions (Redfield et al. 1936). And indeed, texts, and hence the cultures that host them, are known to have been affected by translations of these selfsame texts, and even revised on the basis of these. It is nonetheless significant that whenever this occurs, it always involves a **reversal of roles**, in full accordance with our starting point: while *genetically* a translation, the affecting entity no longer *functions* as one.

Nor is it just *any* translation that would be in a position to exert influence on its original. Rather, such a translation is always a fact of a particular (target!) culture, which – for that very reason – is regarded as privileged. The fact that translated texts often serve as a point of departure for further acts of translation, into other cultures/languages, is no refutation of the target-orientedness of our assumption either: while, in such instances, a translation does function as a source text, it does not really act as one. Rather, 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.<sup>4</sup>

On the other hand, translation activities and their products not only can, but very often do cause changes in the *target* culture. Indeed, it is in their very nature. After all, cultures resort to translating precisely as a **way of filling in gaps**, whenever and wherever such gaps may manifest themselves: either in themselves, or (more often) in view of a corresponding non-gap in another culture that the target

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4. The significance of mediated translations in the framework of a target-oriented approach will be addressed in Chapter 9.

culture in question has reasons to look up to and try to exploit for its own needs. Semiotically speaking, **translation is thus as good as initiated by the target culture**. What this means is that the initial state is one of deficiency in the recipient culture, even if sometimes – e.g., in so-called ‘colonial’ situations – an alleged gap may be pointed out for that culture by a patron of sorts, who also purports to know better how the gap may best be filled. In fact, even here, a more persuasive rationale is not the existence of something in another culture/language in itself, but rather

**the observation that there is something ‘missing’ in the target culture which should rather be there and which, luckily, already exists elsewhere, preferably in a prestigious culture, and can be taken advantage of.**

In the simplest of cases, both void and fill-in can be reduced to mere **textual entities**. Being an instance of performance, every text is of course unique; it may be more or less in tune with existing texts and prevailing models, but **in itself it is a novelty**. As such, its introduction into a culture always entails some change, however slight, in the latter. The novelty claim still holds for the *n*<sup>th</sup> translation of the same text into a given language: it is the *resulting* entity, the one that would actually be introduced into the target culture, that is decisive here; and in all cases, it will never have 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 (re)production of previously existing translations. Even alternative translations into one and the same language, whether produced at the same time or at different points in time, are not likely to occupy exactly the same position and fulfil the same functions in the culture that hosts them. This in itself is reason enough why **no translation should ever be studied outside of the context in which it came into being**. Any other kind of observation would be a mere mental exercise leading nowhere.

In more complex cases, whole **models** for the establishment of acceptable texts may be imported into the culture. Such a migration normally involves groups of texts which realize the same pattern or else are translated in ways that bring them closer to one another.

The likelihood itself of causing changes in the target culture beyond the mere introduction of an individual text stems from the fact that while translations are indeed intended to cater for needs felt in a target culture, be they real or presumed, they also tend to deviate, on some level, from its sanctioned patterns, not least because of the postulate of **retaining ‘invariant under transformation’ certain features of the source text** – a defining characteristic of translation, no matter how it is realized. This tendency often renders translations quite distinct from non-translations, and not necessarily as a mere production mishap either. In fact, it is not unusual for an amount of deviance from normal patterns to be regarded

not only as *justifiable*, or even *acceptable* in translations, 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 linguistic make-up of the texts – are not necessarily a cause of concern for 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 special systems (Dressler 1972), even 'genres' (James 1989: 35–36) of their own in the target culture. What is totally unthinkable is a translation that hovers in limbo between cultures, sometimes referred to as an 'interculture' (most notably by Pym; e.g., 1998; 2000). As long as a (hypothetical) interculture has not developed into an autonomous (target!) systemic entity, e.g., in processes analogous to pidginization and creolization, it is necessarily part of an *existing* system. A *target* system, to be sure.

### 3. In need of proper contextualization

Bringing all this to bear on our point of departure, the target-orientedness of translation, this assumption can be reformulated to read as follows:

**Translations are facts of target cultures; on occasion facts of a peculiar status, sometimes 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 for states of affairs of this kind would require a proper **contextualization**, which is always specific to a given case and never adequately evident already. Rather, the establishment of the position of translations in a culture forms an integral part of the study itself. 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:** namely, that (sub)system which proves to be best equipped to account for it in terms of product, underlying process and function, in all their multifarious interconnections.



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 (Lambert and van Gorp 1985), may be no more than tentative, or temporary; it may often have to undergo revision as the study goes on – on the basis of its own interim findings, i.e., in a process of continuous negotiation. Above all, one should take care not to fall into the trap of assuming that the identity of the (sub-)culture that hosts texts assumed to be translations is known just because we know what *language* it was that the translator intended to use. Seemingly an easy way out of many a dilemma, the identification of language and culture often proves misleading, the more so as the exact status of the TL itself may have to be reconsidered in the course of the study. For instance, the language that the Septuagint is formulated in – is it ‘Greek’, ‘Jewish Greek’, or maybe an ad hoc mixture of several varieties that never existed as a language in itself? (See e.g. Toury 2006.)<sup>5</sup>

To take an extreme example: one of the four versions of the note warning passengers on German trains against improper use of the emergency brake looks like an intended utterance in English. In spite of the fact that it is officially presented merely as *physically* parallel to the other three versions, in German, French and Italian, there are enough reasons to regard them as functionally equivalent too. Moreover, there are sufficient indications for tentatively regarding the English text as a *translation*, presumably of the German version.

At the same time, the text does not appear to pertain to any of the many cultures where English is used as a language. These cultures all have codified versions of the warning in their repertoires, all different from the one we have here. Finally, to the best of my knowledge, it does not conform to any of the English versions used in other non-English speaking societies. Ignoring the untenable possibility that this text does not belong to *any* culture, there is no escape from regarding the English notice as situated in the *German* culture, albeit in a very specific sector of it where the English language is assigned a role.

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5. An interesting case of cooperation: a group of Septuagint scholars led by Prof. Albert Pietersma have recently attempted to check the applicability of the basic assumptions of DTS to studies in their own field. At the Twelfth Congress of IOSC, the International Organization for Septuagint and Cognate Studies, held at Leiden in July 2004, a special Panel Discussion was organized under the title of “LXX and Descriptive Translation Studies – Making the Connection”. The papers presented in the Panel were published in Volume 39 of the *Bulletin of the International Organization for Septuagint and Cognate Studies* (2006).

Thus, the system best suited to host the text is owned by the virtual group of speakers of various languages who also have at least some English but no German, French or Italian, for as long as they are in Germany (or, rather, on board a German train). This is the only attribution that ensures a satisfactory explanation of the linguistic make-up of the text and the (reconstructed) strategies utilized by its producer, to be submitted to closer analysis in Chapter 6. Moreover, this is not the only situation of its kind. Indeed, on German trains and elsewhere, there are more sets of notices of the same kind. Methodologically speaking, this fact increases the explanatory power of our hypothesis.

Proper contextualization also involves a heightened differentiation between translational items pertaining to one and the same culture: namely, in terms of their respective positions within it. As already indicated, not even two translations of a single text can occupy exactly the same position. Nor would a translation's position in the target culture ever be a mere reflection of the position of the original in its own cultural setting. If differences of textual-linguistic make-up, or of relationship to the shared source text, are to receive viable explanations, the position appropriate to each translation will have to be *established* rather than presupposed; and the reconstructed context will have to be taken into account in all seriousness.

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**. After all, this is the only position that may be claimed to have directed the translation act and the decisions made in the process. However, it is only towards the end of a study that such an intended position can be established with reasonable certitude. This would be achieved by weighing 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 translation tradition in which it came into being and of which it became part. Consequently, rather than being a 'fact', the status of such a positioning would be that of an *explanatory hypothesis* for descriptive findings.

Also significant is the possibility that translations, while retaining their status as facts of the target culture, may nevertheless see their position in it change over time. What is important to bear in mind is that changes of this kind have no bearing on the *intended* position of the translation or even the position it *initially* had in the target culture. They can often shed light unexpectedly on preferences pertinent to *later* periods of time, and some of them may prove relevant to our understanding of translation as performed in those periods.

#### 4. The notion of ‘assumed translation’

In most paradigms of Translation Studies, a **definition** of translation would have long been given – a list of features, more or less fixed, 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 leave behind the discussion of idealized notions and account for **real-life phenomena in their immediate contexts** instead; indeed, they tend to block rather than encourage and foster research.

Thus, any definition, especially if couched in essentialist terms, specifying what is allegedly ‘inherently’ translational, would involve the untenable pretence of fixing, once and for all, the boundaries of a kind of object that is characterized by its inherent **variability**:

- difference across cultures,
- variation within a culture, and
- changes over time.

Not only would the field of study shrink considerably, in comparison to what cultures have been, are, and probably will be willing to accept as belonging to the field of translation, but research limited to such boundaries would probably breed *circular reasoning*: the logical fallacy in which the proposition to be proved is assumed in one of the premises.

Thus, to the extent that the definition offered is indeed adhered to (which is not an easy thing to accomplish!), whatever is tackled – i.e., selected for study *because* it is regarded as falling within this definition – is bound to reaffirm it. Unless one is willing to transcend the arbitrarily set boundaries, that is – which is what research practice often seems to involve, even if the studies are performed within an essentialist and hence classificatory frame of reference.

Thus, the unpleasant truth is that even those who have sworn by the need to proceed deductively, on the basis of a well-formulated definition, never had any scruples about picking for study texts, or parts thereof, or other phenomena occurring in them, on the simple grounds that they had been presented, or otherwise regarded as translational. Always within a particular culture, to be sure, and in its own terms. Thus, in fact, they too were adopting the pre-systematic attitude of the persons-in-the-culture, but as little more than a necessary evil: there was hardly ever any attempt (or even willingness) to follow it in a meaningful way or to adhere to the research procedures it may have suggested.

What scholars tended to do instead was tamper with the 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 optimum, or else by introducing

additional (a priori, and hence non-cultural and ahistorical) distinctions, most notably between 'translation' and 'adaptation'. Of course, the number of such distinctions could be multiplied almost ad infinitum, and in any case they do not offer much explanatory power when it comes to culturally contextualized phenomena.<sup>6</sup> Such scholars do not seem to have considered assigning to their forced target-orientedness a *systematic* status, which would have entailed an *inductive* attempt to derive general principles from the facts themselves rather than make do with sheer speculation within a rigid frame of reference.<sup>7</sup>

It is important to note that the principles we have formulated were never intended to stealthily impose an alternative definition of the "Gegenstand der Übersetzungswissenschaft", as wrongly posited by some critics, most notably Werner Koller (1990, 1992). Rather, what those principles constituted right from the start was just **a working hypothesis providing guidelines for the establishment of corpora for studies of one basic kind**, sharing one set of goals. Within our frame of reference, this hypothesis is applied to what I have gradually come to call 'assumed translations': that is, all utterances in a [target] culture which are presented or regarded as translations, on any grounds whatever, as well as all phenomena within them and the processes that gave rise to them.

Under such observation, there is no pretence that the nature of translation will ever be fixed, let alone that it is a given. What is addressed, even in the longest run, is not really what translation *is* – by nature, so to speak – but what it is in a particular place and/or point in time (description), and hence what it may be *expected to be* under one or another set of specifiable conditions (prediction).

There may be any number of reasons for regarding a TL entity as a translation within a particular culture. On the other hand, there is also the possibility of encountering texts and phenomena that could plausibly be regarded as translational too, but which were not, whether they were regarded as something else or whether the basic distinction between translations and non-translations was simply

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6. In almost any other paradigm, the Hebrew translation of a German *Schlaraffenland* text, which will be submitted to detailed discussion in Chapter 10, would probably have passed as 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 kind of treatment sheds precisely on the concept of translation pertinent to the period it was produced in and for, and on the audience's expectations.

7. I believe Menachem Dagut was the first to have put his finger on the basic differences between various scholarly approaches to translation in terms of whether they require a deductive or inductive mode of reasoning. 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 of inductive reasoning in the descriptive part of his work on translation from Hebrew into English (Dagut 1978).

non-functional, and hence a non-fact in the culture in question. Of course, items of the latter kind can also be studied within Translation Studies, but an account will have to be given precisely of the fact that they were *not* presented/regarded as translational; otherwise the required goal will never be attained. The point here is precisely to tackle questions such as why a text, or an activity, 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 this 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 brings two important benefits:

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

## 5. The contents of the notion of ‘assumed translation’

Proceeding from culture-internal observations often involves problems of local, pre-systematic nomenclature, and it stands to reason that many of the distinctions recognized as functional within a particular culture would also find expression in language, with labelling as an 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) and others, so that there is hardly room to question their applicability to German *Übersetzung*, Amharic *tirgum*, or any other ‘ethnic’ label. It is the *content* of 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*, their existence is posited rather than factual, at least not of necessity. Therefore, rather than constituting answers, they are designed to give rise to *questions*, to be addressed by anyone wishing to study translation in context.

Let us have a brief look into the three postulates, in an attempt to clarify their posited status and the way they reflect the 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 presumably *preceded* the one taken to be its translation, but it is also assumed to have served as a *point of departure* and a *basis* for the latter.

Crucially, it is not the source text as such, nor even the possibility of actually pointing to it, that is at stake here, but only the *assumption* that one must have existed. Therefore concrete texts in languages other than TL are not part of the necessary equipment for launching research either, for even if no SL text is used, the study will still pertain to Translation Studies: namely, as long as the assumptions of the temporal preexistence and logical priority of such texts are taken into account.

To be sure, a TL fact that was tentatively marked as a translation, with the Source-Text Postulate implied, may later turn out to have had no corresponding text in another language/culture, and not just as a result of a mere failure to locate it. A concrete source text may never have existed, to begin with. Thus, within the target-oriented paradigm of DTS, pseudotranslations emerge as fully legitimate objects of study: until the mystification has been dispelled, their functioning within a culture is no different from that associated with genuine translations. As will be claimed in Excursus A, pseudotranslations may even *mimic* genuine translations. On the other hand, an assumed translation may later be found to have had more than one source, being a case of compilational 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.<sup>8</sup> (See Chapter 5, 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 **transfer from the assumed source text of certain features that the two now share**. This assumption is clearly the result of bringing two different kinds of knowledge to bear on each other: knowledge about products, on the one hand, and about (cross-linguistic and cross-cultural) processes, on the other.

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8. 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.

When regarded from a target point of view, transfer operations – both their very existence and their exact nature (and that of the transferred/shared features) – first manifest themselves as axiomatic too. Of course, both aspects can be put to the test, but they would remain distinct in their very essence: even if recourse to transfer operations will have been confirmed, these will not necessarily be found to match the one posited. However, either one can be put to a meaningful test only inasmuch as the appropriateness of the source text has been secured. (See Section 6 below.)

### 3. *The Relationship Postulate*

Finally, adopting the assumption that a particular TL text may be a translation also implies that there are tangible relationships that tie it to its assumed original, an obvious function of whatever the two texts allegedly share (Postulate 2) and of what 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 concerning the level(s) at which translation relationships are established (in the act) and sought (by the researcher). 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 need not be reflected in reality either: upon examination, relations actually tying together pairs of texts (or parts thereof, or entities within them) may well be found to *differ* from the ones postulated. Since there is no inherent need for intertextual relationships to always be of the same kind and 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 rather than speculation, let alone mere wishful thinking.

Another indication of the posited nature of translation relationships as thus conceptualized is the fact that they can often be at least tentatively identified (reconstructed) in the absence of an SL text: namely, on the basis of certain features of the assumed translation itself, along with the concomitant assumption that it is indeed a translation.

Fictitious translations often manage to pass for genuine without arousing much suspicion by making manipulative use of this precise fact. On the other hand, this often serves as a basis for the identification of the text that served as a source of a particular assumed translation, e.g., when it is suspected of being compilative or indirect.

If we take the three postulates together, this is what an assumed translation would amount to:

any target-culture text for which there are reasons to tentatively posit the existence of another text, in another culture/language, from which it was presumably derived by transfer operations and to which it is now tied by a set of relationships based on shared features, some of which may be regarded – within the culture in question – as necessary and/or sufficient.

## 6. Discovery vs. justification procedures

It should have become clear by now that neither source text nor transfer operations and transferred features, nor even target–source relations, would be excluded from DTS. They will just have to be assigned a different status. In other words, being target-oriented is far from being exclusively a part of the target culture, as wrongly assumed by many. The approach we have adopted has been defined as target-oriented because *this is where observations begin*, but by no means should it be taken to imply that this is where they will also end.<sup>9</sup>

From another angle, it is only reasonable to posit that a study into translation activities which have reached their processual end and yielded their products would start with the *observables*: first and foremost, the translated utterances themselves, along with their identifiable constituents. From there on, the study may proceed to facts that are observable in the second order (that is, facts which first need to be constructed before they can be submitted to scrutiny), most notably relationships that link the output and input of an individual act of translation. However, the ultimate goal is to reconstruct the *non-observables* at their root, particularly the processes whereby they came into being, the strategies that were adopted towards that end and the reasons for adopting those strategies and rejecting others.

TL texts assumed to be translations thus constitute the most obvious first research candidates for study, and should, moreover, be studied *under that very assumption*. Such texts, or aspects thereof, would first be studied on their own terms; namely, in terms of their **acceptability** on all relevant levels, not only as TL texts, but also as translations into the target culture.

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9. In fact, I cannot see why such an approach would concern itself with transfer any less than, say, the position adopted by the group that used to work in Göttingen, Germany (SFB 309 “die literarische Übersetzung”; e.g., Frank 1990: Section II), largely as an (over)reaction to my programme. It is my firm conviction that unless all target constraints are taken into consideration, which can only be done within a target-oriented approach, transfer remains at best only partly explainable, even if it can be addressed in other frameworks too.



Once a particular text in a language other than TL has tentatively been marked as the corresponding source of an assumed translation, the next step is to **map the assumed translation onto its assumed counterpart**, in an attempt to determine the (uni-directional, irreversible) relations that obtain between the pairs of texts and hold them together. At the same time, such mappings constitute the safest means of conclusively settling the appropriateness of SL texts as assumed source texts, a very tricky process, to be sure.

Owing to many inherent limitations, some of them no doubt cognitive in nature, it will normally be *segments* of the assumed target text (rather than the text as a complete entity) that would be mapped onto parallel segments of the assumed 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 translation problems as manifested in the particular act that yielded the TL text under observation (PROBLEM<sub>2</sub>; see Chapter 2), and on their solutions. Shifts (from a given notion of ‘maximal’ or ‘optimal’ rendering) can also be identified and studied, if deemed justified, interesting and/or feasible in the framework of the research undertaken,

Having been established for a series of paired segments, and grouped together on the basis of the comparisons themselves, translation relations could then be referred to the concept of translation that may be said to underlie the text as a whole. This will be done through the mediation of a revised notion of *equivalence*, conceived of as

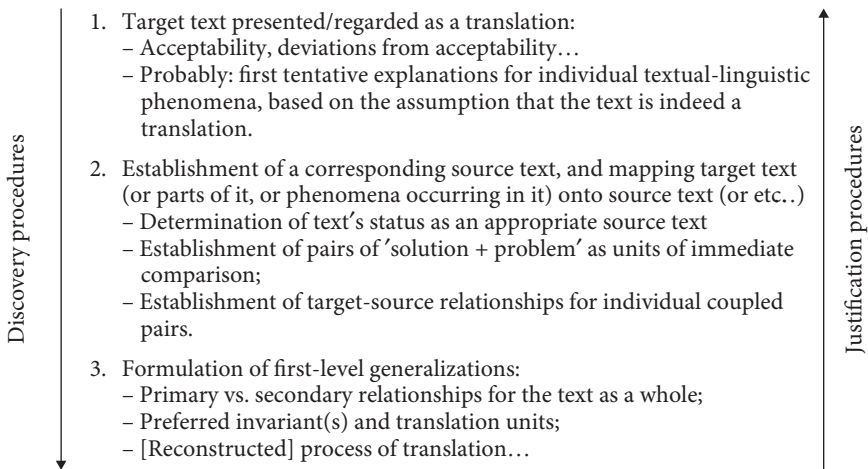
**that translation relationship which would have emerged as constituting the *norm* for the pair of texts under study.**

Actually, it is these last two concepts that form the ultimate goal of studies into individual pairs of texts assumed to be translation and source, respectively. Nothing on the way to the establishment of the norm of equivalence, or of the underlying concept of translation, can be fully accounted for without reference to them. On the other hand, these concepts cannot be established in any controlled way before the whole gamut of discovery procedures is thoroughly investigated. Even though intuitions as to their nature, however plausible they may be, may present themselves at an earlier stage, these intuitions would have to be *justified*, if they are to be given the status of explanations; and systematic justifications necessitate systematic management of the study.

Once the prevailing concept of translation, established for one pair of texts, is introduced into a broader context, it may also become possible to start speculating on the *considerations* that may have been involved in the decisions whose results were first to be identified, along with factors that may have constrained the act. This speculating may involve a confrontation of competing models and

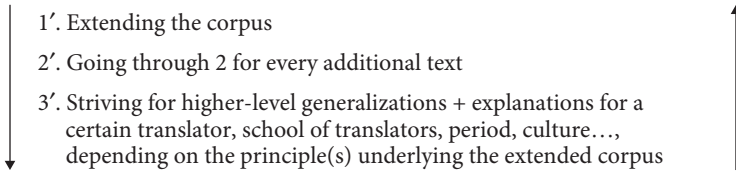
norms underlying the target and source texts and systems, which were responsible for the establishment of the individual replacing and replaced segments, along with the relationships shown to obtain between them.

Justification procedures thus emerge as a mirror image of the corresponding discovery procedures, as shown in Figure 6. At the same time, it would be wrong to assume that, in the course of a concrete study, justifications are first offered when the discovery procedures have been exhausted. Rather, in every phase of the study, from the very start, there is room for suggesting tentative explanatory hypotheses, which will then reflect back and affect subsequent questions and discoveries. The normal progression of a study is thus *helical* rather than linear: there will always remain something to go back to and discover, with the concomitant need for more (or more comprehensive, or more elaborate) explanations.



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

Needless to say, insofar as the intention is to expose the culturally determined interdependencies of function, process and product, one pair of assumed translation and source text would not normally constitute a sufficient corpus, not even when the focus is indeed on that pair. Any aspiration to supply convincing explanations would rather entail an expansion of the data according to some guiding principle: translator, school of translators, systemic position (near the centre or on the periphery), period, text-type, textlinguistic phenomenon of interest, or any other principle that may prove relevant (see Figure 7). Explanations formulated in previous studies, pertaining to other texts, groups of texts or types of phenomena, can also be brought to bear on the corpus in question, which would involve further expansion, albeit indirect.



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

## CHAPTER 2

# The notion of ‘problem’ in Translation Studies

Translation Studies typically fails to reflect on the concepts it uses in its own internal terms. Not only have many of these concepts been imported from other fields of knowledge (which is quite understandable), but they have undergone very little adjustment to the specificities of their new setting (which is less understandable and much less forgivable). In this chapter, one such notion will be isolated for a closer look: the ‘translation problem’. This notion will be submitted to scrutiny, preparing for a more enlightened and responsible use in the discipline. It is to be hoped that other notions will be submitted to a similar treatment in the future.

The notion of ‘problem’ has loomed large in discourse about translation, scholarly and non-scholarly alike, regardless of whether the discourse has had theoretical (e.g., Holmes’ category of ‘Problem-Restricted [Partial] Theoretical Translation Studies’), descriptive-explanatory or applied aspirations. Moreover, the prevalence of the notion has not depended on whether the word ‘problem’ itself was used or not, and whether the focus was on mere “questions to be solved or decided” or, more narrowly, on “issues which pose special difficulties”. The transition from one sense to the other is not difficult to make, which would explain the frequent sliding from more or less neutral accounts of problems encountered in translation to attempts to show how *problematic* a specific issue, or even the whole business of translation, is. Interestingly enough, the logical complement of ‘problem’, ‘solution’, is much rarer. This peculiarity will also be addressed.

### 1. ‘Problem’ and its terminological status

It is easy to show that the word ‘problem’ was often adopted by translation scholars with only partial awareness of the ensuing implications and complications. In fact, the use of this word has become a matter of course to such an extent that

most authors haven't bothered to have it included in their indexes. Itemizing the notion of 'problem' must have seemed pointless to them.<sup>1</sup>

In what follows, one major source of difficulties encountered in dealing with the notion in the context of translation will be tackled. My intention is not to analyse any particular problem in the field, but rather to shed some light on a "grey zone" that seems to have formed between the word 'problem' and its use as a term in Translation Studies, following an (as yet unfinished) process of terminologization which the field has been undergoing.<sup>2</sup> As we will see, the greyishness of the zone frequently involves an unknowing transgression of boundaries between a number of different types of expert discourse on translation.

The mere recurrence of a word in a text, let alone across a range of texts, does not guarantee sameness of designated concept. This is especially the case when the word has been taken over from another field, or from a pre-systematic kind of discourse, both of them relevant here. Rather, concepts are always embedded in **conceptual networks**, so that each one of them can only be rendered intelligible, and hence be accounted for, within that network and in its own internal terms.<sup>3</sup> Under no circumstances should different conceptual systems be allowed to interfere with each other even if the same words are employed in some or all of the texts therein. Indeed, it is precisely in the latter cases that the risks accompanying indiscriminatio are the greatest.

Unfortunately, this received logic has not been adhered to in most uses of 'problem' in expert discourse about translation, not even among terminologists, generally believed to know better. The word is certainly present, and rather abundantly, but its terminological status is far from clear. Nor have translation scholars undertaken such clarification, except in some marginal, typically brief remarks

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1. The same holds for some more recent publications of a general nature such as the *Routledge Encyclopedia of Translation Studies* (Baker 1998) or the first volumes of *Translation Studies Abstracts*, where this practice goes on. Thus, while the issue itself may be treated, here and there, there is neither a special essay entitled "Translation Problems" nor an index entry to that effect. Shuttleworth (1997), which has no index, doesn't have an entry for "[Translation] Problem" either, even though he does have one for Holmes' "Problem-Restricted Theories of Translation".

2. See especially the special theme issue of *Target* on "The Metalanguage of Translation" edited by Yves Gambier and Luc van Doorslaer (2007).

3. See the method employed by the editors of the recent four-language *Translation Terminology*, Jean Delisle, Hannelore Lee-Jahnke and Monique C. Cormier (1999), as it is described in the Introduction to the book (French: pp. 2–3; English: pp. 108–109; Spanish: pp. 214–215; German: pp. 324–325).

(e.g. Lörcher 1991). Moreover, even though it is the notion of 'problem' which has been singled out for the present discussion, this lack of refinement is not confined to it alone. It applies to various other related notions, including the overarching concept of 'translation' itself, as both process and product, along with the kind of 'invariant' retained during the transition from source- to end-product and/or the 'relationships' that tie the latter (or parts of it) to the former.<sup>4</sup>

As I see it, the word 'problem' has come to serve as a term-in-the-making in at least three (interconnected, but essentially different) contexts of discourse, all of them situated in Translation Studies and involving expert-to-expert communication.<sup>5</sup> In each one of these contexts the underlying concept is engaged in a different set of relations with an array of other concepts, thus acquiring different traits, or "existence conditions", which account for the differences of meaning between what amounts to three different terms.<sup>6</sup> In fact, it is not inconceivable that in languages other than English, these different meanings may be assigned **different** words.

Let us go some way now towards the delineation of the three types of discourse and some of their implications for the corresponding notions of 'problem': PROBLEM<sub>1</sub>, PROBLEM<sub>2</sub> and PROBLEM<sub>3</sub>, as they will henceforth be referred to.<sup>7</sup> By contrast, I will make no attempt to distinguish between different levels of problem (e.g., problems of reception vs. problems of production) in any of these discourses, or different phases of the translation process where a problem may manifest itself, or any other aspect of translation problems. The focus will be on the **contexts of discourse as such, the positions of the respective terms within them, the "existence conditions" of the underlying concepts and the ensuing meaning of each notion of 'problem'.**

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4. In this connection, see Table 1 in Part One above, summing up the differences between Translation Theory, Descriptive Translation Studies, and the applied extensions of Translation Studies with respect to different notions of "translation relationships".

5. A fourth context, that of translator training, where communication is typically asymmetric, between experts and novices, seems to have always been eclectic, showing a kind of eclecticism which appears to be inherent in it.

6. Thus, my (rather intuitive) notion of "context of discourse" seems to include Pearson's (1998) "communicative settings" and Thelen's (2002) "levels of communication" without, however, being fully reducible to either.

7. Whenever a word is offered as a term, SMALL CAPS will be used. The context of discourse to which the term pertains (1, 2, 3) will be indicated by subscript numbers.

## 2. The three terminological uses of ‘problem’

### PROBLEM<sub>1</sub>

PROBLEM<sub>1</sub> has its place in discourse about source texts (or parts/aspects thereof, or phenomena occurring in them) and the way they constrain their envisaged translation; this sense may concern translation in general, or, more often, translation into a particular culture, language and textual tradition where the establishment of a translational SOLUTION<sub>1</sub> is set as the goal. PROBLEM<sub>1</sub> is thus a matter of **potentials**, not actual facts; in other words, of **translatability** rather than translation.

In this first context of discourse, not just any translation replacement would assume the status of a SOLUTION<sub>1</sub>: only those replacements that can make a claim for **appropriateness**. To be sure, the appropriateness of ‘translation’, and hence of ‘translatability’, are perceived within the target culture in question.

Interestingly enough, translatability, which is so central to the meaning of PROBLEM<sub>1</sub>, seems to pertain exclusively to the first context of discourse: unlike ‘problem’, ‘solution’ and ‘translation act’, and many other polysemous words which may serve as terms in a number of types of discourse with different terminological meanings, there is only TRANSLATABILITY<sub>1</sub>; namely,

**the initial potential of establishing optimal correspondence between a TL text (or textual-linguistic phenomenon) and a corresponding SL text (or phenomenon).**

This correspondence can vary greatly. In fact, it may be anywhere between 0 and 1, non-existent and absolute, without ever coinciding with either of the two extremes.

While the need to search for an *appropriate* solution is a major issue here, SOLUTION<sub>1</sub> itself as the realization of this requirement is all but present. As PROBLEM<sub>1</sub> is not really something which is either there or not there, but rather a projection of some kind of an analysis designed to facilitate ordered thinking about translational problem-solving in general, SOLUTION<sub>1</sub> **can have no physical reality**. In other words, PROBLEM<sub>1</sub> is never really solved. This is one thing I had in mind when I mentioned the symptomatic absence of the notion of ‘solution’ from many discussions about translation.

In cases where a concrete replacement is ventured nonetheless, this replacement would not really be submitted to comply with PROBLEM<sub>1</sub>, the one involving issues of translatability. Rather, the replacement – along with the discussion as a whole – will have shifted to a different mode, one dealing with actual translation(s), where both ‘problem’ and ‘solution’ are concrete entities.

Nor is the notion of ‘translation act’ part of the first package either: not only will no such act have taken place, but, so long as it is referred to within the first

type of discourse, a **translation act needn’t be undertaken at all** – in principle, as it were. Thus, it is quite normal to discuss the SOLVABILITY of PROBLEM<sub>1</sub> (which may be higher or lower), even alternative ways of going about solving it, without actually *performing* the act, most certainly without reaching any binding SOLUTION<sub>1</sub>, not to mention coming up with a single replacement couched in TL, without which no act of translation will have been exhausted.

If there is any room for the notion of ‘translation act’ in the first context of discourse, it is therefore little more than a *generic label*, imported from other types of discourse to quasi-realistically denote the theoretical inevitability of the presence of some entity (and activity) mediating between PROBLEM<sub>1</sub> and any SOLUTION<sub>1</sub>; and it is clear that this activity is basically one of **problem-solving**. What is implied is simply that no transition from ‘problem’ to ‘solution’ is conceivable unless the existence of such an act were hypothesized, whatever its nature or the additional constraints that may be imposed on instances of actual problem-solving. (To be sure, when it is the realization of the translation act which is at stake – namely, in one of the *other* contexts of discourse – it may indeed assume a multitude of different forms. Moreover, none of these would be a straightforward realization of TRANSLATABILITY<sub>1</sub>, the initial potential dealt with in the first type of discourse. Theoretically speaking, at least, there will always be ways to achieve greater proximity to the source text.) What all this amounts to is that the nature of TRANSLATION ACT<sub>1</sub> cannot be studied in any direct way. At best, it can be *speculated on*, more often than not in ideal (or, better, idealized) terms, which go so very well together with the speculative notion of TRANSLATABILITY<sub>1</sub>.

In fact, in this context of discussion, the embodiment of the entity whose role it is to mediate between PROBLEM<sub>1</sub> and SOLUTION<sub>1</sub>, i.e., TRANSLATOR<sub>1</sub>, is just another theoretical construct: a **persona** rather than a **person**. On the one hand, this entity is devoid of any concretizing features such as gender, age or previous experience, while, on the other hand, it is often assigned almost mythical capabilities: full mastery of the languages and cultures involved in the act, unlimited memory, an ideal capacity to analyse, interpret and compose texts, and many more.

Let me recapitulate, using an alternative approach which many may find more pleasing: whereas the (virtual) TRANSLATION ACT<sub>1</sub> can indeed be likened, as Jiří Levý did, to a “game with complete information” (that is, “a game in which every succeeding move is influenced by the knowledge of previous decisions and by the situation which resulted from them”); Levý 1967: 1172),<sup>8</sup> this would not be the case

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8. As for Levý’s famous (and somewhat obscure) notion of ‘minimax strategy’, that is, opting “for that one of the possible solutions which promises a maximum of effect with a minimum of effort” (1967: 1179): in reality this is only one of many strategies a translator may resort to, and not necessarily the unmarked one either. Putting it mildly, we can probably assume that



with either TRANSLATION ACT<sub>2</sub> or TRANSLATION ACT<sub>3</sub>, where the game of translation tends to be played “with *incomplete* information”. Not even all the information that can, in principle, be obtained is necessarily taken into account.

An important corollary of what I have been saying about the first context of expert discourse on translation is that **there is no way TRANSLATION ACT<sub>1</sub> can be simulated**, simulation being the representation of a certain act through the use of an act of a *different* kind, in a more or less *controlled* environment, with a *reduced risk* of having to bear some consequences.<sup>9</sup> Once again, TRANSLATION ACT<sub>1</sub> is no act at all, only a theoretical concept. What is more, in any alleged simulation of translation, the simulated activity is represented by an act *of the very same kind*, and, if anything, the stakes involved are *higher* than in any real-life translation, if only due to the greater amount of time and effort involved in its execution, driven by the ostensible demands of the intended audience, which are usually other experts and would-be experts. No wonder, then, that the end-product of ‘simulated’ translation may be, and often is presented (and readily accepted) as a bona fide translation!

Simulating translation is thus tantamount to actually *performing the act*, albeit under specified (and extreme) conditions. But then, conditioning holds equally true for any translation situation: no act may be performed out of context and every context imposes constraints. This means that any pretension to realize a translation act in the first type of discourse is an illusion,<sup>10</sup> whereby the

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only rarely do translators adopt an ‘optimal’ mode of work. Thus, while we may be justified in assigning the ‘minimax strategy’ a special status in the *first* context of discourse, which features *idealized* notions, this has no necessary implications for either of the two other contexts of expert-to-expert communication: like the act itself, the issue of ‘translation strategies’ will have to be addressed in each of these anew and tackled on the basis of the relevant evidence available.

9. Classical examples of simulation would be the medieval tournament (a public contest between armed men intended to simulate real battle) and most games. More modern instances would be wind tunnels and an assortment of computer-generated environments associated with so-called “virtual reality”.

10. Consider the extreme case of translation attempted by a trained linguist, normally with respect to a rather small-scale and low-level textual-linguistic segment, for the purpose of producing an illustrative example for a general point s/he wishes to make vis-à-vis the *optimal* translation of one particular phenomenon; i.e., allegedly within the first context of discourse. (A perfect example would be Doherty 1997: 10–14.) This is certainly common practice among theoreticians of translation, who often fail to realize that what they are doing is not really to fully (or even optimally) “realize the translatability potential” of whatever they may be working on, but rather they are doing actual “translation”, just like any other practitioner, only their praxis is contextualized differently and therefore tends to yield different “solutions”. To different “problems”, I hasten to add.

notion of 'problem' itself undergoes a shift of category: from the initial, abstract and ideal(ized)  $PROBLEM_1$  to either  $PROBLEM_2$  or  $PROBLEM_3$ , which will soon be differentiated.

To sum up,  $PROBLEM_1$  is **prospective** (i.e., it refers to translation which would at most be performed in the future) and **utopian**. It involves a phase of **recognition** before any measure can be taken. In fact, its recognition is a precondition for the very possibility of taking any such measures. This is to say, no  $PROBLEM SOLVING_1$  can be contemplated unless  $PROBLEM_1$ , the one regarded as requiring a  $SOLUTION$ , is established **correctly**; both in full (which is complicated enough) as well as in the appropriate hierarchical order assigned to its various components and the functions they fulfil. (These, needless to say, multiply the complications.) The only issue associated with  $PROBLEM_1$  is actually one of options; namely, **the initial range of cross-linguistic, cross-cultural replacement**. The options themselves may be, and at times indeed are, **logically ordered**. They may even be presented as flow charts or derivation trees reflecting reasoning in terms of series of binary oppositions. What does *not* follow is that any such presentation will necessarily have a validity for real  $PROBLEM SOLVING_{2-or-3}$  in any real act of translation – whether psychological or any other kind of validity.<sup>11</sup>

Inasmuch as the first kind of discourse has dominated expert discourse on translation throughout most of the modern era,  $PROBLEM_1$  is by far the most common variety found in the literature. And in view of the highly utopian nature of this discourse and the concepts it uses, it is not surprising that many of those who have propounded the notion of  $PROBLEM_1$  have come to shun its very solvability. In principle, that is, rather than in any particular case. For them, so long as they stay within the boundaries of that kind of discourse,  $SOLUTION_1$  can, at best, be likened to the horizon: an imaginary line which keeps receding as one seeks to approach it; and at exactly the same pace.

The fact that many scholars purport to offer concrete 'solutions' nonetheless is no refutation of the point I have been trying to make. What they are offering is really solutions to  $PROBLEMS_2$  or  $PROBLEMS_3$ ;  $SOLUTIONS_{2-or-3}$ , that is, and not the non-existing  $SOLUTION_1$ .

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11. Note how Levý (1967) activates his notions of 'definitional' vs. 'selective' instructions, or see my own schematic presentation of a 'living metaphor' as a  $TRANSLATION PROBLEM_1$  (Part Four), or Leppihalme's flow charts representing the main available "translation strategies" as a hierarchical decision process of the 'minimax' type in the case of two different types of allusions (1997: 106–107). – Whereas Levý seems to think that his schematic presentation of the theoretical possibilities indeed represents a *mental* process, both Toury (ibid.) and Leppihalme (especially 1994: 180) have their reservations.

PROBLEM<sub>2</sub>

Unlike PROBLEM<sub>1</sub>, PROBLEM<sub>2</sub> features in discourses which are **retrospective** (i.e., they refer to acts which have already been performed) and where the basic issue is one of **factual** replacement in concrete acts of translation. Consequently, PROBLEM<sub>2</sub> is not a given, neither in the source text as such nor even in its confrontation with the initial capabilities of a particular receiving language/culture to solve it. The only way to get hold of it is to analyse actual translation events.

This is to say: PROBLEM<sub>2</sub> bears no necessary relation to PROBLEM<sub>1</sub>, which – had it existed – would have rendered PROBLEM<sub>2</sub> a kind of ‘second-order given’. Rather, it manifests itself individually in each TRANSLATION ACT<sub>2</sub> performed by a TRANSLATOR<sub>2</sub>; not merely in *temporal* terms (i.e., during the performance of the act), but in the very purposes of the act (i.e., in *causal* terms). However, ACT<sub>2</sub> vanishes very quickly, along with the PROBLEMS<sub>2</sub> that TRANSLATOR<sub>2</sub> was actually seeking to solve, leaving a single lasting imprint – the SOLUTIONS<sub>2</sub> comprising the translated text.

Unlike PROBLEMS<sub>1</sub>, PROBLEMS<sub>2</sub> can be identified only by looking at concrete texts assumed to be translations, for whatever reason, and mapping them onto other texts, in another language/culture, which are assumed to have served as their respective sources. The mapping itself is performed on the assumption that an accountable TRANSLATION ACT<sub>2</sub> was indeed involved in the transition from the source to the target text in each pair. Unlike the generic TRANSLATION ACT<sub>1</sub>, which is totally abstract, any ACT<sub>2</sub>, despite its disappearance, is still capable of being **reconstructed**, at least tentatively and in part. This provides ample background material for carrying out the analysis, and especially for the description and tentative explanation of its findings.<sup>12</sup>

As reconstructed entities, PROBLEMS<sub>2</sub> can only be established backwards, so to speak: that is, from the replaced members of coupled pairs of replacing + replaced segments established ad hoc during the comparative analysis of the two texts in question and for its sake, where the replacing members of each pair are simultaneously taken to represent the corresponding SOLUTIONS<sub>2</sub>. The realizations of the two complementary notions as well as the relationships obtaining between them are therefore relatively easy to observe. This suggests the possibility of accounting for them and inquiring into their motivations, despite the many difficulties – both methodical and practical – which will no doubt be involved in any attempt to realize the project. What remains a true stumbling-block is

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12. Sometimes hypotheses can be at least tentatively formulated on the basis of an assumed translation alone, with no factual source text to match, but I would refrain from following this side-track.

the concealed TRANSLATION ACT<sub>2</sub>. Even though, unlike ACT<sub>1</sub>, it has once been put into effect and thus can be claimed to have had a real existence, the task of accessing it in retrospect is a hard one, and hence it is still a complex notion. Consequently, much as one may wish to shy away from it, *speculation* still forms part of the way PROBLEM<sub>2</sub> and SOLUTION<sub>2</sub> are established, presumably connected by a concrete ACT<sub>2</sub> that has completely evaporated.

Thus, even though we certainly know better, from both introspection and studies carried out with various methods, most retrospective analyses of translation have been, and will probably go on being performed on the simplistic (and not always fully acknowledged) assumption that ACT<sub>2</sub>, the one that is reconstructed as having yielded the assumed translation in question, is **linear, unidirectional** and **non-interrupted**. It is on this basis that the text is regarded as a series of more or less discrete segments, with respect to which translation replacement can be said to have been made in one fell swoop. An account like this is sometimes closer to reality (e.g. in cases where [assumed] translations have been produced which look very 'literal'), while at other times (e.g. in seemingly 'freer' [reconstructed] acts) it may be quite remote from the source text. What such an account should *not* do is serve as a starting point and framework for research in general, as translation acts seem to also involve **self-monitoring** (i.e., interruptions of the basic unidirectionality with occasional changes of direction, e.g., in the form of loops. Unfortunately, however, these aspects of the process can be heeded only when 'positive' clues have been found to lead to them, which is simply not the case with the concealed ACT<sub>2</sub> that is tentatively established on the basis of coupled pairs of SOLUTIONS<sub>2</sub> and PROBLEMS<sub>2</sub>. This claim would be valid even if the pairs consist of the two integral texts, which is a totally unviable requirement to begin with.

The conclusion seems clear enough: the possibility of finding clues to actual processes of translational decision-making, and introducing them into the discussion, gives immediate rise to a *third* kind of discourse which has notions of 'problem' and 'solution' of its own; namely, PROBLEM<sub>3</sub> and SOLUTION<sub>3</sub>, respectively.

### PROBLEM<sub>3</sub>

PROBLEM<sub>3</sub> is no less factual than PROBLEM<sub>2</sub>. Like the latter, it is also associated with the performance of a single TRANSLATION ACT<sub>3</sub> which is situated in a particular point in time and space. However, its factuality is less straightforward, and its establishment cannot possibly be regarded as purely retrospective. Here – even in cases when the act (ACT<sub>3</sub>) would have come to an end – observation will not venture backwards from the point of its termination, towards that point which marks its commencement, as is the case with the reconstruction referred to in the second context of discourse. The only way PROBLEMS<sub>3</sub> can and will manifest

themselves is step by step, alongside the gradual unfolding of ACT<sub>3</sub> itself. Rather than being punctual, they may therefore be regarded as **processual**.

This kind of observation can be attempted only inasmuch as ACT<sub>3</sub> has left more traces than just the end-product, as was the case with ACT<sub>2</sub>; most notably, temporary, **interim replacements**, on the one hand, and **reflections** on the act on the other: on both PROBLEMS<sub>3</sub> and their SOLUTIONS<sub>3</sub> (final or interim) – as well as on ACT<sub>3</sub> itself. This will add to the available data factors which could not have been taken into account before, such as information about differences in the amount of time, effort and consciousness invested in different parts (or phases) of ACT<sub>3</sub>, and the processual PROBLEM<sub>3</sub> will be reconstructed in direct proportion to the presence of such clues.

Unlike the first two notions of ‘problem’, PROBLEM<sub>3</sub> is thus a **dynamic** notion, which may, moreover, assume various forms. It may even change its primary disposition in the course of ACT<sub>3</sub> (or rather – from the researcher’s point of view – in the course of its unfolding/reconstruction). This changeability is inherent to TRANSLATION ACT<sub>3</sub>: The ultimate SOLUTION<sub>3</sub>, which – in retrospect, and in the absence of any reconstructable ACT<sub>3</sub> – may well have been posited as identical to SOLUTION<sub>2</sub>, is not necessarily the only SOLUTION<sub>3</sub> entertained, or even realized in the course of ACT<sub>3</sub>. Rather, any number of INTERIM SOLUTIONS<sub>3</sub> may be, and often are explored along the way. The multiplicity of SOLUTIONS<sub>3</sub> can be unearthed in several ways, in retrospect (for instance, by studying manuscripts which have undergone revision [see examples in Chapters 11–12]) as well as in real time, as it were (for instance, by making use of Think-Aloud Protocols [e.g. Bernardini 2001] or special computer programs such as Translog [Jakobsen and Schou 1999], which record every single key-stroke made by the translator). Be this as it may, rather like the way TRANSLATABILITY and SOLVABILITY feature solely in the *first* context of discourse on translation, the notion of INTERIM SOLUTION has its place in the *third* context only.

Once a concretized translation act has become a real factor (TRANSLATION ACT<sub>2-or-3</sub>, that is), and to the extent that it is still regarded as an act of problem-solving, the notion of SOLUTION<sub>2-or-3</sub> becomes highly **technical**. It is anything that is there whenever the act is discontinued, whether it has reached its end (= SOLUTION<sub>2</sub> or FINAL SOLUTION<sub>3</sub>), or is just temporarily suspended (= INTERIM SOLUTION<sub>3</sub>). After all, it is the fact that something *was* seized upon, be it ever so briefly, which lends a translational replacement the tentative status of SOLUTION<sub>2/3</sub>, and not any alleged ‘fullness’ or ‘quality’. This technical sense of SOLUTION<sub>2/3</sub> is thus devoid of value judgments. To be sure, even if TRANSLATOR<sub>2/3</sub> (i.e., the real mediating person of ACT<sub>3</sub> rather than the abstract persona of ACT<sub>1</sub>) is not keen on a particular SOLUTION<sub>2/3</sub> (an attitude which may be traced, e.g., through changes made in and/or comments about it, whether during ACT<sub>3</sub> itself or post factum), it

is still a SOLUTION<sub>2/3</sub> in this technical sense, as the act would have been discontinued, at least for a while.

But there is more: while some INTERIM SOLUTIONS<sub>3</sub> may represent alternative ways of solving one and the same PROBLEM<sub>3</sub>, others may involve a *change* of the PROBLEM<sub>3</sub> actually being addressed by TRANSLATOR<sub>3</sub>, or even of the textual-linguistic segment where the “problem” is taken to reside. This lends a **variational** character not only to SOLUTION<sub>3</sub>, but to PROBLEM<sub>3</sub> as well, in striking contrast to both the initial (idealized) PROBLEM<sub>1</sub> and the single (reconstructed) PROBLEM<sub>2</sub>. It is only when, while a particular ACT<sub>3</sub> is being exposed, a straightforward, more or less automatized replacement seems to have occurred (that is, one where neither INTERIM SOLUTIONS nor any conscious reflections can be detected) that SOLUTION<sub>3</sub> would seem to coincide with SOLUTION<sub>2</sub>, and hence PROBLEM<sub>3</sub> with PROBLEM<sub>2</sub>. Such a phenomenon seems to be rare indeed.

Incidentally, even if automatized acts were conceivable, the ostensible appearance of automatic responses may still prove false. After all, researchers only have at their disposal what they can lay their hands on; in the case of written translation – only those SOLUTIONS<sub>3</sub>, INTERIM or FINAL, formulated in language, often only those actually put on paper, spoken aloud and recorded (or typed into the computer). If we could add to our data other kinds of information (e.g. on certain temporal aspects connected with ACT<sub>3</sub>, non-verbal clues such as hesitancy and lengthy pauses, etc.), we would probably find indications suggesting that many of what might seem automatic SOLUTIONS<sub>3</sub> were not in fact all that automatic either, and actually entailed a problem or two. While the latter may not have been very serious, they still constitute PROBLEMS<sub>3</sub>, and therefore they have to be exposed and accounted for as part of the reconstruction of ACT<sub>3</sub>. It is precisely here that research methods such as Think-Aloud Protocols may come in most handy.

**Table 2.** The main attributes of the three notions of ‘Translation Problem’

Notion		Attributes			Complementary notion
PROBLEM <sub>1</sub>	source-oriented	prospective, posited	initial possibilities of tr. replacement	utopian, abstract, potential	SOLVABILITY <sub>1</sub> , way to go about SOLVING
PROBLEM <sub>2</sub>	target-oriented	retrospective, punctual, reconstructed	factual tr. replacement	concrete, realized	SOLUTION <sub>2</sub>
PROBLEMS <sub>3</sub>	process-oriented	processual, reconstructed	factual tr. replacement, variational	concrete, realized, at least momentarily	SOLUTIONS <sub>3</sub> (final or interim)

Table 2 sums up in a simplified manner the main attributes of the three faces of the notion of ‘problem’ we have identified in expert-to-expert discourse on translation. This summary is not offered as a replacement of the detailed presentation throughout this chapter, but only as a mnemonic aid.

## Pseudotranslations and their significance

Being persons-in-the-culture themselves, producers of texts, translators included, tend to be aware of the positions translations and translators, as well as the activity of translation as such, are allotted in the culture and the functions they may fulfil. This awareness is often accompanied by, and finds its expression in certain behavioural patterns, including textual-linguistic features. On occasion, one may decide to manipulate this awareness, put it to active use and offer texts, often even actually compose them, as if they were translations. From the point of view of the culture that hosts them, the resulting texts, these pseudotranslations (or fictitious translations),<sup>1</sup> are really on a par with genuine translations. What is activated here is the Source-Text Postulate: i.e., the tacit agreement that for every assumed translation, a corresponding text in another language/culture must have existed (see Chapter 1). However, as such sources prove impossible to locate in this case, the hypothesis of their existence may eventually fade away.

For the researcher, there is an obvious catch here. After all, a text can only be tackled as fictitious when the veil is no longer part of reality: in other words, when the position it has in the culture is no longer the one it was initially designed to have and once had, whether the knowledge that the text in question used to be identified as a translation is still current or has completely been erased from collective memory. Only then can questions start being asked as to why the decision to use a disguise was made in the first place, why it was that particular language/textual tradition that was adopted as 'source', and finally, what it was that made the public fall for it for a longer or a shorter period of time. Why and how the veil was then lifted may also be of interest.

There is a meta-paradox here: if the answers to these questions and their like are to be historically significant, texts taken to be fictitious translations will have to be properly contextualized like any other text. In this case, proper contextualization involves having the texts tentatively reinstated in the positions they were designed to have. Of course, there may exist a myriad of cases where the mystification has not been dispelled, and maybe never will be. Such cases, whatever they

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1. A number of scholars (e.g., Sohar 1999) have suggested that a line be drawn between 'fictitious translations' and 'pseudotranslations'. I fail to see the point in a further division of this category.



are, genuine or fictitious, can only be tackled as translations whose sources have remained unknown. (See Chapter 5.) The postulate is still there, but it finds no realization. Thus, there is really no way, let alone a foolproof one, to distinguish between texts whose sources have simply vanished and texts which never had a single-text source.

Pseudotranslating has not always been so marginal a strategy as it may now look. True enough, in our times, with the world rapidly transforming into MacLuhan's (in)famous 'global village' (1964), and especially with the tightening of copyright laws, on the one hand, and the evolution of investigative journalism on the other, fictitious translations would usually no longer be located in the most canonized sectors of a culture, let alone in its epicentre, where the mystification would be next to impossible to maintain for long. This fact notwithstanding, the strategy is far from a mere curiosity, let alone a cheap means of cheating the public, as it has often been presented in the literature.<sup>2</sup> Rather, pseudotranslations often prove highly revealing for the understanding of cultures or cultural sectors and processes of change in them, and especially the role played therein by translations at large. Their significance has been amply demonstrated in the last few years, as the phenomenon has been rapidly gaining popularity, mainly among translation scholars.<sup>3</sup> I am proud to say that I have had a hand in bringing about this development.

### 1. Some uses of pseudotranslating

From the evolutionary point of view, the most significant aspect of the production and distribution of TL texts as translations into TL is the fact that this strategy offers a convenient and relatively safe way of breaking with sanctioned patterns and introducing novelties into a culture, and not only in the realm of literature. Indeed, it has often been one of very few roads open to writers to divert from norms and traditions without arousing too much antagonism, especially in cultures which were highly resistant to innovations. Given the fact that translations

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2. One early exception which confirms the rule is an article by Julio Cesar Santoyo published in 1984; although he certainly tackles pseudotranslating in a non-anecdotal fashion, he still approaches it first and foremost as a *textual* fact (a 'narrative technique', in his terminology) rather than in terms of its historical role (i.e., as a cultural fact), and even less so as a *translational* phenomenon. The article includes a select list of European pseudotranslations throughout the ages (Santoyo 1984: 50–51), which should prove useful to the reader.

3. E.g., Sohar 1999; Hung 1999; Apter 2005; Du Pont 2005; Rabadan 2000; Rizzi 2008; Tahir Gürcağlar 2008, 2010; Beebee and Amano 2010. See also fn. 5.

tend to be assigned *secondary* functions within a cultural (poly)system (Even-Zohar 1978a), there can be no wonder that deviations occurring in texts assumed to have been translated often meet with greater tolerance, and for this very reason.

One of the most extreme cases of introducing ‘innovations under disguise’ is no doubt *The Book of Mormon* (1830). Here, the novelties introduced into the American (Christian) culture of the first third of the 19th century by means of a text which was presented (and meticulously composed) as a translation, gave birth to a new Church, “The Church of Jesus Christ of Latter-day Saints”, which caused a redeployment of a lot more than just the religious sector.<sup>4</sup> One cannot but wonder what history would have looked like, had Joseph Smith Jr. claimed an angel had given him golden plates originally written in English, or had everybody around him taken the claim he did make, i.e., that the inscriptions on the plates used the characters of “Reformed Egyptian”, as a mere hoax!<sup>5</sup>

Innovativeness may also be sought on the level of the individual. It is normally connected with the previous activities of someone who is now seeking to change course and who would rather have his/her new endeavour dissociated from what s/he has come to stand for. Apparently a triviality, even choice of names may nevertheless play an important role here. As the real author now assumes two different functions, invented author and assumed translator, s/he may well decide to name one or both of these functions, to emphasize their alleged difference. The most common way is to invent pseudonyms, but one of the names may well be real (i.e., the real author is sometimes presented as just the translator). One of the functions (or both) may also remain nameless. The names may also be part of the larger guise, e.g. by hinting at the ‘foreign’ culture/language. Like many other features, name-giving may be likened to a dance, alternately taking a distance and coming closer. To be sure, on occasion, one of the author’s concealed interests may be to leave some traces, albeit not too obvious ones, in order to make it easier to claim ownership later on, if not to be actually exposed.

Two famous examples are Horace Walpole’s *The Castle of Otranto* (1765), “translated by William Marshal, Gent”, not accidentally “from the ... Italian”, and

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4. *The Book of Mormon* also has a literary facet. Thus, it has often 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).

5. It is interesting to note that a few years ago, Mormon scholars, mostly believers interested in theological issues, started turning to the notion of ‘pseudotranslation’ in an attempt to appease the many tensions between their strong wish to stick to their belief and various phenomena of a historical, linguistic and textual nature which have long been presented as stumbling blocks. (See especially Shepherd 2002; Tvedtnes and Roper 2003.)

Karen Blixen's *Gengældelsens Veje* [The Angelic Avengers] "by Pierre Andrézel", "translated into Danish by Clara Svendsen" (1944). Walpole was a born aristocrat who served as a member of the British Parliament and published mainly serious historical works, whereas Karen Blixen (Isak Dinesen) had won fame and awards for her literary writings, and the pseudotranslation she produced was her first and only full-scale work in the 'inferior' genre of the novel.

Blixen wrote *Gengældelsens Veje* during the Second World War in occupied Denmark, and the horrors experienced by her book's young heroines were taken as an allegory of Nazism. It is thus very clear that she was also trying to defend herself from identification by the authorities. (At the same time, she used her secretary's name for the translator.) This was not the only case of its kind. In fact, extreme historical conditions have often driven authors to disguise their texts as translations out of fear of censorial measures against them or their work. (See e.g. the case of Montesquieu's *Lettres persanes* [1721].) Then there is the naive claim that such a guise was used as a way to achieve commercial success. But why should a text with a foreign source be expected to bring higher profit than a domestic one? There are certainly serious socio-cultural reasons and motives involved in these cases too.

Be it as it may, the decision to disguise a text as a translation always implies a deliberate act of *subordination*, namely, to a culture which is considered prestigious, important, or dominant in some way. An attempt is thereby made to impart to the new text part of the prestige of the 'donating' culture as it is seen in the eyes of the persons-in-the-'domestic'-culture, as a way of directing, even manipulating, the reception of the new work by its intended audience.<sup>6</sup>

The decision to put forward a text as if it were a translation is always an ad hoc one. And yet, in certain cultures, circumstances seem to have prevailed which gave rise to a multitude of pseudotranslations in a short period of time, often from the same 'source' language/culture. Thus, a whole tradition came into being in the 'receiving' culture, a sub-system whose significance was much greater than that of the sum total of the individual texts. Such a proliferation may shed interesting light on the organization of the 'target' culture as a whole, as well as its relative position in the 'world language system' (see e.g. de Swaan 2001; Heilbron 1999, 2000). Above all, it highlights the position and role of translations, or possibly of

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6. In the 1820s, the period when *The Book of Mormon* was created, 'Old Egyptian' written in hieroglyphs aroused a lot of enthusiasm even among laymen, and Joseph Smith Jr. was certainly one of these. Not long before (1824), Champollion's decipherment of the Rosetta Stone, discovered in 1799, had been completed, and the American press, including local newspapers, carried stories about it.

a particular sub-system thereof, within the culture, which the pseudotranslators are aware of and put to use.

For instance, it has been shown (in Masanov 1963) that Russian literature of the beginning of the 19th century was hungry for texts which would have the identity of, and hence the prestige associated with, a type of British novel, which became known as 'Gothic novels'. In response to this felt gap, an internal demand ensued for a fill-in, which gave birth to a great number of books produced in Russia itself, and in the Russian language, which were presented as translations from the English. Quite a number of these books were attributed to Ann Radcliffe, whom the Russians regarded as the culmination of the Gothic novel.

Similarly, 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, both genuine and fictitious, all fulfilling the same needs.

As a third example of an overriding tendency towards pseudotranslating I would cite Rachel Weissbrod's demonstration of the decisive role fictitious translations, again mainly "from the English", have played in establishing certain 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, to the extent that they were produced at all, were considered highly inappropriate (Weissbrod 1989: 94–99; 355–356).<sup>7</sup>

Every cultural mechanism which can be grasped can also be put to deliberate use, for instance 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 under a totalitarian regime. Thus, in his memoirs, the Russian composer Dmitri Shostakovich (1979: 161–162) gives an instructive example of how pseudotranslating was used and misused – one might even say abused – in Stalin's Soviet Union.

According to Shostakovich's account, which is by no means a neutral one, there was an old Kazakh folk singer named Dzambul Dzabayev, who was famous throughout the Empire as a patriotic poet. Yet nobody has ever encountered this man's poems in anything but Russian, a language he himself did not speak. "And it turns out it was all made up. I mean, naturally, Dzambul Dzabayev existed as

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7. Hints of similar practices are often scattered in scholarly works of a historical kind 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.

a person, and the Russian texts of his poems existed too; the translations, that is. Only the originals never existed.”

The Russian ‘translations’ of Dzambul’s non-existent Kazakh poems were in fact written by “an entire brigade of Russian poetasters”, who, in turn, didn’t know a word of Kazakh. Some of them were rather well-known figures in the Soviet culture, which is why they were given the assignment in the first place: they knew only too well what was expected of them and of their poems. In spite of the fact that Dzambul “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 travelling throughout the Empire and promoting the poems by his personal appearances in public. The team (says Shostakovich) “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 Dzambul’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 also used in music and in several other arts, which makes the use of masks in Stalin’s Soviet Union part of a major culture-planning operation, and a very successful one too, from the point of view of those who thought it out: mere disguise applied systematically turned into outright forgery.

## 2. Pseudotranslations and Translation Studies

From what has been said so far it would seem clear that pseudotranslating is closely linked to genuine translation first and foremost in terms of the cultural position of such texts. That is, they come into the world disguised as translations not just because there exists a notion of translation, but because this notion and its realizations are assigned certain functions within the culture, which are, moreover, recognized and acknowledged by the-persons-in-the-culture, producers and consumers alike.

This in itself would have been reason enough to tackle fictitious translations together with genuine ones in any *function-oriented* kind of approach. However, there is much more to this status, which qualifies the strategy and its results to feature in product-oriented studies too, to a certain extent even in *process-oriented* studies, despite the lack of a genuine source text, and hence of real translational relationships. Thus, it is not as if anything and everything presented as translation would unconditionally pass for one, let alone without arousing any suspicion. An author wishing to put on a serious act as a pseudotranslator would do well to invest some effort in the attainment of that goal. S/he would not only have to find (or carve out) an appropriate niche for his/her prospective text (in terms of the systemic organization of the host culture), such as co-opting a (pseudo-)SL text-type which would be in keeping with it. S/he would also have to invest some effort in the formation and formulation of the text itself, in a way which would make it sufficiently persuasive to be accepted as a translation into TL in the period in question.

What pseudotranslators often do, to facilitate the attainment of this last goal, is incorporate in their texts features which have come to be associated, in the culture in question, with translation, and more often than not with the translation of texts of a particular type and/or from a particular source language and textual tradition. By enhancing the resemblance of their texts to genuine translations they would be making it easier for their texts to pass as such.

It is due to this practice that it is sometimes possible to reconstruct from a fictitious translation at least rudiments 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.<sup>8</sup> In fact, this is what Macpherson is believed to have done, when asked to produce the originals of his Ossianic poems: he proceeded from his English texts and 'backtranslated' some of them into a language which had many features of Gaelic in it but which did not amount to a known variety of the language. In fact, it may even be claimed that what we have here is a case of cultures in contact where the mediation does not rely on the presence of individual source texts but often involves the importation of features which are associated with the 'contributing' culture at large.

As is the case with parodies, which are akin to them in more than one respect, many pseudotranslations represent their pseudo-sources in a rather *exaggerated* manner. After all, the possibility, if not the need, to actually activate a non-existent original in the background of the text is often an integral part of its proper realization as an intended translation.

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8. Cf. the interesting case of the New Testament: the earliest known version of the book is in Greek, but at least parts of it were probably first put down in Aramaic or Hebrew and then translated into Greek.

For instance, occasional quotation from the Old Testament is one of the important literary devices of the New Testament, although it is used quite sparsely. By contrast, the pseudotranslator of the *Book of Mormon* hugely overdid it: about 25,000 words in his text consist of passages from the canonical English translation of the Old Testament, and about 2,000 additional 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, Smith did make 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). While the King James Bible uses the phrase in 1.45% of the verses, the *Book of Mormon* uses it in almost 20% of the verses! As Mark Twain put it in his sharp tongue: “If he had left that out, his [Smith’s] bible would have been only a pamphlet”. (From Chapter 16 of *Roughing It*, 1872.)

Of course, there is no one agreed method of counting. What is clear, however, is the fact that some of the occurrences of “it came to pass” were deleted from the second edition, which goes to show that Smith was (or became) aware of at least some of his exaggerations and was willing to introduce changes into the text. At the same time, it may be taken as a strategy of a genuine translator who wishes to improve his first version and hence serve as part of the disguise.

No wonder, then, that many pseudotranslations are in a position to give a fairly good picture of notions shared by the members of a community, not only as to the status of translated texts, but also as to their salient characteristics. “The point is that it is only when humans recognize the existence of an entity and become aware of its characteristics that they can begin to imitate it” (James 1989: 35), and overdoing something in imitation is a clear, if extreme, sign of such recognition.

Thus, while it was never my intention to claim that pseudotranslations provide the most central objects of Translation Studies, pseudotranslating (as a strategy) and pseudotranslation products (as its embodiments in language) nevertheless emerge as proper objects of study within DTS. In fact, they are no less an object of the discipline than normative pronouncements on translation – prefaces, afterwords, reviews, interviews, and suchlike – whose problematic status as sources of data on norms will be touched upon in Chapter 3; and for the very same reason: they testify to what a society has become *conscious of* in how it conceives of translation, a conception whose establishment constitutes one of the ultimate goals of the kind of studies advocated here.

### 3. The enlightening case of *Papa Hamlet*

To conclude our cursory discussion of pseudotranslations and their uses, let us dwell on some of the aspects of this phenomenon through a detailed presentation of one case in point. The text in question pertains to German literature of the end of the 19th century – another cultural sector where pseudotranslating was resorted to quite massively, with serious as well as parodistic intentions. (See fn. 13.) 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 the book contained three pieces of prose fiction: “Papa Hamlet” (pp. 11–90), “Der erste Schultag” [First School Day] (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 in translations that 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 the central passages of the introduction 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 begreiflicherwise nur so wimmelt, mussten in der deutschen Wiedergabe sorgfältig vermieden werden. Doch glaube ich, dass dies mir in den meisten Fallen gelungen ist. Ich habe keine Arbeit gescheut, sie durch heimische zu ersetzen, wo ich nur konnte. (Holz and Schlaf 1889: 8)

In 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 proclaimed source literature), had any idea about Mr. Holmsen and his literary career. All the information the reviewers supplied on these matters was drawn from the preface offered 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



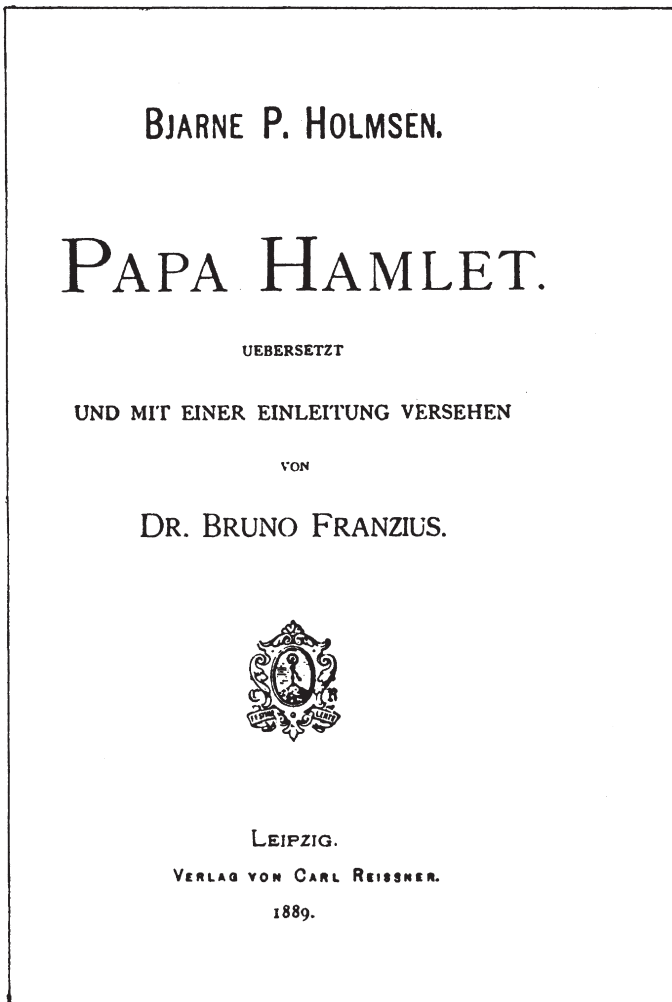


Figure 8. *Papa Hamlet* – title page

to those which a contemporary Scandinavian writer could have been expected to have. 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, although he was even less known to them than the author, having published

nothing before and having betrayed no 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 even claimed to have 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 be treated as a translation through and through.

Unless, of course, there is 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 no translation at all. Rather, the three texts were original German stories, the first results of the joint literary 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 name: /bjARNe HOLmsen/) and translator, fabricated the translator’s preface, including 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]).<sup>9</sup>

Thus, towards the end of the year it was the uncovered device of *pseudotranslation* which became a literary fact (in the sense assigned to this notion by Jurij Tynjanov [1967b (1924)]<sup>10</sup>) within the German culture. Later on this device almost completely lost its status and remained partly a biographical (or genetic) fact, partly a textual one (as 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

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9. Owing 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.

10. “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 Derzavin’s ist ein Milieufaktum, ein Freundesbrief der Zeit Karamzins oder Puskins – ein literarisches Faktum” (Tynjanov 1967c [1927]: 43; italics added).

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 the assumption of his knowledge of the Norwegian language and acquaintance with its literary scene. As it turned out, all this was *factually wrong*. Nevertheless, it had certainly been *functionally effective*.<sup>11</sup>

What were Holz and Schlaf trying to achieve by producing – and distributing – their work as if it were 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 which had been dominating the German literary scene – and in getting away with their gross deviation from dominating norms. And they chose to do so by adopting a series of norms and models of contemporary *Scandinavian* literature, which were indeed considered ‘naturalistic’ in a way different from the French ones, 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 parts of its centre. At the time when Holz and Schlaf were writing *Papa Hamlet*, however, German literature was still highly resistant to the new trends, and Scandinavian-like innovations were more or less acceptable only inasmuch as they were carried by texts of an actual 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.<sup>12</sup>

Nor was *Papa Hamlet* the only ‘Scandinavian’ pseudotranslation in Germany during those years, although in most of the cases, the *parodistic* element was much more marked.<sup>13</sup> No less significant is the fact that most of the texts in question

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11. 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.

12. In this connection, see also Zohar 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).

13. 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.

came from the very same literary circles in Germany, thus constituting a kind of sub-culture, or mini-tradition. One literary-historical factor which may have furthered Holz and Schlaf's decision to turn 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 translated into French; and the French translation, now regarded as an original, was then [back-]translated into German. (See Bleibtreu's foreword to the 1884 edition.) A similar route for *Papa Hamlet*<sup>14</sup> seems to have been blocked, maybe due to the fact that the mystery had been resolved early on.

What Holz and Schlaf actually did, in terms of introducing novelties while tying them to a hypothetical contributing tradition, was to embed a host of Scandinavian-like features in the stories. Needless to say, these features were not taken *directly* from Norwegian, or from any other genuinely Scandinavian works, to which the two had no access, not knowing any Norwegian. Rather, they were linguistic, textual and literary features used in previous German *translations* of impressionistic and naturalistic texts by Scandinavian authors. Looked at from the German vantage point, these were therefore features which had come to be associated, whether correctly or incorrectly, with Scandinavian texts. It is reasonable to assume that it was 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 two 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.

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14. 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).



## CHAPTER 3

# Being a norm-governed activity

The claim that, being a culturally-determined kind of activity, translation is basically norm-governed, is closely related to the observation that this activity is inherently (that is, non-arbitrarily) characterized by immense **variability**, both across cultures (in space or time) as well as within single ones. Let us dwell a little on these premises and follow some of the implications for DTS. The main aim will be to lay down some preliminary grounds for a **methodological framework** where controlled studies can be carried out on translation as it manifests itself under actual circumstances. The scope of the presentation will perforce be limited: the focus will mainly be on the *questions* that will arise, and to the extent that answers will be attempted at all, they will be tentative and fragmentary.

### 1. Pairing ‘translation’ and ‘norms’

Pairing ‘translation’ and ‘norms’ has more to it than the mere possibility of giving it theoretical justification, and its adoption as a starting point for descriptive-explanatory research promises to hold a lot in store. There is nothing radical about this pairing itself. I may indeed have to carry the responsibility for having instilled a heavy dosage of ‘norms’ into the veins of the discipline, but in itself, the association of the two has nothing to it which is truly new, let alone revolutionary.

Indeed, this practice has had several precedents, most notably (from where I am standing) Jiří Levý (1969 [1963]), James S. Holmes (1988) and Itamar Even-Zohar (1971). All three could easily have carried their thinking about translation well into the realm of norms, had they cared to do so, because the essentials were already there. Moreover, for each one of them, a number of predecessors could have easily been named, which would make it possible to trace the association of ‘translation’ and ‘norms’ further back in time as well as into other cultural and scholarly domains.

How, then, are we going to tackle the notion of norm in the context of the organization of socio-cultural activities?

## 2. From social agreements to norms

### 2.1 Agreements, conventions and behavioural routines

An important argument put forward by sociologists, anthropologists and socio-psychologists is that there must be some humanly innate flair for socializing, which some (e.g. Davis 1994) have named **sociability**. This faculty is assumed to be activated whenever a group comes into contact and starts exploring its situation with a view to establishing life together, whether this means the founding of a new community or just the sustenance of an existing one. As put by Davis, who tried to systematize the notion of ‘social creativity’ and render it serviceable in explaining the making and maintenance of social entities,

[p]eople use their given sociability to create *agreements about actions*. So, our worlds achieve the appearance of *stability and regularity* because we agree that certain actions are acceptable in appropriate circumstances, and others are not. (Davis 1994: 97; italics added)

Agreements about actions are far from given. Rather, they result from **negotiations** held in the group, whether language is used in this process or not. These negotiations breed **conventions**, according to which members of the group then feel obliged to behave in particular situations. With time, sets of accepted conventions may crystallize into quite complex **behavioural routines** which become a kind of second nature of people as members of a particular community.

What we create is – within agreed limits – a *predictable event*, from which certain choices have been excluded ... So when we are [socially] creative we attempt to *create order and predictability* and to *eliminate choice*, or at any rate to confine choice within certain prescribed limits. (Ibid.; italics added)

Insofar as a group is formed, or its existence sustained, continuous processes of negotiation, making agreements and establishing conventions-cum-routines emerge as vital for the very well-being of the group. What is not given in advance is the exact *shape* the processes would assume in any particular case, as that shape is a function of the prevailing circumstances. Sometimes it may even look as if “it could so easily have been otherwise” (ibid.). However, in retrospect, the choices actually made can usually be accounted for and explained: that is, the conventions themselves as modes of behaviour as well as how they were negotiated and came to be accepted.

The creation of a societal group requires time and usually involves power struggles. Sometimes this may be practically a never-ending process: as long as the group persists, social order is constantly being (re)negotiated, the more so when there are new members wishing to join the group or when there is a challenge by a rival group.

## 2.2 Conventions, norms and strategies

Conventions, the necessary outcome and manifestation of any struggle for order and stability, are at the same time a means for their attainment. However, in itself, a convention may be rather vague. It is neither specific nor binding enough to serve as a true guide for (and/or supply a reliable mechanism for the assessment of) actual instances of behaviour. Indeed, owing to their vagueness, the acquisition of social conventions often poses problems to newcomers. There must be a missing link here, which the notion of norm seems to be in a good position to provide.

Norms have long been regarded as the translation of general values or ideas shared by a community – as to what would count as right or wrong, adequate or inadequate – into performance ‘instructions’ appropriate for and applicable to concrete situations. These ‘instructions’ specify what is prescribed and forbidden, as well as what is tolerated and permitted in a certain behavioural dimension (see Figure 9).<sup>1</sup> They do so even if one refuses to accept that values act as *causal* elements of culture (i.e., as ultimate ends of a sort, towards which action is directed), and maintains instead that

... culture influences action not by providing the ultimate values toward which action is oriented, but by shaping a repertoire or ‘tool kit’ of habits, skills, and styles from which people construct ‘strategies of action’. (Swidler 1986: 273)

As long as a distinction is retained between what is culturally appropriate and what is inappropriate, there will be a need for ‘instructions’ to guide the persons-in-the-culture in their performance. Norms are therefore an important part of what Swidler and others would call a ‘tool kit’: while they are not strategies of action in themselves, they certainly give rise to such strategies and lend them both form and justification.

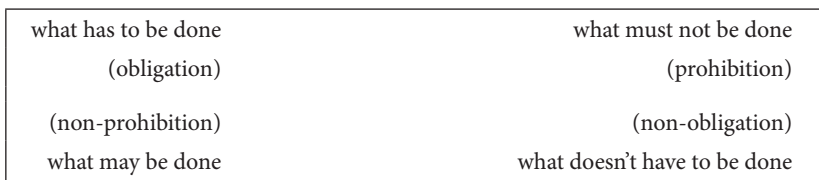


Figure 9. The semiotic square of normativity. Based on De Geest 2003: 208

1. The so-called ‘semiotic square of normativity’ has recently been elaborated with specific regard to translation, e.g. in De Geest 1992: 38–40; 2003: esp. 208–219.



Unlike the vague and fluid conventions, *norms* involve **sanctions**, actual or at least potential. Such sanctions can be **negative**, even **punitive** (in the case of the violation of a norm, or failure to act in accordance with it), or **positive** and **rewarding** (when one excels in abiding by a norm). Consequently, norms also serve as a yardstick for the assessment of instances of behaviour and/or their tangible results.

Not only can socio-cultural negotiations be held with no recourse to language, but the norms themselves may exist, be learnt and operate without ever being verbalized. This indeed is the main reason why the word ‘instructions’ has been enclosed in inverted commas. At the same time, giving a norm a linguistic formulation is always a viable possibility, whether in order to merely make it easier to discuss it (or its tangible manifestations), or as a way of imparting it, or knowledge about it, to others so as to secure social coherence (in the present) and continuity (with an eye to the future).

If such verbalizations do exist within a culture, what they indicate is a heightened awareness not only of the very existence of a norm, but also of its significance for the culture. Often they also imply certain interests of members of the group, in particular a desire to *control* behaviour, whether by dictating modes of conduct to others, or by trying to prevent such from being adopted. Be that as it may, normative pronouncements are *slanted* by their very nature and should therefore be handled with circumspection. Above all, one should be careful not to take them naively, at mere face value.

This warning notwithstanding, normative formulations may still be used as sources of data for an account of norms, albeit indirectly: if one wishes to lay bare the nuts and bolts of a particular behaviour, the available formulations – especially those used by the people-in-the-culture themselves – will have to be stripped of the interests they were intended to satisfy in order to make them fit for descriptive-explanatory use.

### 2.3 Regularities of behaviour and norms

There is a point in assuming the involvement of a norm only in cases **when the situation at hand allows for different kinds of behaviour**. In other words, norms imply the need to *select* from among a series of alternatives, not necessarily a final one, with the additional proviso that the selection be *non-random*.

Inasmuch as a norm is active and effective, it should therefore be possible to distinguish **regularities of behaviour in recurrent situations**, which is the manifestation of Davis’s “order and predictability” (1994:97). One consequence of the occurrence of such regularities and their recognition is that – even in cases when

the persons-in-the-culture are unable to account for the norm in any positive way – they can at least tell when sanctioned practices **have failed to be adhered to** (see e.g. Friedkin 2001).

As already indicated, regularities and norms are not just two words used to denote a single phenomenon. In fact, they are not even observable in the same way, let alone on the same level. Many of the regularities, some might say all of them, are the results of the activity of norms and may therefore be taken as direct evidence of their activity. The norms themselves will still need to be recovered from instances of behaviour, using the observed regularities as a clue. Thus, from the scholarly point of view, norms do not appear as entities at all, but rather as **explanatory hypotheses for actual behaviour and its perceptible manifestations**. What this actually implies is that the route taken by the researcher appears as a mirror image of the course presumably taken by the translator her/himself. **The researcher looks back from the end of the process towards all that has caused it to become what it did**. Consequently, the correlation between norms and strategies is hardly 1 : 1. In other words, one should not be surprised to find out that one strategy was fed by several different norms (syncretism), or, conversely, that a single norm was at the root of a number of different strategies.

#### 2.4 Gradation and relativity

The norms themselves are far from monolithic: not only are some of them more binding than others at any given moment, but their validity and potency may not be fixed for a very long time.

Firstly, in terms of their relative **potency**, constraints on any kind of behaviour can be described along a **scalable continuum anchored between two extremes: general, relatively objective rules on the one hand, and idiosyncratic mannerisms on the other**. Being intersubjective in nature, norms therefore occupy the central part of the scale, very often amounting to the whole continuum minus the small patches taken up by the two extreme points.

All in all, these constraints form a graded continuum reflecting their proximity to (or distance from) either pole: some of them are more rule-like, others are virtually idiosyncratic. The centrality of norms is not a mere function of their relative position along the posited scale. In a very real sense, all factors constraining behaviour are **variations of norms**, and it is really quite easy – and not in the least unjustified – to redefine them in terms of norms: rules as [more] objective, idiosyncrasies as [more] subjective [or: less inter-subjective] norms; this makes ‘norm’ the most central notion in the constraint domain.

Secondly, **the borderlines between adjacent types of constraints are diffuse**: there is no fixed point of passage from one to the other. Each one of the constraints in itself is relative too, depending on the point of view from which the field is observed or the context into which it is entered. Thus, what is just a favoured behaviour for a large and/or heterogeneous group may well be assigned a real binding force within one of the latter's subgroups, which is both *smaller* as well as more *homogeneous*; e.g., translators among text-producers, literary translators or subtitlers among translators, translators of poetry among literary translators, translators active in the cultural centre vs. translators operating on the periphery, younger vs. older translators, experienced vs. inexperienced, female vs. male, etc., etc.

Similar relativity can be discerned in terms of **types of activity**, forming either parts of each other (e.g. interpreting, or legal translation, or subtitling, within translation at large) or just occupying adjacent, at most partly overlapping territories (e.g., actual translation vs. translation criticism vs. translator training [see Section 2.3 below]). Thus, even if it is the case that one and the same person is engaging in several kinds of activity, and/or belongs to more than one (sub)group, s/he may well be found to abide by different sets of norms and manifest different kinds of behaviour (and maybe subscribe to a different value-system altogether) in each role and/or context of operation.

To be sure, a person of this last kind may also be in a position to **transfer** modes of behaviour and/or the norms they are associated with from one context to another, thus serving as an agent of contact and change. The ability to manoeuvre between alternative norms may well be a significant aspect of life in the socio-cultural dimension, and the acquisition of this ability surely forms an indispensable component of socialization.

Finally, along the **time axis**, constraints of all types may move up and down the scale and enter their neighbouring domain(s). Thus, under certain circumstances, mere whims may catch on and become more (and more) binding, and norms can gain so much validity and power that, for all practical purposes, they become rule-like (and the other way around). Needless to say, what was taken up in synchronic terms under the second observation above can also be projected onto the diachronic axis, which would compound the possibilities and complicate the task of the scholar's hunt for norms.

Shifts of validity and potency are intimately connected with **changes of status**, and hence with struggles for priority and domination, be it between (sub)cultures or within individual ones (which follows from their being regarded as **systemic entities**). Frictions about status may reveal themselves within a group, on whatever level, or attempts may be made to exert pressure from without (a claim which –

when stripped of its specific ideological overtones – is far from an innovation of postmodernist, feminist, post-colonialist and suchlike ways of thinking about society and culture; however, we introduce no presuppositions regarding possible, let alone necessary, ethical implications).

Whatever changes may occur, it can be taken for granted that they are connected with the notion of norm, especially since, insofar as the process repeats itself and social agreements are constantly (re)negotiated, **all constraints are likely to actually cross the middle ground on their way up or down**, and hence *become* norms for parts of their life.

### 3. Translation and norms

#### 3.1 Acts and events

It has become customary to maintain that any act of translation involves a unique encounter of a human agent with a single text, such that the locus of the act is **the human brain**. Since it appears as a mental activity, the argument goes, it should be possible – in fact, necessary – to account for translation in terms which are **cognitive** in essence.

I have no quarrel with such an argumentation as long as it is not pushed to absurd extremities, such as complete divorce of the mental from the environmental, i.e., the situation in and for which the act is performed. And by which it is further constrained. Thus, there is indeed a lot to be said for elaborating the cognitive approach. However, contrary to common belief, the relation between a mentalist approach to translation and its observation through a socio-cultural prism, which is at the root of the application of the notion of norm, is not really one of *opposition*. Rather, it is a relation of *complementarity* and *containment*. Thus, the two perspectives are not only justified and promising each one in itself, but they may well be of equal validity. This would call for closer cooperation between Cognitivists and Translation scholars. At the same time, it would always be necessary to bear in mind that **each perspective allows different kinds of questions** to be posed.

The key to alleviating whatever tension might still be felt between the mentalist and situational approaches is thus to be fully attentive to where the focus lies: is it on the internal structure of the process or on its embeddedness in a particular context? In other words, are we addressing the **act** alone, or the overall **event**? Clearly, no event can be said to have taken place unless an act was performed in its framework. However, it is no less clear that, at least in what has been referred to as

“socially-significant translation”,<sup>2</sup> no act is, or can be performed outside a particular context which serves to frame and condition it. It is not unthinkable that the internal and the external may even exert mutual influences. Unfortunately, however, so far we do not have a solid enough basis for tackling those relationships.

The next question applies to the notion of norm in its relation to the ‘act’ part of the ‘event’. Is such a notion not overly rigid, as has sometimes been maintained (e.g. in Schäffner 1999, *passim*)? Is it not the case that accepting the idea that a translation event is inherently norm-governed inevitably leads to the denial of *free choice* which is associated with a focus on the individual and her/his cognitive apparatus? – By no means! As Jewish wisdom has it, “All is foreseen but freedom of choice is granted” (*Pirke Avot* [The Ethics of the Fathers], 3:15). Surely, even within the socio-cultural paradigm, the actual decision is up to the individual. In spite of all the restrictions caused by responsibility to society (sanctions, remember?), translators are still given great latitude and considerable autonomy. It is precisely here that the norms intersect with the translator’s liberties and give rise to the decisions that are actually made.

To be sure, freedom of choice is at play not only when one’s behaviour involves *deviations* from prevailing patterns. It is no less present when one’s commitment to the norms is *reaffirmed*. The possibility is always there that one would be willing to take the risks which non-normative, let alone anti-normative (i.e., subversive) decisions may involve. However, one would normally try **to avoid negative sanctions for improper behaviour as much as to obtain the rewards which go with proper conduct**. Consequently, studying the sanctions, possible and actual, associated with translation and their effect on actual performance in defined settings is an important part of the study of both the translational event and the act of translation embedded in it. Among other things, it may help relate the concept of ‘norm’ to Bourdieu’s notion of ‘habitus’, an aspect whose importance in the context of translation has been stressed time and again in the last few years.<sup>3</sup>

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2. While socially-insignificant translation may exist (i.e. individuals translating for themselves – for the drawer, so to speak; e.g. Harris and Sherwood 1978) – this practice is surely negligible. Moreover – and more importantly – most socially-redundant cases of translation can be expected to imitate socially-relevant ones anyway, wittingly or unwittingly. Consequently, norms are bound to affect them too, and the same norms, at that (which is one important way in which *potential* sanctions are taken into account).

3. If I am not mistaken, the first to relate the notions of ‘norm’ and ‘habitus’ was Daniel Simeoni in a review article on the first edition of the present book (Simeoni 1998). In the meantime, a considerable amount of thinking has been devoted to the idea of regarding translating, translations and translators in sociological terms, most notably the volumes recently edited by Wolf and Fukari (2007) and by Sela-Sheffy and Shlesinger (2009–2010).

Many translators are known to have behaved differently, practically to the extent of producing diverse realizations of the notion of translation (superficially, different-looking texts), under different circumstances, including the mere identity of the commissioner of the task. They may do so in an attempt to enhance their chances of being re-employed by the same, or a similar agent, or at least escape the need to have their texts edited (and, from their own point of view, tampered with) by others, which many translators abhor.

### 3.2 The ‘value’ behind translation

By now, it should have become clear that translation is not a uniform kind of activity. It would be much more revealing to approach it as a class of phenomena which are connected by what Wittgenstein (1967: Section 67) called “family resemblance” (*Familienähnlichkeit*). Rather than sharing a cluster of essential features, members of such a family are conceived of as tied by “a complicated network of similarities, overlapping and criss-crossing” (1967: Section 66). This kind of resemblance also serves to demonstrate the lack of rigid boundaries between the individual members of the category, which therefore (as we have already seen) seem to flow rather freely from one domain to another.<sup>4</sup>

What this means is, that there is no single feature that all translations, in all cultures, past, present and future, will ever have in common; hence the insurmountable difficulties in producing a definition of translation (see Chapter 1, Section 4). The ‘value’ of translation, or the basic tools in a translator’s ‘kit’ (see Section 1.2 above), may be described as consisting of two principles whose realizations are interwoven in an almost inseparable way:

**the production of a text in a particular culture/language which is designed to occupy a certain position, or fill a certain slot, in the host culture,**

while, at the same time,

**constituting a representation in that language/culture of a text already existing in some other language, belonging to a different culture and occupying a definable position within it.**

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4. I first suggested that Wittgenstein’s concept be applied to translation back in 1979 (Toury 1980: 17–18). I then called for the investment of more effort in this direction. Unfortunately, my shaky background in philosophy prevents me from performing the job myself. See also Gorfée 1989.

These two principles have been termed **acceptability vs. adequacy**, respectively. Although the discrepancy between realizations of the two in reality may vary greatly, they should be tackled in isolation, as they are different *in principle*. The two cultures involved in a translation event may thus show greater or smaller similarities, whether by sheer coincidence or as a result of a previous history of contacts between them. What they can never be is identical. The normal state of affairs is a degree of *incompatibility* between adequacy and acceptability, so that any attempt to get closer to the one would entail a distancing from the other. Any concrete case thus involves an **ad hoc compromise** between the two.

Be that as it may, a translation will never be *either* adequate or acceptable. Rather, it will represent a blend of *both*. This is to say, no translation can reveal a zero amount of either adequacy or acceptability, no more than it can be 100% acceptable or 100% adequate. Moreover, since adequacy and acceptability are measured on different bases, and hence separately and independently of each other, their total should not be expected to yield 100 either, no more than it can amount to 200 (a hypothetical result of being fully adequate and fully acceptable at the same time). At the end of the day, it is the compromise between the two which will reflect the overall influence of the norms.

The joint activity of norms can be seen as a strong factor of *efficacy*. Thus, were it not for their strong regulative capacity, the tensions between a translation's adequacy (vis-à-vis the source text) and acceptability (vis-à-vis TL and the target culture) would have had to be resolved each time anew, on a fully ad hoc basis and with no clear yardstick to go by. Indiscriminate, totally free variation would have ensued, which would surely frustrate all attempts to offer a systematic account of a translational event. Everything might have looked equally significant alongside everything else, which reality teaches us it is not. Rather, as any cursory look will ascertain, translation as practised in a particular cultural context does tend to manifest quite a number of regularities, in terms of both criteria and the distance between them.

As is the case with all kinds of norms (see Section 1.3 above), here too, **regularities** are a key concept, and the establishment of recurrent patterns forms a basic requirement in the pre-explanatory phases of a study, when the data are being collected, classified and subjected to analysis, on the way to making first-level discoveries (see Chapter 1, Section 6). All in all, it is those regularities which will then be given an explanation, rather than any isolated phenomena which can be observed directly.

The beauty of socio-cultural behaviour in general – and this is very true of translation – is that 100% regularity is hardly ever to be expected, not even when one is down to dealing with the behaviour of one person finding her/himself in

similar situations during the translation of a single text. By the same token, zero regularity should be regarded as marginal too: looking deeper, or else expanding the body of data to be taken into account, is bound to yield more and more regularities.

What may seem more frustrating than a failure to come up with absolute findings (which is what ‘never’ and ‘always’ share) is that, very often, patterns will first reveal themselves in small numbers and low density. On occasion, it wouldn’t even be clear just what significance should be assigned to a regularity which is taking shape. One important problem here is a *methodological* one: namely, **the difficulties inherent in the establishment of sampling rules for translational behaviour and/or its results**. Obviously, any wish to take absolutely everything on board is untenable, even if there was a way of determining just what constitutes ‘everything’ and when that point will have been reached.

What one would normally start out with, in terms of materials for analysis, is far from a well-defined corpus built around an organizing principle. Rather, it is a more or less arbitrary selection – a number of texts, or a series of textual-linguistic phenomena, or at best a crosscut of the two – and there will always be the risk that such a heap would not only be overly heterogeneous and too sizable to handle, but also not representative of anything but itself.

The way to proceed from a mere heap of pre-data to an organized set thereof is to break up the initial material into subgroups, using as a basis one or another principle (variable) which will have emerged as significant during the initial scrutiny of the raw material. This procedure is likely to yield an increase in homogeneity too, reducing each subgroup’s randomness and enhancing its representativeness. Within these subgroups, a certain *order* will emerge, and if a resulting body of data then appears to be too small again, subgroups can be expanded on the basis of other variables. Such a procedure is bound to contribute greatly to the non-arbitrariness of the corpus, and hence to the significance of the study as a whole.

### 3.3 Uniquely translational norms?

Because of their conflicting sources, there is no way that the norms governing translation events would be identical to those found to operate in another field, be it ever so close to translation. At the same time, this does not mean that no correlations, even occasional partial overlaps, would be found to exist between different sets of norms. Indeed, a deliberate search for correlations may provide an impelling subject for many kinds of cultural studies beyond translation itself.



Norms may also be found to have been transferred from one field to another, although the significance of a transferred norm is bound to change, in keeping with its altered function. The same holds for cases where different groups engage in the same kind of activity, e.g. translation, but under different circumstances: consider for instance the different languages used in a bilingual society, or the ‘same’ language as it is used in different cultures.

Let us take a brief comparative look at translation and three closely-related activities: (a) communication in non-translated utterances, (b) translation assessment, or criticism, and (c) translator training.

a. *Communication in translations vs. non-translations in the same culture/language.* As any translation involves formulation in TL, a measure of overlap can be expected between translations and non-translations produced in the same culture/language. The norms directing this phase (or aspect) of translation may of course be more or less akin to those underlying a case of non-translation, but an act of translation cannot be reduced to mere formulation in TL, for which reason any perceived overlap can only be *partial*. The extent and nature of the similarities actually present and encountered will be connected with the position allotted to translation in the target culture, which – as we have been claiming – is purely a matter of norms.

On occasion, differences between translated and non-translated texts and the norms underlying them will find their explanation through their location in time. The norms which will have been found to underlie the make-up of translations may thus concur with those governing non-translations at a different period, either earlier or later. At the same time, as we have been able to see, attempts can also be made to imitate in a non-translational act of communication textual-linguistic behaviour in another culture/language (that is, in translations from it into TL), making the resulting texts bear a close resemblance to translations and fulfilling similar functions to theirs, with no felt need to even assume the existence of a proper source text. As shown in Excursus A, this option helps convey the (factually false but culturally functional and effective) impression that the texts in question had indeed been translated. As for the role of the audience, they will often activate the implanted similarities, being led astray by the presence of features which will have come to be associated with genuine translations.

There may be various reasons for, and many ways of, blurring the borderlines between translations and non-translations within the same culture. In extreme cases, the very opposition between being ‘translated’ and ‘non-translated’ may lose its cultural significance. While the activity itself may still be recognized, it will have lost its systemic standing and be conceived of as just another way of producing TL texts.

b. *Translation vs. translation assessment.*

These two activities differ in another way: namely, in terms of their respective products. While both yield textual entities, the texts they yield are of a different kind and order: whereas during an act of translation, translational norms are actually **mobilized**, assessors busy themselves first and foremost in constructing an **argument** about translations which are already there, and the norms underlying them.

True, sometimes translational norms do play a subsidiary role in acts of assessment too, but they do not have the same role. Thus, as long as it is evaluation which is at stake, all that one is expected to do, vis-à-vis translational norms, is **react** – either to the norms as principles underlying the assessed translation, or to how they seem to have been employed.

Of course, one text (or translator) may well draw different assessments from different persons, even when they all share the translator's values and attitudes. Thus, an assessor may favour the norms reflected by the text but be critical of their application, and especially of its visible results. In other, more extreme cases, translators and their evaluators may subscribe to different norms altogether. Since our focus is on socio-cultural **roles** rather than on their embodiments, there is no wonder that norm differences may occur even in the case of individuals wishing to play both roles (translator and assessor).

True enough, assessments are often not purely evaluative. They may also involve elements of non-evaluative activities, e.g. the extraction of norms from assumed translations, as mentioned above. Assessors may even give the extracted norms a verbal garment and intertwine the results within their argumentation. What they are *not* expected to do is **supply an alternative translation**. Even though such an alternative often does reflect the author's critical stance, the activity as such will have undergone a major change.

c. *Translation vs. translator training.*

This is probably the trickiest pair of all. One might be inclined to think that persons entrusted by society with the preparation of cadres of new translators in an ordered way and at an accelerated pace, would see their task as **imparting to the non-initiated certain modes of behaviour, in the form these behaviours are practised in society**, thus training them for the role they will be expected to play. Unfortunately, this is often not what happens. What many trainees are given instead is an admixture of concepts and practices imported from sources that are deemed more 'respectable' than mundane behaviour; mainly from scientific disciplines which have gained prominence, at one time or another. The intuitions underlying these concepts cum practices are often fine, but they tend to be seasoned with more than a speck of wishful thinking, the logical fallacy whereby mere wishes and desires stand for realities. In the most extreme cases, the claim is

even made, at least implicitly, that there are things that simply *have* to be (or else should *never* be) done – not as a mere socio-cultural preference (i.e., as a norm) but as an apparent truism.

Of course, teachers should not be expected to behave in a totally neutral way. In fact, their behaviour is always underpinned by a certain world-view which may easily urge her/him to *manipulate* real-life norms. Thus, many teachers see it as their obligation to **effect changes** in the world of our experience, wishing to partake of the ongoing process of (re)negotiating the norms and shaping the translational sector of the target culture. Very often, they proceed from a general impression that the current situation is bad, and in urgent need of improvement, which they purport to take upon themselves. Thus, they are basically trying to fulfil a *remedial* function with little or no regard for the prevailing norms, and even less concern for the reasons why they have come to be – and are – what they are. After all, there are always *reasons* for a normative situation which should not be equated to mere ignorance, which many like to blame their students (and others) for. From the socio-cultural perspective, what is important is the fact that, whatever teachers may try to do, they do it from a position of great *power*, which has been bestowed on them by the establishment, not even necessarily on the basis of their own merits as translators, critics or scholars in the culture.

In fact, training institutes often behave very much like old-time guilds of craftsmen, whose position was somewhere in between that of trade unions, cartels and secret societies. They developed conventions and norms of their own, which they then tried to defend as well as impose on newcomers to the field, and through them on society as a whole. To be sure, the victory of these modern guilds is all but assured. In fact, very often the institutes turn out to be just one out of a number of options existing in the culture, some stronger, others less strong, which work together to create a balance in the field, the proverbial “parallelogram of forces”, and define that balance.

It is no wonder then, that, unfortunately, the transition from the hothouse of a training programme into the wider world is not always smooth. In extreme cases novices may come to suffer real agony, even frustration, especially if and when they find themselves pressured to *unlearn things they have been taught* and adjust to the norms that actually operate in the culture at large, not seldom the very same norms their teachers wished to see done away with.

### 3.4 Where are translational norms negotiated?

If agreements and conventions may be constantly negotiated, and if norms are one of their outcomes and modes of implementation, it would only be proper to enquire as to where those negotiations take place and who can be expected to take part in them. Here are some of the questions which seem pertinent:

- How homogeneous (or heterogeneous) would the group be?
- Would it always consist of members of the same limited number of categories, and how much room would there be for variation and change?
- More specifically, would the group consist of acting translators only, who will actually implement the norms, or would it include persons who play other roles as well, be it in the production of the texts themselves (editors, revisers, teachers, especially of translation, critics, censors, publishers, etc.) or around it (language teachers)?
- And what about the *consumers* of translations: under what circumstances would they be granted permission to take part in the negotiations, let alone be listened to? And would the notion of ‘group’ not be stretched too thin as a result of such an expansion, and its basic homogeneity be compromised?
- In a heterogeneous forum, how much power would be allotted to the practitioners themselves in the creation, negotiation, maintenance and change of norms? Would they be located in the systemic centre or closer to the periphery?
- And what about individuals who play more than one role in the field? Is it all that certain that they would represent similar norms when assuming their different positions?

This is an intriguing domain about which Translation Studies has barely started collecting and processing data. There seem to be several alternative patterns here: there may be larger and smaller groups involved in the negotiations, more or less closely-knit or diffuse, more or less homogeneous, more or less rigid in their (personal or sectorial) make-up, etc. And it is not totally unjustified to assume that these differences would manifest themselves as significant in their implications for translation behaviour and the norms it is governed by.<sup>5</sup>

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5. The interesting question of how an individual acquires the translational norms pertinent to his/her culture and becomes a culturally-relevant translator will be dealt with in Excursus C.

### 3.5 Alternative and competing norms

One thing which makes decision-making in translation less demanding than it may have sounded so far in terms of the risks taken,<sup>6</sup> although more complex in terms of its mechanisms, is probably the fact that, at every point in the life of a social group, especially a comprehensive and/or heterogeneous one, there tends to be **more than just one norm for each behavioural dimension**. Again, the need to *choose* between alternatives is built into the very system, so that socialization *re* translating often includes the acquisition not only of the alternatives themselves as a list of options, but the **ability to manoeuvre meaningfully among them** as well.

To be sure, a multiplicity of alternative norms is not tantamount to the lack of binding force in a culture. Much less does it lead to anarchy. For it is not as if all the norms which are available are equally accessible and of exactly the same status, so that choice between them is not devoid of implications for the assessment of a person's behaviour and/or his/her position within society. Manoeuvring between alternative modes of behaviour thus exposes just another norm-governed domain, entailing risks of its own.

By the same token, norms operating within one and the same group may be not just *different* from each other. Quite often they are literally *competing* with one another. After all, the dynamic structure of a living society is characterized by an ongoing struggle for domination. The result is that norms may change their position vis-à-vis the systemic centre of gravity, the more so as the centre itself is often shifting.

What complicates matters even more is the fact that each group-within-a-group (and the groups tend to be hierarchically organized) may have its own structure of centre and periphery, entailing internal struggles for domination in addition to (although sometimes as part of) its participation in the struggle of the group as a whole vis-à-vis other groups.

Firstly, there is **variation within a culture**. Whether within one (sub)system or between the various (sub)systems regarded as constituting one higher-order

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6. Not long ago, Anthony Pym suggested that translation be tackled in terms of 'risk taking', which for him is "the possibility of not fulfilling the translation's purpose" (Pym 2005). He even formulates some 'rules' for teaching translation, most notably: "When translating, work hard on the high-risk elements, and do not work too hard on the low-risk elements". Such a rule may indeed come in handy for teaching purposes, but it is still (or again) not based on what translators actually do. Nor does Pym contest this. As he himself says: "We would hope that the above are lessons that all professionals apply, probably intuitively (this is a supposition that we should be testing empirically)". A possibly better way of thinking about it is to say that mastery of the norms and the ability to manoeuvre between them may help reduce the risks taken by a translator.

(poly)system, it is not rare to find side by side three types of competing norms, each having its own followers and a position of its own in the culture:

- norms that dominate the centre, and hence direct translational behaviour of what is recognized as the *mainstream*, alongside
- remnants of *previous* mainstream norms, which are still there but have grown weaker and become relegated to the margin, and
- rudiments of what may eventually become part of a new set of norms.

Consequently, as is the case with any other socio-cultural domain, it is possible to speak – and not derogatorily either – of being ‘trendy’, ‘old-fashioned’ or ‘progressive’ in translation too.

Secondly, **changes may occur over time**. One’s status as a translator, in terms of the norms adhered to, may of course be temporary: translators may fail to adjust to changing requirements, or do so to an extent which is deemed insufficient. Thus, as changes of norms occur, progressive translators may find themselves just trendy, on occasion even downright *passé*, unless they make efforts to keep up with the changes.

At the same time, regarding this process as reflecting mere *age differences* can be misleading. While age may indeed be a relevant variable in determining a translator’s position along the ‘dated’–‘mainstream’–‘avant-garde’ axis, it cannot, and should not, be taken as inevitable. As reality teaches us, it is often people who are in the *early* phases of their translation career, whether young or not so young, who show the most standard behaviour. Insecure as they understandably are, novice translators would try to avoid taking risks – in other words, play safe – thus performing according to norms which, though they may have become dated, are still considered ‘respectable’. One way of explaining this is to realize that societies would rather mark a deviant performance of a novice as an ‘error’ rather than ‘innovation’: while both terms may easily be applied to one and the same kind of behaviour (or its observable products), the different *values* assigned to them make all the difference in the data interpretation.

Conservative tendencies may be perpetuated, even further enhanced, if novice translators receive encouragement from socialization agents, especially powerful ones, holding to dated norms themselves. No wonder that fairly rapid revolutions – i.e. large-scale changes of paradigms – have often been made by experienced translators who had, moreover, attained considerable prestige in the host culture, often by behaving in full accord with mainstream norms. Having internalized those norms, and having been granted more than mere recognition by society, they can afford to start practising deviations from accepted patterns of behaviour. However, only when they have been followed by others, will a new norm be seen as having been truly introduced into the culture.



## Studying translational norms

The previous chapter was devoted to validating the centrality of the notion of norms for socio-cultural behaviour of whatever kind, putting translation into relief against that general background. We will now probe a little deeper into the nature of norms in the specific contexts of translation, and then discuss their role within a target-oriented version of DTS.

### 1. The initial norm in translation

It has proved useful to regard the basic choice which is made – whether consciously or not – between the two contending sources of constraints comprising the value underlying translation (see Chapter 3, Section 2.2) as constituting an **initial norm**. In keeping with that concept, any translator is called upon to make an overall choice between two extreme orientations: heavy leaning on the assumed original (**adequacy**<sup>1</sup>, in our terminology), and sweeping adherence to norms which originate and act in the target culture itself, thus determining the translation's **acceptability**, whether as a TL text in general, or, more narrowly, as a translation into that language.

If the first stance is adopted, the translation act will be dominated by attempts to have the ensuing text reflect the source text along with the norms embodied in it, and through them features of SL itself, or of a particular tradition within it, occasionally even of the source culture at large. Such an attitude is likely to result in certain **incompatibilities** with normal target culture practices.

If, by contrast, the second stance is taken, target norms will be triggered and set into motion, thus relegating the source text and its unique web of relations based on SL features to a secondary position as a source of constraints. Obviously,

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1. The best definition of 'adequacy' in the sense it is used here (which is not universally accepted) is Even-Zohar's: "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" (1975:43; my translation).



target-orientedness of this kind may assume more than one shape and the distance between its realizations and the source-oriented norms may vary in nature and extent. Be the distance as it may, **shifts from the source text** (or its ideal reconstruction in TL, if it lets itself be underpinned to begin with) would be an inevitable price for taking that course of action.

Clearly, such shifts are to be expected even in the most extreme adequacy-oriented translation. In fact, at least since Popovič (1978), shifts have been recognized as a distinctive feature of translation. And although I have been growing suspicious of the quest for ‘universals’ in the realm of translation, which I for one would rather attempt to reformulate as probabilistic laws (see especially Toury 2004a, b), the inevitability of shifts is still one of the best candidates to serve as a translation universal. Some may well say: the only real one. (We will return to this issue in Part Four.)

Even if the need itself to deviate from SL-text patterns is inevitable, there will never be just one way to realize that need. Moreover, this inevitability applies not only to so-called non-obligatory shifts, but to obligatory ones as well;<sup>2</sup> and to the extent that the actual realization is non-erratic, it appears as truly norm-governed.

All this notwithstanding, one should take care not to read too much into the initiality of the initial norm. First and foremost, it should be clear that it is not intended to cover any hidden judgment. Associating it with the highest-level decision (in contradistinction to all the other choices, which are all of a lower level and greater specificity), lends it *logical* priority. At the same time, it does not follow that a choice of this kind is made just once during the act, or even that it is always the first choice to be made. Rather, the choice between adequacy and acceptability may be (or should I say: is?) repeated time and again during the act, whereby proximity to either extreme serves as a central feature of lower-level decisions. In fact, so strong is the initial norm that other decisions tend to reflect it even in cases where no overall choice has been consciously made to prefer one source of constraints over the other.

To be sure, the notion of the initial norm is intended to serve first and foremost as an *explanatory tool*. Thus, even if no clear macro-level tendency can be discerned towards either adequacy or acceptability, it should still be possible – and helpful – to account for **micro-level decisions** in these terms. On the other hand, in cases where an overall choice *can* be pointed out, it is by no

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2. Obligatory shifts stem from *systemic* differences between the languages involved in the act. As Roman Jakobson has taught us, “[l]anguages differ essentially in what they *must* convey and not in what they *may* convey” (1959: 236).

means necessary to assume that every single low-level choice was made in full accord with one and the same initial norm. As we have seen (Chapter 3, Section 2.2), it would be unrealistic to even expect absolute regularities. In fact, I would now make an even stronger claim: namely, that – except perhaps on the very margin – encountering 100% regularity should arouse **immediate suspicion** as being too good to be true.

The opposite question, i.e., how *small* a pattern would still count as a regularity, is a mute question. I fail to foresee any answer which would not just be made ad hoc for a particular case, and in a tentative manner, and the only rule of thumb I can think of is ‘the bigger – the better’ (for the research work). The methodological implication is clear: searching for regularities, one would find oneself going up and down a scale of generalization, an incessant pursuit to be sure, yet one which should never lead one to give up the search and refrain from looking for even more regularities.

We have now arrived via a slightly different route at the claim that actual translation decisions will normally be found to involve some combination of, or compromise between, the pressures of the two extremes, the choice between which constitutes the initial norm. Still, mainly for methodological reasons, it seems profitable not to give up the two extreme poles but retain them as distinct *in principle* even though neither of the extremes would ever reveal itself in a pure, unmitigated form. Otherwise, how would we be able to distinguish between and account for different kinds and extents of compromise?<sup>3</sup>

## 2. Translational norms: an overview

Norms can influence not only translation of all kinds, but also at every stage of the act. Indeed, traces of their activity can be noticed in every aspect of the end-product. From the point of view of the overall process in which a translation comes into being (as against what goes on in the brain of one individual), norms may well be brought into the picture, and norm-governed behaviour may also take part in the event (including the actual generation of the resulting text), e.g. by editors, revisers, publishers, censors, proofreaders, etc. Different persons may

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3. It should also be emphasized that being norm-governed applies to translation *of all kinds*: not only of literature, philosophy or the Bible, which is where scholars have always been prone to look for norms. As has been demonstrated (for the first time in a brief exchange in *Target* (Shlesinger 1989b followed by Harris 1990), the same approach can also be applied even to conference interpreting.

share attitudes, in which case their decisions will tend to reinforce each other, or they may subscribe to non-concurring, even flatly contradictory norms, which would make it much more difficult to reconstruct the translation event underlying a given corpus and the act implied by it.

It has proved convenient to first distinguish two larger groups of norms applicable to translation: preliminary vs. operational norms.

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

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

Considerations concerning *directness of translation* involve the threshold of tolerance for translating from languages other than the ultimate SLs: is indirect translation permitted at all? In translating from what languages / text-types / periods (etc.) is it permitted / preferred / prohibited / tolerated? What are the permitted / prohibited / tolerated / preferred mediating languages and why are they given that status? Is there a tendency ignored / camouflaged, maybe even denied? If the fact is mentioned, is there a tendency / obligation to mark a translation as having been mediated, and is the identity of the mediating language made known? And is the language mentioned as a mediator necessarily the one which has served the translator? (An example of mediated translations given a strictly socio-cultural motivation will be taken up in Chapter 9.)

**Operational norms**, in turn, may be conceived of as directing the decisions made during the act itself. These norms affect the text's matrix – i.e., the way linguistic material is distributed in it – as well as its textual make-up and verbal formulation. Directly or indirectly they thus also govern the *relationships* that would obtain between target and source texts or segments thereof; i.e., they determine what would more likely remain intact despite the transformations involved in translation, and what would tend to get changed.

So-called *matricial norms* govern the very existence of TL material intended as a replacement of corresponding SL material (and hence the degree of *fullness* of translation), its location in the text (or the way linguistic material is actually *distributed* throughout it), as well as the text's *segmentation* into

chapters, stanzas, passages and suchlike.<sup>4</sup> The extent to which omissions, additions, changes of location and manipulations of segmentation are referred to in the translations themselves or in the 'paratexts' surrounding them (Genette 1997), may also be determined by norms, even though the one can very well occur without the other. As already said, on occasion, they may even be attributed to different persons.

The borderlines between the different matricial phenomena are not clear-cut. Thus, large-scale omissions often entail changes of segmentation as well, especially if the boundaries of the omitted portions or their textual-linguistic standing are not very well defined. By the same token, a change of location may sometimes be described as an omission at one point in the text combined with an addition at another one. The decision as to what might have 'really' taken place, especially if what was encountered deserves to be seen as a case of compensation, is thus description-bound. However, as I keep stressing, what we are actually after is (more or less cogent) *explanatory hypotheses* anyway rather than 'true' accounts, which is something we can never be sure we have achieved anyway.

Finally, *textual-linguistic norms* govern the selection of linguistic material for the formulation of the target text, or the replacement of the original material. Norms of this kind may be more or less general, e.g. apply to translation as such, or to one text-type and/or mode of translation only. Some of the textual-linguistic norms may be similar to the norms governing non-translational text-production in the same culture (see Chapter 3, Section 2.3), but such a similarity cannot be taken for granted. Much less can it be adopted as a framework for actual research. In other words, it would be methodologically wrong to proceed from the assumption that the language of a particular assumed translation, let alone of translations in general, is representative of TL or any subsystem or tradition thereof. (This point will be taken up in Chapter 13, with the focus on translation-specific lexical items.)

Obviously, preliminary norms enjoy both *logical* and *temporal* priority over the operational ones. This is not to say that between the two groups there are no relations whatsoever, including mutual influence, or even two-way conditioning. However, these relations are not given in any way, so that their establishment

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4. 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 universals as possible. In actual fact, there have been various traditions (or 'models') of segmenting a text, 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.

forms an integral part of the very study of translation. Nevertheless, we can safely assume at least that the relations which would be found to exist will have to do with what we have identified as the ‘initial norm’. They might even be found to *intersect* in it, another important reason to retain the opposition between ‘adequacy’ and ‘acceptability’ as a basic coordinate system for the formulation of explanatory hypotheses.<sup>5</sup>

Operational norms may be said to constitute a model, in accordance with which translations come into being; such a model may involve the norms realized by the source text (i.e., adequate translation) plus certain modifications, or purely target norms, or (most commonly) an ad hoc 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 at the same time. Consequently, when the first position is fully adopted, the translation can hardly be said to have been made ‘into’ TL. Rather, it was made into a model-language, which is at best some **part** of TL and at worst an artificial variety which has no existence anywhere else.<sup>6</sup> In this last case, the translation can be said to have been *imposed* on the target culture rather than ‘introduced’ into it. In extreme cases it may indeed behave like an alien implant, which may even be *rejected* by the target culture. To be sure, in the course of time, such an implant may carve a niche for itself and its possible followers in the target culture and lose its alienness (consider the case of King James’ English version of the Old Testament!), but the fact that initially, there was no attempt to accommodate it to any ‘slot’ existing in the target culture, is bound to retain its historical significance.

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5. For instance, in cultural sectors where the pursuit of adequate translation is marginal, it is not inconceivable to find that indirect translation would be rather common too, on occasion even preferred to direct translation. By contrast, a norm which blocks mediated translation is likely to be connected with a greater 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 openly denied.

6. And see, in this connection, Izreël’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 made 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 flavour 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 into grammatical and syntactic structures which were marked for belonging to the written varieties, which also meant pouring ‘new’ into ‘old’.

By contrast, when the second position is adopted, what would be introduced into the target culture will be some distance from the original text itself. Thus, for instance, Hebrew (like English) culture has never had, nor is it ever likely to have, the novel *Преступление и наказание*, only possibly *versions*, or *representations* thereof, each one of them cut to the measure of a different model and possibly (but not necessarily) bearing the titles of *Crime and Punishment* or *ha-xet ve-onšo*. (For a detailed discussion of some literary examples see Chapter 10 and Excursus B.)

The apparent contradiction between the 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 **the type and extent of equivalence actually exhibited by a translation vis-à-vis its source are determined by norms**. The study of norms thus constitutes a vital step towards establishing just how the functional-relational postulate of equivalence has been realized, be it 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 subset. (See a detailed discussion of “Equivalence and Non-Equivalence as a Function of Norms” in Toury 1980a: 63–70.)

What this approach clearly entails is a wish to retain the notion of equivalence, which various contemporary approaches have tried to do away with, while introducing one essential change: a move from an ahistorical, largely prescriptive concept to a historically-oriented notion with a descriptive potential. Rather than being a single type of relationship, anchored in a recurring invariant, it now refers to **any relation which is found to have characterized translation under a specified set of circumstances**, which would bring us back to the use we made of Wittgenstein’s “family resemblance” (see Chapter 3, Section 2.2).

Towards the end of a full-fledged study it will probably be found that translational norms, and hence the realization of the equivalence postulate, are all, to a large extent, dependent on the position held by translation – the activity and the people performing it as well as its products – in the target culture. (And see Even-Zohar’s most influential article on “The Position of Translated Literature within the Literary Polysystem” [1978a].) An interesting field for study is therefore comparative research, examining the nature of translational norms as compared to those governing non-translational kinds of text-production in the same culture. To be sure, this kind of study is absolutely vital, if translating and translations are to be appropriately contextualized. Needless to say, **non-translations comprise part of the context of translations, as well as vice versa**.

### 3. 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 features inherent in the very notion of norm, and are therefore not unique to translation: the **socio-cultural specificity** of norms and their **potential 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 culture. 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 over time between subsystems within a single culture, or between entire cultural entities, and hence a manifestation of interference.<sup>7</sup> Even then, it is often a matter of apparent *similarity* more than of a genuine *identity*. After all, whatever cultural significance a norm might have is assigned to it by the network of relations it is embedded in, and those networks remain different even if certain instances of behaviour within them look alike.

Norms are also **unstable** entities. They are liable to change, not owing to any flaw, but by their very nature as norms. At times, norms may change very quickly; at other times they are more enduring and the process of change takes longer. Either way, substantial changes, in translational norms too, often occur within one’s life-time and are therefore relatively easy to detect. (See e.g. the case of the three “Killers” in Chapter 5, Section 2.)

All this is not to say that all translators remain passive in the face of such changes. Rather, many of them, through their very activity, help shape the process, as do translation criticism, translation ideologies (including those emanating from academia, often in the guise of theories), 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 a natural, dynamic course of events and to divert it according to their own preferences. Yet the success of these 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 any attempt to further clarify them would require a lot more research.

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 events and the consumers of their

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7. For an attempt to tentatively formulate some general rules of systemic interference, see Even-Zohar 1990: 53–72.

products – do so only up to a point. As we have seen (Chapter 3, Section 2.5), it is not rare to find side by side in a society **three types of competing norms**, which we have called **mainstream, old-fashioned and avant-garde**, each one with its own adherents and position in the culture at large.

Finally, *non-normative*, even *anti-normative behaviour* is always a possibility too. The price for opting for this possibility in translation may be as low as a felt need to submit the end-product to revision. However, it may also be far more severe, to the point of losing one's recognition as a translator. On the other hand, in retrospect, deviant instances of behaviour may be found to have effected *changes* in the system as a whole. This is why they constitute such an important field of study, as long as they are regarded as what they have really been and are not put indiscriminately into one bag with everything else. Implied are intriguing questions such as who is 'allowed' to introduce changes, and under what circumstances such changes may be expected not just to occur, but to be adopted by others as well and influence the system as a whole.

#### 4. Extracting translational norms

So far we have been discussing norms mainly in terms of their activity during a translation event and their effectiveness in the production of translated texts. To be sure, this is precisely where and when they exert their power. However, what is available for observation is never the norms themselves, but rather **instances of norm-governed behaviour**, or – to be even more precise – their end-products. Thus, even when acts of translation are claimed to be approached directly by the researcher, as is the case of the use of 'Think-Aloud Protocols' (see Chapter 14, Section 3), it is only **products** which are susceptible to direct study, albeit products of different kinds and orders.

There are two major sources of data which may be used to reconstruct translational norms, textual and extratextual:<sup>8</sup>

1. **Textual sources:** the translations themselves, for all kinds of norms, as well as analytical inventories of translations established within a research project and for its needs and assigned the status of a virtual text for various preliminary norms;
2. **Extratextual sources:** semi-theoretical or critical formulations such as prescriptive 'theories' of translation, statements made by translators, editors,

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



publishers, and other persons involved in or connected with the event, critical appraisals of individual translations, or of the activity of a translator or 'school' of translators, and so forth.

Between these two types of source there is a fundamental difference. Texts are primary **products** of norm-regulated behaviour. They 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 other attempt to formulate a norm in language, they are partial and biased, and should therefore be treated with every possible circumspection, all the more since – emanating as they do from interested parties – they are likely to lean toward propaganda and persuasion. Moreover,

in the social and socially applied sciences, it is customary ... to distinguish between **attitudinal norms**, which have to do with "shared beliefs or expectations in a social group about how people in general or members of the group ought to behave in various circumstances" (Perkins 2002: 165), and **behavioural norms**, which have to do with "the most common actions actually exhibited in a social group" (*ibid*). Attitudinal norms do not necessarily determine behaviour; as Perkins puts it (*ibid*), "How most other community members believe everyone should behave and what behaviour is most common may be correlated, of course, but each component may also be somewhat distinct". (Malmkjær 2008: 51)

There may therefore be gaps, even contradictions, between explicit arguments and demands, on the one hand, and actual behaviour, on the other, due either to subjectivity or naiveté, or even lack of sufficient knowledge on the part of those responsible for the verbalizations. 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 translation act itself is already over); and the way the 'real', possibly concealed intentions are then realized in practice may well constitute a further, third group of data.

Yet all these reservations – proper and serious though they may be – should not lead us to doubt the legitimacy of semi-theoretical and critical formulations as sources for controlled studies of norms. In spite of all its faults, this source still has its merits, both in itself and as a key to the analysis of actual behaviour. However, if the pitfalls inherent in them are to be avoided, we should make it a rule **never to take normative pronouncements at face value**. They should rather be regarded as pre-systematic *explicanda* and given explication in such a way as to place them in a narrow and precise framework, lending the resulting *explicata* the coveted systematic status (see Carnap 1950: 3–15). While doing so, an attempt should be made to clarify the status of each verbal formulation encountered in

the field, however slanted it may be, and uncover the sense in which it was more than a mere accident; in other words, to find out how, in the final analysis, that formulation reflects the cultural constellation within which, and for whose sake, it was made.

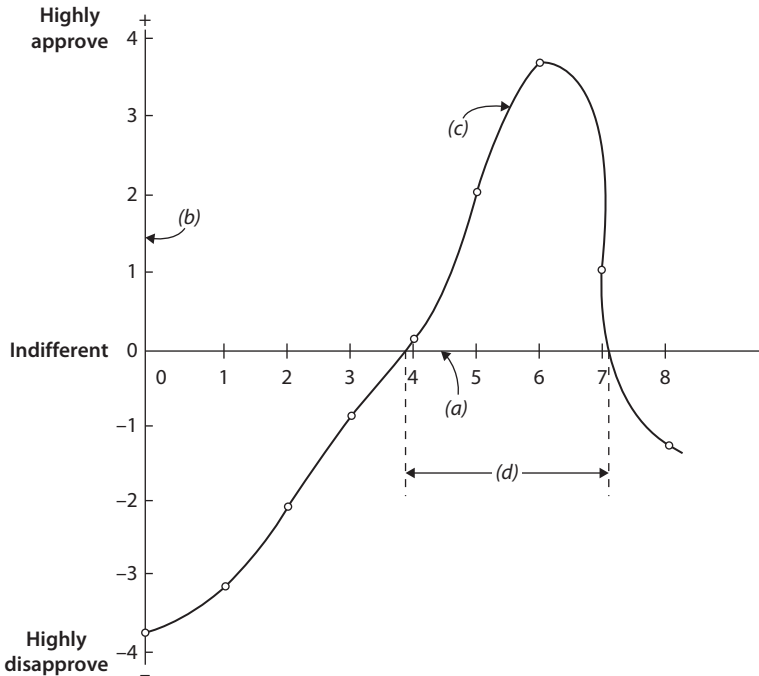
Apart from sheer speculation, this process would involve the comparison of various normative pronouncements with each other, as well as their repeated confrontation with the patterns revealed by (the results of) translational behaviour and the norms reconstructed from them – all this with full consideration for what German scholars used to call their *Sitz im Leben*.

It is natural, and very convenient, to commence research into translational behaviour by **focusing 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. the translational replacements of SL metaphors) or from the target text's vantage point (e.g. TL binomials of near-synonyms in their use 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, one should take care not to get stuck in the blind alley of the 'paradigmatic' aspects of one's study, which would at best yield lists of discrete norms (or 'normemes'). Rather, **one should aspire to proceed to a 'syntagmatic' phase**, involving the integration of normemes pertaining to different 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 individual findings and weighing them against each other. Obviously, the thicker the web 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 once again be noted that a translator's behaviour should not 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 a translation assignment within a single problem area. **Consistency in translational behaviour is thus a graded notion** which is neither nil (totally erratic) nor 1 (fully regular). Rather than being presupposed, its extent in any particular case should emerge *at the end of a study*, as one of its conclusions.

A few decades ago, 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 10) makes it possible to make a gradual distinction between norms in terms of *intensity*

(indicated by the height of the curve, i.e., the distance from the horizontal axis), the total range of *tolerated behaviour* (that part of the behavioural dimension approved by the group), and the ratio between these two properties of the norm.



**Figure 10.** 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.)

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

- 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 behavioural latitude.
- 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 the 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 (that part which lies above the horizontal axis), and therefore of minimal intensity.

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

- c'. **Symptomatic devices.** Though these devices may be infrequently used, their occurrence is typical to given narrow segments of the group under study. On the other hand, their complete absence 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 lot; it is always a subgroup of one that was constituted by norms of a higher rank. True, even idiosyncrasies (which, in their extreme, are shared by a group-of-one) often manifest themselves as personal ways of realizing (more) general attitudes rather than deviations in a completely unexpected direction.<sup>9</sup> Be that as it may, the reconstruction of norms is always relative to the sector 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, owing to human limitations, will always be applied to samples only, even though

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9. See the example of the idiosyncratic use of Hebrew *ki-xen* as a translational replacement of English 'well' in a period when the most common linguistic behaviour was the use of *u-vexen* (Chapter 6, Section 3). This is better seen as a personal variation of the dominant norm rather than a different norm altogether.

developments in computing would no doubt enable us to use many more and much bigger samples). As far as I can say, it would probably always be necessary to be content with our intuitions, which, being based on previous knowledge and experience, allow us to make at least 'educated' guesses. This does not mean that we should forever abandon hope for greater methodological rigour. On the contrary: much energy should still be directed toward the crystallization of systematic research methods, including quantitative 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, as I have already said, would only be probabilistic anyway. To be sure, descriptions and tentative explanations achieved in actual studies can themselves supply us with clues for necessary and possible methodological improvements. Besides, if we delay research until the most systematic methods have been found, we might never get any research done at all. For a proponent of DTS, limited research is certainly better than no research at all!

## Constituting a method for Descriptive Studies

Having prepared the grounds for a target-oriented approach to translation, let us zoom in now on the suggested method and the discovery procedures it entails and elaborate on its basic notions, mentioned briefly in the course of the skeletal presentation of the method in Chapter 1.

### 1. Assumed translations and their acceptability

When an investigation starts from within a target culture, what lends itself first to observation, although by no means entirely directly, is found on the *textual* level. It is a text or a body of texts – or some phenomenon extracted from them – which can be tackled on the assumption that it is translational. It is therefore **assumed translations** which constitute the most obvious object for study within DTS.

As already noted, there may be a variety of grounds for regarding a TL utterance – written or spoken – as a translation into it. A text may be explicitly *presented* as one, or it may be produced in a situation where translations are habitually *expected* in the culture under study. A text may just as well *exhibit features* which have come to be associated with translations (or, more narrowly, translations from particular cultures/languages): words and phrases, grammatical features, syntactic structures, etc. Knowledge of the existence of a particular text in another language/culture, which a TL text is taken to represent as its translation, may also serve as a trigger for activating the ‘assumed translation’ assumption. This last option is of paramount heuristic importance for cultures, or historical periods, where translating is known to exist but the existence of its products is concealed;<sup>1</sup> this may mean only that the presentation of a text as being derived is

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1. I am grateful to José Lambert for this invaluable observation which he made many years ago. See also the use made of this notion in Tahir Gürçağlar 2010.

not customary, or that the very distinction between ‘translations’ and ‘non-translations’ is non-operational, sometimes so much as blurred.<sup>2</sup>

Unlike the first two options, starting off with a textual entity which has no standing in the target culture would seem at odds with the recommended order of discovery procedures, if not with the very target-orientedness of the approach we are trying to endorse. In actual fact it is not. After all, the position of a text as a source has a meaning only inasmuch as there is at least one text in the (target) culture being studied which can be paired with it. Thus, whereas a TL text can be approached and studied as a translation even in the (temporary, or even permanent) **absence** of a corresponding SL text, a study applied to a non-target text alone can never claim to constitute a study in translation. At best, it would be possible to probe the **translatability** of these texts, that is, the initial potential of them remaining invariant under transformation vis-à-vis the language/culture one is interested in checking their translatability into.

Once a TL text has thus been earmarked, research can go on in the foreseen order. An important methodological consequence is that **recourse to the assumed source texts themselves can easily – and justifiably – be postponed until the point where the study can no longer proceed without them**; that is, when passing on from collecting *data* and making *discoveries* to supplying *explanations*. After all, there will always be the possibility that the assumed translation under study will be found not to have been derived from a particular assumed source text after all, or not from it alone.

All in all, whatever the reasons for marking a text as a translation, it is advisable to start by studying **assumed translations in terms of their ACCEPTABILITY in the system(s) of the target culture of which they purportedly form part**. The main justification for tackling acceptability first is *methodical* rather than theoretical. In principle, questions of acceptability can be addressed at any point in the study. It is only that once an ‘alien’ entity has been brought into the picture, these questions will start getting blurred – especially in cases where the text’s acceptability as a **translation** and the conditions underlying it do not fully concur with its acceptability as a **TL text** in general; that is, when the norms governing translations differ from those that govern non-translations. When the source text is brought into the picture, it may well prove difficult to re-adopt a ‘naive’ stance towards the translation and approach it as a text in its own right again rather than

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

a reflection of another text. Elements of the assumed source text will find themselves dragged into the account, pretending to supply explanations before any real data have been collected and questions asked.

The most important point here is that studies of assumed translations as texts constitute an integral part of the study of translation and of nothing else; this is true both in cases where the principles embodied in the texts are found to concur with those manifested by 'native' texts and in cases where differences are observed. Most significant is the case where the differences found show regularities which are attributable to the texts' belonging to distinct (sub)systems. At a later stage of the study, it may become possible to attribute some of those findings to the assumed source text and expose formal correspondence to it. However, since such relationships can often be tentatively extracted from the assumed translation itself, with no need to bring in any external source of data, on the mere assumption that it has indeed been translated, they may be found to be pertinent in the case of *pseudotranslations* as well and charge them with high significance for Translation Studies (see Excursus A). Tentatively established relationships may also give rise to the assumption that a TL text drew on a text in a language other than the one initially assumed, or on more than one source text, in more than one language.

## 2. Levels of comparative study

It should have become clear by now that the most elementary stage of a target-oriented study into translation already amounts to more than a mere collection of data. Even though this is its main objective, this stage also offers several possibilities of **comparison**, which are likely to add a dimension to the account of the texts' acceptability.<sup>3</sup> Even though no assumed source text would be *physically* present in any of these initial comparisons, its *posited* existence (and that of a set of relationships linking the assumed target and source texts together) will nevertheless hover in the background, giving rise to various **hypotheses** of an **explanatory** nature. At the same time, at this stage, any such hypothesis will be merely tentative, and it would still need to be checked and verified.

The simplest type of comparative study would involve a number of **parallel translations into one TL**, which came into being at **one point in time** (that is, at the same point in the evolution of TL and its culture). Such a comparison is

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



the easiest to justify as well as perform, because it involves the smallest number of variables. Inasmuch as each text has been properly contextualized in the host culture (e.g., a drama translation ‘for the page’ or ‘for the stage’, a translation for adults vs. children, a translation for religious vs. non-religious usage), the analysis is bound to shed light on the correlations, if not dependencies, between values, systemic positions and functions, on the one hand, and surface (textual-linguistic) realization on the other.

A good example of such a study is Tiina Puurtinen’s 1989 MA dissertation, precisely because of its low pretensions: 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 with a view to their relative *readability*, which was taken to constitute one aspect of their overall acceptability as books for Finnish children. To the extent that a translation’s readability is influenced by the strategies adopted by the translator, it is intimately connected with the relationships between the target and source texts. However, in itself it characterizes the translation *as a TL text*. Moreover, in the present case, differences of linguistic choice which have determined the relative readability of the two parallel translations were found to have had very little to do with features of the source text, but rather with choices made by the two translators within the norm system of the target culture/language and the options it offered vis-à-vis translating the source text they have in common. (See Puurtinen 1989a, 1989b.)

Much more common are **parallel translations into one TL**, which were made at **different points in time**. Owing to their greater proliferation, the comparison of these parallel translations has always been more common among translation scholars, notwithstanding the fact that this kind of comparison constitutes a much more complex task than one would be inclined to think. Thus, if the study is to have any real significance, at least the notion of (one) language applied to the TL would have to be rethought, in view of the fact that languages undergo constant changes and do not really remain the same with the passage of time. Thus, a text translated into a certain TL at different times would not really have been translated into one and the same language: even if the norms haven’t changed much, the textual-linguistic wrapping may well have undergone substantial changes. The need for such a modification is all the more urgent the longer the intervals between the translations are, or the quicker the changes in TL and its culture.

In the years 1955, 1973 and 1988, i.e., at almost identical intervals, three different Hebrew translations of Hemingway's short story "The Killers" were published in Israel. In linguistic terms, each Hebrew text is different, which is anything but surprising, the more so as every translator may well have been aware of the earlier version(s). In fact, it would be tempting to regard some of the decisions made by later translators as indications of so-called 'polemical translation' ("an intentional translation in which the translator's operations are directed against another translator's operations"; Popovič 1976:21).

Groups of students at Tel Aviv University, amounting to hundreds of subjects over a period of several years, were asked to read the three Hebrew texts, knowing – or, at least, being able to guess – that they were all translations, probably of the same original. However, no text in any language but Hebrew was given to them. Also, they were not asked any particular questions about the text. Rather, they were given one simple task; namely, to put the three versions in what they thought their correct *chronological order* was – and to justify their decision.

As it turned out, everybody – including complete newcomers to thinking about translation, experienced translators, teachers of translation and budding scholars – came up with exactly the same order. Moreover, with very few local disagreements, the justifications they gave for their ordering were all based on the same series of features: basically an assortment of semantic, grammatical, syntactic, pragmatic and stylistic markers, as well as tentatively reconstructed translation relationships, which the students seem to have associated with 'typical behaviour' of literary translators into Hebrew and the way it changed over the years.

Now, the significant thing is that, in spite of a full agreement between so many different subjects who have taken part in this pseudo-experiment, they were all wrong: the order they came up with – which was based on their intuitive and/or learned ability to identify relevant markers and associate them with modes of translation (and, yes, the norms which governed them) – that order did not conform to reality.

When the names of the translators were divulged, there were a number of students who were able to correct their initial ordering. The names thus served them as an added contextualizing information. In the most extreme cases, they had some cultural knowledge (based e.g. on book reviews they might have read) as to who was more or less likely to count as 'dated', 'mainstream' or 'avant-garde' (in the terms they were thinking in, maybe just 'good' or 'bad') in fulfilling their role as translators.

Finally, once the subjects were made aware of their error, it was relatively easy to explain to them how, *historically* speaking (that is, in terms of the appropriate norms and their position in society rather than on mere chronological

grounds), they had actually not been all that wrong, after all. As it turns out, only one of the three translations, version (A), was truly representative of the time it was produced in, and hence of the expectations its intended readers had. The other two versions were either ahead of their time (B) or somewhat lagging behind (C); two kinds of deviation from mainstream norms which were automatically ‘corrected’ by all readers.

The bottom line is very clear: not only are there norms associated with translation, but persons-in-the-culture know how to, and actually do, activate them, not only when they are *producing* translations, but also when they act as *consumers*. And all this (and more) could be said without taking a single look at any assumed source text. Needless to say, when the time comes to bring in a source text, it is bound to add a lot to our understanding of the case. (An anecdotal finding, which is however not uninteresting: on the basis of a number of translation-specific lexical items [for which see Chapter 13], I was able to determine that translator (A) made consistent use of a particular dictionary.)

Then again, one may wish to compare **different phases of the emergence of a single translation**, using interim stops in the emergence of the TL text to trace the way individual translators waver between different conceptions of both acceptability and adequacy, and especially the changing balance (compromise) between the two extremes, as they move along. Chapter 11 will be devoted to the specific possibilities of, and problems involved in, studies of this kind, and Chapter 12 will present in some detail an account of the successive revisions made by a translator in one textual portion, a Hebrew translation of Hamlet’s monologue “To be or not to be”. Among other things, it will be shown again how instructive the temporary suspension of the (assumed) source text may be.

In this last case, an aspiration to approximate the target text to the original does not seem to have affected the translator’s subsequent decisions: he hardly looked for a closer fitting of his text to the SL text. Rather, his concern was more, and increasingly, with the positioning of the Hebrew text in a particular niche of the *recipient* culture and the way to achieve this. So marked was his tendency towards acceptability in contemporary Israeli theatre that the very question of what text(s) he used as his immediate source emerges as marginal. Intriguing as it may be, this is one case where the conclusive identification of the source text would change practically nothing in our understanding of the Hebrew text, or of the process whereby it came into being. There are, of course, acts of translation where measures *are* taken to bring the emerging translation closer to adequacy. In such cases, postponing the introduction of the source text into the study, which is the procedure I have been recommending all along, may 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 distinguishing between what is culture- or language-specific in translation and what is more general, maybe even universal. Differences of language and cultural tradition are of course much more difficult to handle than a mere difference within a single tradition. Because of the great number, and great variety, of variables in the multilingual case, what is bound to be compared here is not really the various texts themselves, or any segments thereof, but rather the *findings of a number of analyses* carried out separately. One interesting aspect here is the decision about which language(s) would serve for the comparison itself: SL, TL<sub>1</sub>, TL<sub>2</sub>, ... NL (i.e., a language that is neither SL nor any of the TLs).

### 3. Coming up with the appropriate source text

What the study of the acceptability of translations involves is basically intrasystemic relationships, i.e., the relations between (assumed) translations and other members of the host systems, as well as these systems as organized wholes. This is a *theoretical* justification for why no need has arisen so far for a text which would be taken as the original. However, as we have argued, the culturally determined notion of translation does entail 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, it is clear that one would first have to determine what the source text might have been, and to do so properly.

Of course, in many cases, the problematics of this task is neutralized by the straightforward nature of the data (e.g., there is just one candidate for the role of source text to begin with). However, when devising a research method, provisions should be made for any kind of possible complication that may arise; and there are indeed several cases where a **multitude of candidates for a source text may exist**. In such cases, any attempt to justify a researcher's selection of a source text would depend, at least in part, on what the assumed translation itself has exhibited, which would render the establishment of the appropriateness of a source text part of the study itself rather than an auxiliary move. In each of these cases, the reasons why the text actually picked to serve as a source text was deemed preferable to others constitute an interesting issue as such. Uncovering these reasons may even have important implications for the overall account of the relationships between function, process and product, e.g., on the level of preliminary norms.

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 preparatory work, which one of them might have served as source, if it was indeed a single text and not a combination of several? This is a real issue, and not only for remote periods in the past. Two literary examples of the 20th century would be *Lady Chatterley's Lover*, which D. H. Lawrence wrote three times and John Fowles' *The Magus* (1966 and 1977).

Then again, there are cases where candidates for being identified as source text appear **in more than one language**. Of course, not only may any translation have proceeded from any one of these texts, but a *compilative* source text<sup>4</sup> is always a possibility too, a situation which may, in turn, be realized in various ways. Again, 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 of multiple sources 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 compilation of the two? Surely nobody could tell, unless the target text itself has been regarded from that standpoint, or, better still, unless some tentative comparative analysis has been carried out. Needless to say, it won't do to pick up for comparison just *any* one of the available versions, much less 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'.<sup>5</sup>

Another group of assumed source texts in a multiplicity of 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 departure point for a second, third, fourth, etc.) act of translation, in spite of its derived nature. In such cases, it is the mediating version that should be compared to the target text which is found to have proceeded from it. Picking up the first link

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4. Popovič (1978:20) defined 'compilative translation' as "a translation realized upon the basis of several translations". In my opinion, it is better described as "a translation which makes use of a number of source texts".

5. 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.: "[b]ecause 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 speculating on.

of the chain just because of its primeness may well lead the investigator astray, the more so if the mediating text in its role as a translation (which of course precedes its role as a source text) was directed by norms that were operative in the target-turned-mediating culture, rendering it a highly acceptable but not so adequate text.

Very often, and for a number of different reasons, the producers of second-hand translations (sometimes the translators themselves, sometimes the publishers) may have failed to make this fact known, especially in cultural contexts where translation is regarded as just one of the ways of generating TL texts, or else where prevailing norms disfavour indirect translation, regarding their use as a sign of weakness. However, assumed translations often betray the fact and literally oblige the conscientious researcher to track down the mediating version and use it as the source. The appropriateness of the source text would thus have been settled in the course of the study again, as one of its very first results.

Let me illustrate the points made so far, this time on the basis of subtitles in the way they are actually used in films and TV programmes:

In cultures which resort to this practice, subtitles appearing on the screen are normally assumed to have been translated. (Occasionally, they may be 'translations' from a spoken variety to a written one within the same language.) Very often they also *function* as translations, for which purpose they may be read differently from texts which are not regarded as translations. The reason is not only the 'technical' fact that, in such a case, one always receives two parallel texts and that the two are in two different languages. It also has much to do with conventions concerning the cultural status of printed lines appearing on the screen in certain positions and in a certain way (which does vary between cultures). These lines are taken to be more or less *simultaneous* with the spoken version (although they may not be), as well as to actually *represent* it, an assumption which is held by the average person-in-the-culture even if s/he cannot understand one of the versions or both: people may still talk about the 'translation' of a film and assign certain features to it, correctly or not.

In pre-systematic situations (that is, with a normal audience rather than researchers, teachers or other professionals) it is the *spoken* version that would normally be taken as the source text, and viewers who are in a position to do so will often find themselves comparing the subtitles to the spoken text as they watch the film. However, this practice cannot be taken as a premise for an *investigation* of subtitles as translations. After all, at least in principle, there are more candidates for a source text: e.g., a written script in the language used for the spoken version, a previous text that the script itself drew upon, a translation of that text into either the language used for the subtitles or some other language, a translated script, and, of course, a combination of some (or all) of these alternatives.

There is really nothing that could be taken for granted, and if the study is to lead anywhere, the identity of the source text(s) will have to be established for each case anew (even though previous knowledge can be very helpful as background for new studies).<sup>6</sup>

Interestingly enough, the possible multiplicity of candidates for a source text has no bearing on the status of the subtitles themselves as assumed translations, which (as usual) 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 appropriate source text will underlie all subsequent investigation of 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 when on 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 full-fledged study, whether the subtitles in all the languages draw 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 serving as a source text for that act?

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 pre-existing 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.

#### 4. (Observed) solutions and (reconstructed) problems

It would have been nice, even though quite difficult to handle, had it been possible to establish translation relationships between textual wholes only. As it is, such relations are much more discernible between textual *segments*, very often

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6. 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 TV 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”.

small-scale, rather low-level linguistic items. Mapping each assumed translation onto its assumed source (in this order!) would thus result in assigning the status of translation solutions to various constituents of the target text, which would so far have been considered, rather vaguely, as ‘translational phenomena’. Owing to this procedure, which yields a series of (ad hoc) coupled pairs of replacing + replaced segments, a target-text solution would not merely *imply* the existence of a corresponding problem in the source text. Rather, for the purposes of the study, **the two should be conceived of as determining each other** in a mutual way.

As argued in Chapter 2, the kind of problems which are most relevant to retrospective studies of translation are therefore *reconstructed* rather than given: like the appropriateness of a particular SL text as a source text, they have to be established in the course of an act of comparative analysis rather than within the confines of the source text alone, or even this text vis-à-vis the TL in question and the initial translatability of that text into it. Consequently, what is identified as a problem for one pair of texts will not necessarily emerge as a problem at all, much less a problem of the same level or magnitude, within another comparative study, even if that study merely involves a different translation of the same source text.

Implied by the last assertion 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 been subjected to solution attempts within the translation act in question**; and this status can only be established through a concurrent identification of the respective solutions. As argued above, even if all potential difficulties established in a thorough analysis of a (source) text indeed occur in an act of translation, 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 their revisions. Problem items of this kind would go completely unnoticed, unless they are established ‘in reverse’.<sup>7</sup>

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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).



A major issue concerns the **boundaries of the coupled pair**: 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, either 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 or addition.

Even though the solution suggested here may well have *theoretical* implications, it is offered first and foremost as a *methodical* way out of a real dilemma. Thus, the analyst will go about

**establishing segments of the target text for which it would be possible to claim that – beyond their 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*. The coupled pair of replacing + replaced segments will be made operative in the next chapter. At this point, let us have a 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 author 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 concede, a very central or complex one. Thus, the status of the unparenthesized portion of the quote as a ‘problem’, let alone the nature of that ‘problem’, would only be established in relation to a particular translation. Here is the parallel text portion in its first translation into Hebrew (here in 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 like wax.

(Luboshitsky 1898:9)

The Hebrew lines, in turn, simultaneously emerge as the ‘solution’ of that ad hoc problem, i.e., as the replacing segment of the coupled pair. Even though it could be done, there is hardly any internal reason for breaking down the two segments any further, in relation to each other. Although possible, this would simply add nothing to our understanding of the translator’s problem-solving and decision-making in this particular text segment.

As it turns out, the main thing this particular translator found problematic stemmed from the confrontation of **two contending models of cooking chicken, pertinent to two different cultures**, the German and the (East European) Jewish ones: the intended target culture is intent on preparing several dishes from a single chicken (to these very days, the result is known as “laundered chicken”), whereas the source culture prefers one rich dish, which, in the target culture, might have been regarded as a waste of good meat. There is no sign of such a problem for instance in the following English translation by Charles T. Brooks:

Through the chimney now, with pleasure,  
 They behold the tempting treasure.  
 Headless, in the pan there, lying,  
 Hissing, browning, steaming, frying.

(Busch 1962:13)

Of course, the mere *existence* of an intercultural incompatibility does not enable us to predict any translator’s way of solving that problem (in our Hebrew example, a triumph of the *target* model). The norms expressed by the source text could well have been preferred even in the Hebrew case, at the expense of the target text’s cultural acceptability, or else some ad hoc compromise between the two contending norms could have been struck where some price would have been paid in terms of both acceptability and fullness of reconstruction. The point is that while analysing each one of these cases in retrospect, the coupled pair, and hence its respective members, would be established *in its own terms*, and all this may well be unique to the couple under study.<sup>8</sup>

Thus, the mode of preparing chicken for eating constitutes no ‘problem’ in 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**

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8. One thing that reinforces this account is the presence of a number of similar cases in the text which could be given the same kind of explanation.

**has actually been elevated to the level of ‘problem’, which was then addressed in a particular translation act.** The nature of this phenomenon as an ad hoc relational issue 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)

Does the difference between the two Hebrew versions imply that Jewish cooking norms have undergone a major change in the forty years that have elapsed? This may of course have happened, in which case the initial incompatibility itself would have diminished. Be that as it may, one thing has definitely changed: namely, the translational norms. They caused the later translation to make a move away from cultural acceptability towards greater adequacy.

It is very clear that in the second Hebrew example, the unparenthesized portion (which, to be sure, is not 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 emerge 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.<sup>9</sup>

The nature of comparative analysis of all kinds deserves some more comments, by way of a brief reminder.

1. Comparisons are always *partial* only: they are not really performed on the objects as such, only certain aspects thereof.
2. Comparisons are also *indirect* by the very nature of the act: they 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 is to be performed.

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9. Dear reader! If you hypothesized that the original illustrations were not reproduced in the first Hebrew translation and were reproduced in the second one (or if you at least wondered about a possible contradiction between the text and the illustrations), add a point to your account. You are on your way to working within DTS!

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, a pair which may well require continual changes and revisions.

Once established, the members of a coupled pair can be compared with each other in greater detail. However, after a large number of pairs have been studied in isolation, it is time to start looking for some *regular patterns* which may be manifested in these pairs, either in all of them or at least in subgroups.

### 5. Prospective vs. retrospective stances exemplified by metaphor

Trying to devise a research method which would be answerable to Translation Studies as a discipline in itself, we have deliberately refrained from importing wholesale models and methods from other disciplines and imposing them on translational phenomena with little or no modification. Before going on to do 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, which has been hinted at many times in the first third of this book. 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 approach to translation.

The nature of metaphor as a translation problem has normally been established at the *source* pole, proceeding from source-text items identified as metaphors (= PROBLEM<sub>1</sub>). In the simplest of cases (e.g. Dagut 1976; 1978:91–120; Newmark 1981:84–96) this was done on purely *linguistic* grounds, and in more complex ones (e.g. van den Broeck 1981) on both textual and linguistic grounds. Very often, SL metaphors were given replacements taken from assumed translations which were then analysed to show how 'good' (or 'bad') those replacements were, in terms of some *preconceived balance* between the features of the original metaphor, mainly meaning, constituents and (type and extent of) metaphoricity. If a given 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 regular translation behaviour under a particular set of circumstances.

Needless to say, generalizations, and especially guidelines for future translation behaviour, hardly ever reflected the system in the madness, i.e., the regularities which could have been established for an entire corpus. Rather, they were filtered through an a priori concept of what would count as a 'better' (or 'worse')

TL replacement, which, even though based on just another normative attitude, nevertheless allowed the researcher to claim better knowledge than the translators whose behaviour s/he was purportedly trying to understand. 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 supplied the data. Only rarely has the focus been on the solutions *as they really are*, with no a priori 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 TL 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:<sup>10</sup>

- (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). It is only that writers intent on the rights of the source text refuse to grant 'zero' replacements *legitimacy* as translation solutions, or else they do so only in the case of 'unimportant' metaphors, or metaphors used in 'unimportant' texts, in both cases in source-oriented terms, of course.<sup>11</sup>

10. (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 to our present argument.

11. 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). It is clear that Koller's conception of a 'solution' is different from mine.

Now, even when proceeding from the *source text*, there can be absolutely no guarantee that the mere existence of a metaphor in a text would ensure its translational treatment *as one unit*, be its replacement metaphorical ((1)–(2)), non-metaphorical (3), or zero (4). This is just another issue which would be illuminated by the use of the coupling method. 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 reconstructed processes). To those who are interested in metaphors and nothing but metaphors, the additional x’s (representing the material that is annexed to the basic metaphor) may seem superfluous. What they are vital for is any genuine attempt to reconstruct translation decisions: one simply cannot take it for granted that, just because a metaphor is present in a text undergoing translation, it would invariably be approached and treated as an integral unit. In terms of the method suggested here, this is a clear example of the application of the ‘no leftover’ principle (either way).

When observed from the target text, the four basic pairs ((1)–(4)) find an immediate supplement in two inverted alternatives where the notion of ‘metaphor’ appears in the *target* rather than the source pole; i.e., 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 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 make some predictions as to how translators would tend to behave in ‘metaphoric’ situations 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 real 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 rather than the mere balance achieved by the translated text. However, 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 the activity of a compensation mechanism. The two practices may well have operated independently of one another, reflecting two unconnected sets of considerations. The point

is, however, that compensation would have been practically impossible to detect if the first four translational options were the only ones recognized by the methodological framework.<sup>12</sup>

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 translated text is reduced, even 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 two languages involved: source-oriented approaches have rather naively regarded these as the only possible causes for shifts (or the only ones worthy of treatment?) – along with weaknesses of individual translators, 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 rarity, not to mention the complete absence of *added* metaphors, and weakened in direct proportion to their abundance. Moreover, it may well be possible to establish some correlations between metaphor translation 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 as metaphors, we cannot simply proceed on the opposite assumption either, namely, that each type of phenomenon elicits a completely different treatment.

## 6. Uncovering the underlying concept of translation

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

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12. In response to my suggestion in the 1985 version of this 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.)

Notwithstanding the enormous problems involved in the establishment of such shifts, both practical and conceptual, there is no doubt that the notion itself is valid: all translations do involve shifts, and therefore this notion is bound to have a place in all branches of Translation Studies. However, it is my conviction that too much emphasis has come to be placed on this notion, not least by myself. This conviction draws primarily on the totally **negative kind of reasoning** involved in 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 can be defined a priori as a goal and guideline and adopted as a yardstick for quality assessment. This posited criterion of success may or may not be relevant to any particular instance of translation, which may well have an idea of its own of what would constitute as a success.

This includes my own old-time hobbyhorse, the hypothetical construct of the ‘adequate translation’ and its suggested use as the ‘invariable’ of a comparative study (Touy 1980a: 112–121). I am more and more convinced now that any real understanding of what translation is (rather than what it ‘succeeds’ in being or ‘fails’ to be) would be achieved by uncovering those principles which are relevant to a particular case, which would mean 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 Kitty van Leuven-Zwart’s intricate method of translation analysis (1989, 1990), it should never be regarded as an end in itself. Like the establishment of TRANSLATION RELATIONSHIPS, with which it is intimately connected, the identification of shifts is part of the discovery procedures, i.e., at best a step towards the formulation of explanatory hypotheses. The latter, in turn, necessitate the establishment of the overall CONCEPT OF TRANSLATION underlying whatever sector one sets out to investigate: one text within a broader context, one problem-area across texts, or a body of texts selected according to whatever principle.

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, with its long tradition of preoccupation with issues of ‘equivalence’ vs. ‘formal correspondence’. What it must still do, however, is rid itself of vestiges of the *prescriptive* bias, which should be left to the applied extensions of the discipline.

In establishing translation relationships, which involve 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, what the members of a particular coupled pair actually share is never given. It should be accounted for in an unbiased way.

In principle, this invariant may be found to reside 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 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, the comparison may be fake.

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 e.g. 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 won’t do to settle for a mere *enumeration* of the types of relationship encountered, not even with the addition of an explicit reference to the level(s) on which each type occurs. Rather, a *hierarchical ordering* should 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 weighed against each other, should finally lead to the establishment of the *hierarchy of relationships* pertinent to the text as a whole (that is, the highest-order entity).

It is here that the DTS notion of TRANSLATION EQUIVALENCE makes its entry, even though it is quite different from notions of equivalence in other paradigms of Translation Studies. Thus, as it is used here, equivalence is not a particular target-source relationship which is established on the basis of a recurring type of invariant. Rather, it is a *functional-relational* concept: namely,

**that set of relationships which are found to distinguish appropriate from inappropriate modes of translation 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, any relationship may, on occasion, assume such a role.

The entire set of possible relationships would therefore be taken to constitute *potential* equivalence, thus belonging to the theoretical branch of the discipline. By contrast, 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 exists between an assumed translation and a text assumed to be its source. What remains to be uncovered is only the way this postulate has been realized in each case, e.g., in terms of the balance between what has been kept invariant and what has undergone transformation.<sup>13</sup>

Equivalence, too, is of little importance in itself. In fact, there is a point in establishing what it is 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 this. What we are referring to here is an *idealized* process, as presented, for instance, by Jiří Levý (1967), and not to any real-life decision-making performed 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.

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13. Obviously, what belongs to the applied extensions of Translation Studies is thus a subset of the entire set of potential equivalences, a subset that functions as a *requirement*, on one basis or another. See Table 1 in Part One above and Part Four below.



## CHAPTER 6

# The coupled pair of replacing + replaced segments

Much as one would wish to regard the text as the ultimate unit of analysis, the mapping of an assumed translation onto its assumed source is impracticable unless both texts are broken down to smaller, and lower-level entities. This necessity is not devoid of theoretical justification. After all, an act of translation is inconceivable without *serial* operations which presuppose the decomposition of the texts. By contrast, the claim that the serial operations are also accompanied, let alone governed, by some mental ‘map’ of the two integral texts (as Holmes 1978 has it) is no more than a *possibility*.

For such a possibility to be realized, the minimum requirement is the translator’s acquaintance with the original as a textual whole prior to the commencement of the act itself, which is precisely what no descriptive-explanatory study can, or indeed should, proceed from. As is so well known, real-life situations may well involve incomplete acquaintance with the source text, and not only in the obvious case of simultaneous interpreting, where the original itself is coming into full being (i.e., is being articulated) as the interpreter is already busy producing its translation.

The restrictive requirement that only acts (and texts) which do entail full acquaintance with the source text and their products be submitted to study is simply untenable. For one thing, there is no way of giving a theoretical justification to this decision inasmuch as it is real-life behaviour which is at stake rather than any ideal(ized) version of it. For another, one can never be sure what process has yielded a given text, and hence which might be the ‘appropriate’ texts to be selected for study. In other words, whether a particular act of translation entailed thorough knowledge of the original is just another *question*. An intriguing question, no doubt, but certainly no starting point of any standing.<sup>1</sup>

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1. The issue of having full acquaintance with the source text prior to the beginning of its translation has been taken up in greater detail and in a more polemical tone in Toury 2010: 155–160.

Finally, even if it does occur, the realization of this possibility tends to be *partial*, at best: that is, a gradual, possibly momentarily establishment of a set of fractional ‘maps’ as a concomitant of the inescapable linear progression through the text. Thus, while the textual option should definitely be provided for by translation theory, the notion of its all-pervasiveness is rather shaky.

### 1. The need for a unit of comparative analysis

What would normally be mapped onto each other, then, are (contextualized) segments of an assumed translation onto (contextualized) segments of the text assumed to be its source. But what would be the logic underlying these segments and their establishment? And what kind(s) of segments would such a logic supply as units of comparative analysis? The crucial requirement here seems to be that whatever units are chosen would be relevant to the operations which would then be performed on them; in our case, this means 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 relevance, which are incompatible by their very nature even if their external manifestations are found to concur in certain concrete cases.

The first, and by far most common notion of relevance 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 ad hoc network of relationships constituted by the original that determines its relevance to that text, including any would-be translation thereof, irrespective of anything else. However, as already argued, it cannot, and hence should not be assumed that a unit enjoying prospective relevance will have been activated in any single act of translation. It simply cannot be taken for granted that whenever a feature occurs in an SL text, be its position ever so high in the latter’s internal hierarchy, this feature will be picked by translators and retained in the translation. The crux of this point is that the establishment of units for a comparative analysis which focuses on **reconstructing** rather than **implementing** translational decision-making cannot proceed on the sole basis of the position and role, let alone on the mere presence of features in SL-texts.

What makes the researcher’s task even more difficult still is the fact that, for any **retrospective** kind of study, no unit can be postulated for either text – at least as soon as more is ventured than the mere identification of shifts vis-à-vis the ‘adequate’ reconstruction of a source text in a TL. Units which are sure to be

relevant for 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 assumed SL counterpart. Hence the importance of the second notion of relevance: retrospective. 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’ segments, respectively.**

There is no need for the members of such a pair to be of the same rank or scope, which would sneak in an undesired prerequisite again. The pairing is subject to a *heuristic* principle instead, namely that

**beyond the boundaries of a target segment no leftovers of the ‘solution’ to a certain ‘problem,’ posed by a corresponding segment of the SL text, will be present.**

Established as they are in the course of the comparison itself, the coupled pairs will be submitted to further analysis as the study proceeds, and it is the relationships found to obtain between the respective members of a pair which would underlie any generalization made about the kind of translation equivalence which is pertinent to it.

## 2. An exemplary analysis of a pair of texts

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

- |     |  |     |   |
|-----|--|-----|---|
| (1) | <b>Alarm Signal</b><br>to stop train<br>pull handle<br>penalty £50<br>for improper use | (2) | <b>Emergency brake</b><br>pull brake only in<br>case of emergency<br>Any misuse will<br>be punished |
|-----|--|-----|---|

Version (1) was used in the 1990s on British trains, and version (2) on former West German trains. Incidentally (but very significantly, from a cultural point of view), whereas British trains carried only English notices, in the German context the warning was presented in four languages next to each other: German, English,

French and Italian. Although even in Germany there are persons who can read all four languages, it is safe to assume that each version was intended for a different audience. Example (3) shows what the German version looked like.

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

(1) and (3) are two independent texts in two different languages. Owing to their habitual use in the two respective cultural contexts, they may be regarded as *codified* entities: while never ceasing to be texts (i.e., being made out of a number of shorter, lower-rank constituents), they also constitute elements of the institutionalized repertoires pertaining to two different ‘train cultures’. And since the two are used in comparable situations, they would normally be taken as functionally parallel too, and hence as interchangeable in 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 on German trains as (1) plays for all passengers on British trains. Those in the UK context who cannot read English may find themselves totally excluded from the act of communication where the notice serves as a message, although I daresay they would not be exempt from punishment if they do pull the emergency brake: authorities tend to assume that the presence of a message testifies to the existence of communication. In both situations, not all readers will have English as their main language; in the UK those who do would form a majority, whereas in Germany they would normally constitute a minority (which may well have been growing with time).

Incidentally, there is a third kind of parallelism here, namely, the one holding between the two neighbouring notices, (2) and (3). It is this kind of parallelism which has so far been the most typical candidate for a study in translation proper.<sup>2</sup>

When the decision to fill the English slot on German trains was first made, I don’t know how many decades ago, the *Deutsche Bundesbahn* (or was it still pre-war *Deutsche Reichsbahn*?) could well have turned to a functionally parallel

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2. 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.

version in English, whether British English or any other variety of the language.<sup>3</sup> A single letter, even a phone call, would have sufficed. Such an imported version could have been adopted verbatim, or with minor modifications, most notably the omission of the exact penalty for improper use, which has not been specified in the German context. Even if it were mentioned, it would certainly not have been quoted in pounds sterling.

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 notices, 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 the particular act of translation which is now under reconstruction. The conclusion would have been that the source text itself had not been broken down during the act, so that replacement was indeed performed on the level of the textual repertoire: a habitual entity for another habitual entity of the same rank. It is not that no lower-rank coupled pairs could 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 be irrelevant for the mode of transition from one text to the other (i.e., the reconstructed translation process): they would have reflected the mere fact that similar (but not identical!) verbal formulations have been 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* have recourse to any ready-made English notice. Rather, as we learn from our comparative analysis, the German version (3) was selected wholesale. It was then submitted to an act of bona fide translation involving decomposition, a series of transfer operations and recomposition, ultimately resulting in (2). When we activate the Source-Text Postulate, (3) acquires the status of a source text: not only did it precede (2) in time, but it also served as a textual-linguistic basis for deriving it.

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3. Here are a few additional versions in English:

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 – R 10,00 (South Africa; literal translation of an Afrikaans version).

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



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 similar 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 ‘wholesale’ one? This time no answer can be ventured unless the English text (2) is mapped onto the German text (3), which is taken as its assumed source text. When 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 the application of this procedure would be a series of lower-rank coupled pairs, which would then be submitted to further comparative analysis.

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

It is clear that the formal decision that each of the four versions appearing side by side should take up five lines, one for the title and four for the body of the text, acted as a powerful constraint on the formulation of the English notice.<sup>4</sup> However, apart from the two titles, there emerges no reason to establish ‘line + line’ as a coupled pair: ‘Pull brake only in + *Griff nur bei*’? This clearly won’t do; the target-language word ‘pull’ would be an obvious leftover. ‘Case of emergency + *Gefahr ziehen*’? Hardly; this time the SL verb *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 case the two principles coincide, which is not always the case, and certainly not of necessity.)

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 already directed the establishment of the coupled pairs as wholes, this condition can be applied once more: namely, to parts of them.

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4. And compare the English versions cited in note 3, where a smaller number of lines (2–3) was used. Consequently, the segmentation was very different too, which is hardly surprising.

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 its establishment, the German text (3), which is both statistically and culturally the strongest of the four versions, was picked and decomposed into lower-rank constituents in descending order: sentences, phrases, words, and down to single morphemes. Each segment was replaced *separately*, in its own turn (the order of the elements was kept intact), so that the end product emerged not only *gradually*, but in a *sequential* manner as well.

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 SL word, *Mißbrauch*, alone, much less on the grounds of their *pragmatic interchangeability* in the train context (where ‘improper use’ seems to be a closer realization of the norm).<sup>5</sup> A strong hypothesis would be 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);
- 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 TL lexicon – the one stored in the translator’s ‘bilingual brain’ or in an actual dictionary – for its appropriateness and meaningfulness as an English noun.<sup>6</sup>

An almost identical 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 *linguistic* terms they are perfectly acceptable, none of them manifests a high frequency of occurrence in the

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5. 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 doubly premodified ‘deliberate improper use’ would probably have been deemed *stylistically* inferior.

6. The use of ‘misuse’ on South African trains (see note 3) 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.

*communicative situation*, or even in 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 a 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.

Moreover, the fact that one explanation can be offered to so many segments of a text which is so short and simple may itself be taken as strong support of the validity of the hypothesis (i.e., its explanatory power). Thus, the norm of equivalence pertinent to the pair of notices, and hence the overall concept of translation which may be said to have underlain it, are both found to have been determined on purely *linguistic* grounds.

Admittedly, in translation 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 give rise to truly translation-specific lexical items (see Chapter 13). Here, however, a positive reply to the question of *linguistic* appropriateness can be taken to have enhanced the insertion of the resulting segments into the English text which was gradually coming into being. Moreover, the possibility cannot be excluded that the sequence of partial decisions of this recurrent type was followed by a question as to the well-formedness and acceptability of the entire end product as an utterance in the English language. 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 version (2) of the warning was adopted as a fill-in for the ‘English’ slot on German trains – a decision which, moreover, has not been reversed in decades. By contrast, no question seems to have been asked as to the normality of this text in the particular context in question.

The obvious question which manifests itself now, with respect to the reconstructed process of translation, is **why**: why did it all happen the way it did? After all, nothing would seem more straightforward than contacting British Rail, or any other company or individual in an English-speaking country, and asking for the warning in actual use on local trains. This question gains in weight in view of the fact that the production of the English text which is used on German trains followed a completely different path from the establishment of the other two parallel versions, the French and the Italian, which were indeed taken wholesale from the two respective repertoires.

At least part of the answer seems to have to do with the difference of **status** between the three non-domestic 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. (Many German trains physically cross into France, Belgium and Italy.) In all likelihood, it was defined in **negative** terms; i.e., as the language of a kind of a (rather large) *residual group*, membership in which is determined by not having any of the other three languages, at least not as a dominant language.<sup>7</sup> To be sure, some of the members (passengers) may still have English as their mother tongue, even though not necessarily English of one and the same brand. However, a large number would not have a very good knowledge of English and might only resort to it as the least possible evil, as it were.

This is the obvious ground 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 some English, for as long as they are in Germany (or at least on board a German train). Some of them may respond to the notice with a smile, but they will no doubt understand it.

The validity of this 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 in use on *Italian* trains (4), which differs from all authentic English versions as well as from the ‘German’ English one.

- (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

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7. 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 German and the two other languages, but not in English.

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, in full analogy to the ‘German’ English text (2).

### 3. Justifying the use of the coupled pair

Let me elaborate a little more on the kind of justification that can be offered for the use of coupled pairs of mutually determining ‘replacing’ and ‘replaced’ segments during a comparative analysis of an [assumed] translation vs. its [assumed] source, 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. Moreover, as claimed above, this unit is not just workable; it is also 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 considerable 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 the translator actually operated with *during the act* itself.

Incidentally, the suggestion that the pairing of TL and SL 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 refer first and foremost to Brian Harris, who made a first attempt to transfuse into the bloodstream of Translation Studies the concept of the bi-text (Harris 1988b: 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 SL and TL texts, or fragments thereof, 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 practising 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',<sup>8</sup> 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 assigning bi-text a kind of textual status.<sup>9</sup> After all, only textable 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 kind of hypothesis; namely, the phenomenon of stock-equivalents (better: stock replacements).

As is well known, translators often come up with the very same TL item to replace a recurring SL 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

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8. 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 (1988b:9) calls 'strictly translations' (in contradistinction to 'parallel texts' of other types).

9. "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 1988b:8-9. For Harris' use of 'meaning', in this connection, see note 8 above).

equivalence alone (and see note 8 again). In fact, quite a number of these automatic ‘responses’ are *idiosyncratic*, which they wouldn’t have been, had there been a reason for 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 binding, this possibility has no bearing on the initial phase we are dealing with now.

Let us take a look at an example from the main corpus of my own studies, translation into Hebrew in modern times.

After an analysis of hundreds of pages of prose fiction translated into Hebrew in the last seventy years or so, 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 nineteen thirties and forties, the most common replacement of ‘well’ was zero: normally, a Hebrew translation had neither a lexical nor any other representation of that item or its functions: no linguistic element, no change of word order, not even a punctuation mark. In the remaining cases, a multitude 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 originating in the Bible (two tokens only: Ecclesiastes 8: 10 and Esther 4: 16<sup>10</sup>). To the extent that *u-vexen* was used at all, it occurred mainly in rather formal styles, and it had a rather small area of pragmatic overlap with ‘well’.

In the fifties and sixties, the share of *u-vexen* among the translational replacements of ‘well’ grew steadily, irrespective of the latter’s function in the English source text, and by the seventies, the association of *u-vexen* and ‘well’ had become habitual. Some translators replaced almost 100% of the instances of ‘well’ by *u-vexen*. In translations from other languages (e.g. as a replacement of German *also*), this item occurred much more rarely, nor did it come to represent a recurring SL element. It was even rarer in original Hebrew writing, negligible in formal speech and hardly ever in used in informal circumstances. As a result, especially when massively resorted to within a single text, the word could be taken as an indicator of translation from English: an almost univocal marker of the occurrence of ‘well’ in a parallel position in the source text. The coupling of ‘*u-vexen* + well’, which started as an ad hoc association made by individual translators, has thus undergone *generalization* as a translation strategy.

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, for one reason or

10. For curiosity’s sake: in both cases, King James’ English version of the Bible has ‘*and so*’.

another, refrained from using this habitualized replacement and 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 would better be seen an idiosyncrasy *within* the boundaries of the norm rather than its breach, 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, nor was this idiosyncrasy adopted by anybody else. As a matter of fact, it seems to have totally disappeared since then.

In the nineteen eighties, the frequency of *u-vexen* as a stock replacement of 'well' went down again. This can be attributed to an overall change of norms in translating from English, although the role played by a number of scholars and critics, the present author included, in accelerating the downfall of this particular item should not be underestimated. At this time, it is still in wide use in one kind of translation: namely, film and TV subtitling, whose dominating norms are indeed lagging behind those affecting other types of written translation. Thus, it is not uncommon to see the word *u-vexen* appearing in isolation on the screen and remaining there for what seems like 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 (see e.g. Carlson 1984).

Phenomena of this kind are easy enough to trace on the word level. However, similar things may also be found on higher textual-linguistic levels, 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" (1988b:8). As far as human translators go, limitations of memory may well have a substantial role to play. These limitations are hardly ever taken into real account by translation scholars, who tend to regard the ideal translator as a 'superman' or ('wonderwoman') in this respect and others. In this sense, the capabilities of the computer may have brought some changes.

#### 4. Testing the coupling hypothesis in real time

It may seem, then, that the connection established between SL and TL 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 TL items tend to be called



up automatically to replace those SL segments which have come to be coupled with them in his/her mind.<sup>11</sup> Consequently, with experience, translation may well become more and more **proceduralized**, almost automatic. In fact, a (gradual) building-up of a **repertoire of coupled pairs**, on various levels and of varying scope, seems to be one thing that acquiring skill in translation involves – one possible component of Levý’s famous ‘minimax strategy’ (“that one of the possible solutions which promises a maximum of effect with a minimum of effort” [1967: 1179]). 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, it is not surprising that it has hardly been put to any deliberate test so far. This is not only because the hypothesis itself hasn’t 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 research, 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 Think-Aloud Protocols.

In spite of their inherent weaknesses, some of which will be pointed out in Chapter 14 (Section 3.1), Think-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 tend to be proceduralized. In those parts, TL 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.

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11. There is at least one interesting field whose understanding would benefit greatly from submission to study under this perspective. I have in mind the traditional practice of oral translation of the Hebrew Bible for the needs of Jewish congregations whose average member hardly knows any Hebrew and for which a local language of small diffusion (e.g. Neo-Aramaic dialects in Iraqi Kurdistan) is used. As attempts to write down such translations before they (and the languages as a whole) are dead (e.g., Rees 2008) show, there are hundreds of cases where bi-texts are used, on various levels, including whole verses, if not longer segments of text. These reservoirs are triggered by the need to translate [again] and activated whenever they are available. It therefore stands to reason that different translators will have different stocks of fixed pairs. On the other hand, there may well be partial overlaps between different reservoirs, not least because every translator of the Bible was trained for it, or at least listened to other translators before starting to translate himself, so that traditions must have formed, which started hundreds of years ago and which probably were in conflict.

Consider the following protocol extract, cited from Dechert and Sandrock (1986: 117). The focus here is on a number of small-scale automatic responses:

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

Unfortunately, within a psycholinguistic framework, comparisons have barely begun between the translation performance of novices and that of experienced translators (e.g. Séguinot 1989b; Lörscher 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 increases with experience in translating or whether there are other kinds of strategic short-cuts. 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. At least, to me it would.

It is clear that, for language learners, the number of coupled pairs is rather limited, owing 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" (Lörscher 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.



## CHAPTER 7

# An exemplary 'study in Descriptive Studies'

## Conjoint phrases as translational replacements

Before moving on to present an assortment of studies into translation, a second layer will now be added to the bare bones of our skeletal Rationale through a step-by-step unfolding of an exemplary case. The presentation will revolve around one type of linguistic phenomenon, **conjoint phrases of near-synonyms**, in their occurrence in assumed translations in one particular tradition. This phenomenon was selected for an extensive presentation because it proved to occur quite densely in my corpus. This would by no means render it equally significant for any other culture. Thus, the discussion is not offered as a test case in the full sense of the word, but first and foremost as an *exercise in methodology*, the main point being to bring the somewhat abstract generalizations made so far down to earth. The focus will be on the different **stages of the study** as they were outlined in Chapter 1 (especially Figures 6 and 7) and on the **points of transition from one stage to the next**, including the types of **questions** each one of them entails. Once again, the answers will be provisory only.

Texts assumed to be translations into Hebrew were first situated in the target system and analysed with conjoint phrases of near-synonyms in mind. At this stage, mainly questions of *significance* and *acceptability* were addressed. Next, individual translations were mapped onto their assumed sources, never losing sight of our phrases. In our terms, the point is that conjoint phrases were taken to constitute **the TL members of the coupled pairs**, to be tackled as a replacement of any source-text item which would be found to pair with them. *Shifts* and *translation relationships* are the key notions here. To complement and balance the picture, pairs having conjoint phrases as members of the *source* text were not ignored either. Some first- and second-level generalizations were then attempted, with prospects for further expansion of the corpus, not only beyond the individual translation (or corpus of translations) in one and the same culture, but also beyond the boundaries of that culture as a whole. Finally, some possible *implications* of a descriptive-explanatory study of this kind for conscious decision-making in subsequent translation work will also be touched upon.

### 1. The phrases' significance assured

Conjoint phrases of synonyms consist in two (occasionally more than two) synonymous or near-synonymous items of the same part of speech, combined to form a single functional unit.<sup>1</sup> 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 have a single object as its referent ( $x + y = x$ ) (Quirk et al. 1985: 955). In the most extreme cases, such a unit would seem to be almost 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 or translated, will be found to contain quite a number of phrases of this kind. Such pervasiveness is reason enough to warrant interest in this phenomenon, the more so as its use in *translated* texts manifests clear patterns of its own, making 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 universal characteristics of language, semantic and grammatical, respectively. Every language probably does have such phrases too. What would distinguish different languages and language varieties, including various periods in the history of one language, is mainly the *extent* to which this potential has been realized, including the ratio of *fixed* vs. *new* expressions among them, the exact *ways* this is realized (e.g., favoured parts of speech, preferred order of constituents, exact relationship between the members, etc.) and the *purposes* it serves; these are all norm-governed aspects, to be sure. Thus, although there is no rule which would make the use of conjoint phrases of (near-)synonyms mandatory in any particular context, this use may still be positively marked, which would encourage recourse to them.<sup>2</sup>

Old written Hebrew texts, from the Bible onwards, have shown a clear predilection for conjoint phrases of this kind. 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 relevance 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, Koskenniemi 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 underwent idiomatization. Because of 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 continuing recourse to conjoint phrases, first and foremost 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 resources to new modes of writing designed to satisfy new needs, which were imported from various European cultures; at first mainly German, then Russian (and Yiddish), and finally English. At that time, the technique of conjoining near-synonymous items was revived. Never quite abandoned, it now enjoyed high productivity again. As a result, the first generations of modern, 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 was there necessarily any local (textual or communicative) motivation for the use of many of the phrases, especially as, quite often, both constituents were almost identical in meaning – a meaning which was often rather 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 'true' Hebrew, which is precisely what was dictated by the ideological stance adopted by the writers: a need to realize new text-types, or new modes of writing, in old linguistic forms or imitations thereof.

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, 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 a lower density. Only seldom is a newly created phrase inserted into a text, let alone several such phrases, so that the *productivity* of the technique has almost been nullified. In fact, when this device is used now the result is often felt to be rather ridiculous, which was not the case in earlier times; otherwise this technique would hardly have survived as long as it did.

There was, however, an interim period of several decades when an abundance of fixed binomials, and especially newly created conjoint phrases, no longer constituted a norm at the centre of Hebrew writing, but they were still resorted to, quite abundantly, on 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).<sup>3</sup>

Further verification of this hypothesis will be supplied later on.

## 2. The use of binomials in translations

Even if one never proceeded to compare the conjoint phrases with their counterparts in the corresponding source texts, there is clearly 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 sometimes appear in such density as to border on parody (see forthcoming example). Not a deliberate one, one would 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.

Taken together, these facts offer evidence enough of the gradual formation of two alternative norms of acceptability (*vis-à-vis* 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, the density of binomials of near-synonyms and/or the occurrence of newly coined ones 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 associated with translations.

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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 rhetoric 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 of a text which made use of this device in full accord with the prevailing norm, but almost ad absurdum, a 1933 translation of Ludwig Lewisohn's 1931 novel *The Last Days of Shylock*, 25 conjoint phrases were found, two of them with three members each (the right-hand column gives the respective replaced entities):

Etsa ve-to'ana u-pitxon pe	pretext
Be-kol et u-be-kol ša'a	0
Le-ma'an hašpilenu ad afar. Le-anotenu	[to] smite [us]
Nehepxu ve-nihyu la-axerim	made them
Efes bitaxon ve-efes emun	not to be trusted or believed
Ru'ax betsura u-mesugeret	disciplined
Koved roš ve-hagut lev	thought
Be-zahav u-be-avanim tovot	In gold and precious stones
Et maxševotenu ve-et ma'arxe libenu	our minds and hearts
Taxbula yešana, segula beduka u-menusa	that hoary trick
segula beduka u-menusa	0
Nisa ve-ala ha-kol	rang [the resonance]
Le-yasdan u-le-titan	to found ... for
Menuxa ve-naxala	0
Ha-nidaxim ve-ha-nirdafim	dispersed and oppressed
ve-ha-munadim	and harried
Ha-munadim mi-goy el goy	harried [+ 0]
Azuv me-elohim ve-adam	in disgrace with fortune and with his own soul
li-šemad u-le-bošet	apostasy and shame
tamrurim va-xamat za'am	bitter
ha-tsar ha-tsorer	enemy
me-az mi-šekvar ha-yamim	0
tsamexa ... ve-hikta šoreš be-libo	entered into him
ba-yom ha-ra ve-ha-mar	on that evil day

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, maybe even centuries earlier.<sup>4</sup>

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'.



When conjoint phrases of near-synonyms encountered in a Hebrew translation are mapped onto their counterparts in the source text, 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.<sup>5</sup>

Thus, in Grossman's translation of Lewisohn's *Shylock* (pp. 60–62 vs. pp. 57–59 of the source text<sup>6</sup>), 12, i.e., almost 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]'), 'evil', and 4 (16%) were an addition to the text, often in a purely conjectural manner. Only 6 (24%) have SL conjoint phrases coupled with them. The cumulative effect of this device in this particular text thus seems indisputable even before shifts and translational relationships have started being explored, which is what we will do next.

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 instances – which do not deserve to be regarded as cases of compensation – can easily be written off as being due to the accidental unavailability of near-synonyms in certain semantic areas of the Hebrew lexicon, or else to the fact that the original expression is an idiom, whose overall meaning was sometimes preferred to its conjoined structure and to the individual meanings of its constitutive elements. (It is too early to start arranging strategies and norms in the order of their importance and influence.)

### 3. Shifts, relationships, first-level generalizations

Frequent recourse to conjoint phrases in a single 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 English source text also makes it possible to establish the *relationships*

5. And see, in this connection, the detailed account of a 1922 Hebrew version of a *Schlaraffenland* text (Chapter 10, especially Section 6).

6. The three pages referred to were selected at random, but they seem to be quite representative of the whole book – and of its translator.

*obtaining between the members of each pair* and look for 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 *redistribution of information*. This redistribution, which stems from the change of 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 organization.<sup>7</sup>

Another kind of common shift, this time stemming from the stylistic markedness of many of the binomials and of the technique as such, is **stylistic elevation**, which is used to characterize translation into Hebrew anyway. Clearly, stylistic elevation is not necessarily a norm in itself. Rather, it often proves to be a result of the activity of other norms such as the **unification of style** (Toury 1980a: 128); this is 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 I have been able to locate, 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):

7. I have found at least one contemporary case, the Hebrew translation of Heinrich Böll's *Ansichten eines Clowns* (1972), 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%. One reason for this attempt may well have been the fact that the basis for the translation fee in Israel is the physical length of the *translated* version. Not only does every letter count, then; it is literally counted! Another possible explanation, which doesn't however contradict the first one, is the translator's proven epigonism. Be that as it may, the Hebrew version certainly looks rather old and obsolete, even though the book is contemporary and the translation was done in 1971. Of course, these explanations are not relevant to the nineteen thirties.

mi-kol *kípot u-šfáyim* }  
 m(e)al kol gív(')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.]<sup>8</sup>

Because of the greater number of syllables in an average Hebrew word (ignoring the compound mechanism which is not relevant to this poem anyway), any attempt to translate a German metrical text into it while trying to stick to its prosodic traits entails an inescapable need to give up parts of the semantic-lexical substance.<sup>9</sup> 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:

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

8. 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.

9. It may be interesting to note that newer translations of the 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 keeping with another target-literature norm of the 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 syllables. At the same time, it makes for an altogether abnormal Hebrew text, even a lyrical poem.

The original poem thus has 38 syllables constituting 24 words (11 of 1 syllable, 12 of 2, 1 of 3), 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 (7 words of 1 syllable, 12 of 2, 6 of 3, 1 of 4): 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 a metrical reading, as indicated by the parenthesized elements in the transliteration of the Hebrew text.)

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 content at the same time. What he seemed to have no misgivings about was 'wasting' a substantial part of the limited textual space he had 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 else 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 follow the dominant normative guidelines and produce a **passable Hebrew poem for the 19th-century reader**. 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 may well have evoked *biblical parallelism*, which made them representative of the most prestigious kind of language. However, he overdid it, so to speak: the density of conjoint phrases in his version of the Goethe poem is considerably higher than what such a short 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 and his constant occupation with the Bible. (In a few years, in 1896, he was to produce a concordance of the Hebrew and Aramaic Old Testament, which became the most widespread and popular concordance for decades.)

In a small percentage of the examples included in my Hebrew corpus, which keeps growing all the time, 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 a

Hebrew text. The hendiadys explanation has indeed been 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 explanation is only seldom convincing, and it is certainly unthinkable in cases like Goethe's poem. All in all, the enormous diversity of the relationships found to obtain between Hebrew conjoint phrases and their counterparts in source texts in various languages, and especially their completely irregular distribution, inevitably bring us back to our previous hypothesis: the translator's aspiration for enhanced Hebraity.

What we would thus claim is that it was not a 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 itself and was designed to serve its needs. This kind of explanation is not 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

This leads us right to the possibility of combining the findings of various studies into a recurring translation 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 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 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 capabilities in the face of this sensed weakness. The few studies available 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. 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).<sup>10</sup>

Unfortunately, these and other studies were not carried out with the same purpose; moreover, they made use of diverse methodologies. Their findings are therefore not at all easy to compare, making it unclear whether our sweeping hypothesis would stand. The bottom line is that this hypothesis 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 descriptive studies 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.

Thus, in my own translation of Mark Twain's *Connecticut Yankee in King Arthur's Court* (1983), 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 at a time when the use of the device had become quite rare in Hebrew translations too. True enough, Twain used a host of English conjoint phrases himself as part of his own parodistically archaizing 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, with respect to 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.

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10. See also Sørensen 1960: 128–132.



## PART THREE

# Translation-in-context

## An assortment of case studies

The time has now come to present a number of case studies which were performed within a 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 the last two centuries. The accelerated pace in which changes occurred in this sector renders it most suitable precisely for tracing the dynamics of the concept of translation, which I have been insisting on incessantly. 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 8 and 9) and gradually narrowing their focus, first to the translation of single texts (Chapters 10–12) and finally to one recurring pattern of decision-making (Chapter 13). 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 14), as well as in the programmatic exposé of the gradual emergence of a translator within a socio-cultural set-up (Excursus C) – a domain which still awaits some dedicated research work.





## CHAPTER 8

# Between 'Golden Poems' and Shakespearean sonnets

### 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 as beginning in the mid-18th century. Needless to say, no work of his had yet been translated into that language, just as hardly anything else had been translated from English literature, let alone directly from an English text (see Chapter 9). By the time of the Enlightenment itself (as amply demonstrated by Dan Almagor in a comprehensive bibliographical survey [1975]),<sup>1</sup> 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, the position of those of his texts which made their way into Hebrew literature remained marginal and his impact on it virtually nil.

Moreover, inasmuch as Shakespeare's name was mentioned in Hebrew writings of the time, he was referred to almost exclusively as a *playwright*. His first translations into Hebrew too,<sup>2</sup> and for almost a whole century thereafter, were either monologues or other short passages from plays, almost exclusively tragedies (Almagor 1975:769–784), and the majority of them were presented – and most certainly read – as *poems*.

Thus, despite the fact that, in most European cultures, Shakespeare's Sonnet Cycle had 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 an important role and enjoyed a much more central position than any dramatic genre,

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1. I wish to thank Dr. Almagor for his assistance in various bibliographical matters. The bibliographical material can now be accessed in the internet: "Shakespeare – from Right to Left" <<http://library.osu.edu/sites/users/galron.1/shak/3072.php>>.

2. The first translations from Shakespeare's writings into Hebrew are listed in note 8 to Chapter 9 below.

Shakespeare's career as a sonneteer had hardly ever been a topic of discussion. While Shakespearean sonnets had long figured in literatures with which Hebrew literature had close contacts – most notably German, Russian and Polish – it had taken even longer for them to start being translated into Hebrew: the first known translation of a Shakespearean sonnet dates from as late as 1916, by which time Hebrew literature already had translations not only of dozens of poetic passages from Shakespearean plays but also five of his tragedies in their entirety: *Othello* (1874), *Romeo and Juliet* (1879), *Macbeth* (1883), *King Lear* (1899) and *Hamlet* (1900–1901). Moreover, even when the first Shakespearean sonnets arrived, the pace at which they joined the repertoire of translation-worthy texts was not only slow but also hesitant.

To anyone who wishes to proceed from the assumption that translated texts form an integral part of *recipient* cultures, the delayed arrival of a translation, especially of such central texts, would be taken as a 'meaningful void', an absence requiring explanation (Kálmán 1986: 117–118). 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 oriented<sup>3</sup> explanation for the fact that Shakespeare's sonnets were so late, slow and hesitant in arriving, even when compared to translations of his other works, is precisely that unlike drama (as well as other literary genres which also supplied texts for translation), the sonnet had a fairly extensive continuous tradition within the Hebrew system, and there was no real need to practise their production, let alone carve a niche for them in the target culture.

Sonnets in Hebrew first appeared in Italy around the year 1300, with Immanuel of Rome (c. 1260–c. 1330) – i.e., a whole generation before Petrarch (1304–1374). Thus, Hebrew literature actually pre-dated by far all literatures which were to adopt this Italian genre. However, Immanuel's was an isolated episode, and a true sonnet *tradition* in Hebrew, with characteristics of its own, had to await another century to emerge. The Hebrew sonnet, modelled as it was on the Italian example, then became, especially in the 17th–18th centuries, the most wide-spread genre in Hebrew poetry in Italy (see Landau 1970). Moreover, not only 'secular' Hebrew poems were written in the sonnet form, as was the case in the Italian tradition itself, but sacred texts as well, including liturgical hymns.

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3. Explanations such as 'the sonnets were unknown', or 'less known than the plays', or the like, are not necessarily wrong when they refer to specific *individuals*. However, their explanatory power on the *historical* level is rather small.

The sonnet was even accorded a Hebrew name, clear evidence of its appropriation by the culture: *šir zahav*. Literally, this expression seems to mean 'a golden poem', but what its establishment actually represents is a numerological manipulation known as *gematria*, whereby the sum of the numerical values assigned to each Hebrew letter of a word is calculated. In this case the word is *zahav*, (actually written *zhhb*) and the resulting number is fourteen – the fixed number of lines in a sonnet ( $z = 7, h = 5, b = 2$ ). At the same time, the selection of the word *zahav* ('gold') from among all the possible words which could have represented the value 14 (e.g., *dod* [lover], *ahuv* [beloved], *avaz* [goose]) testifies not merely to the *institutionalization* of the genre in Hebrew literature, but also to the *prestigious status* it has been allotted.

In its modified Italian version, the Hebrew sonnet enjoyed a relatively uninterrupted history. 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, in the poetry that developed out of it in Europe, and eventually in Eretz Yisrael (Palestine), on the one hand, and in the United States of America on the other. Thus, there was no 'void' with respect to this genre in Hebrew literature, and no need arose to compensate for a deficit. To put it in a way that would emphasize the important role of translation: there was simply no need to put any (literary and linguistic) tools to the test on the way to assimilating the sonnet into Hebrew literature.

This explanation gains in validity in view of a number of other cases of avoidance when it came to translating sonnets into Hebrew, which could be given the same analysis. For instance, of the fairly large number of sonnets written by Goethe, not a single one was translated until rather late, even though Goethe was among the poets most massively rendered into Hebrew (consult Lachover's bibliography of Goethe in Hebrew [1952–1953]). The same holds for many other authors whose poetry included sonnets. And as far as sonneteers *par excellence* are concerned, not least Petrarch himself, Hebrew translations of their works did not appear until much later.

## 2. 1916–1923: modified 'Golden Poems'

We are already well into the 20th century when the first two of Shakespeare's sonnets (nos. 18 and 60) appear in Hebrew translation. The exact year is 1916 and the period as a whole is one of mass Jewish immigration to the US, mainly from Eastern Europe, which, at that time, still functioned as a centre of Hebrew literature. An obvious result was that many Jews started acquiring English and were beginning to come into direct and more intense contact with Anglo-American culture.

The first translator of a Shakespearean sonnet, Israel Jacob Schwartz (1885–1971), was fully representative of that generation. Born in Kovno (Lithuania), he attended a *yeshiva* (higher religious school). In 1906, at the age of 21, he emigrated to the US, where he took a teaching job. He then became a writer and translator and joined the famous group of Yiddish modernist poets, *Di Yunge* [the young ones], founded in 1907 in New York (see Miller 2007). His literary work was mostly done in Yiddish, but he did try his hand in Hebrew as well, producing both original writings and translations, mostly from Hebrew into Yiddish.

Schwartz's first Shakespearean sonnets appeared in *Ha-toren*, a Hebrew periodical published in New York, which, in terms of 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 (see fn. 1), failed to notice them until after his comprehensive bibliographical survey was published in 1975. Seven years later, in 1923, the same Schwartz published another group of ten sonnets. This time they featured in the journal *Ha-ikufa*, one of the central organs of Hebrew literature, and therefore they must have attracted considerably greater attention. At the same time, they too hardly left a mark on Hebrew culture. As far as I can tell, none of them was ever reprinted or anthologized, nor did they become part of Hebrew literary heritage in any other way. They were simply there.

The second bunch of ten sonnets appeared as a group, under the inclusive – and somewhat 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 and presented randomly.<sup>4</sup> Even the original order itself was not even kept (66, 33, 73, 61, 71, 34, 32, 29, 27, 17). To be sure, those numbers do not appear anywhere, which might have given at least a hint at their being part of a Cycle, let alone their relative position in it. All this indicates a lack of grounds for assuming any Sonnet Cycle consciousness on the part of the translator (and/or the editors), or any awareness of the relative positions of the individual sonnets within the Cycle, whatever one may feel about the nature and significance of their sequencing in the original (which does constitute a problem in Shakespearean scholarship).

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

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4. It might be worthwhile to check whether these enjoyed a special status in the US, for instance in the school curriculum, which could have affected their selection.

more conspicuous in Hebrew than in English, since a gender distinction is *obligatory* in the second person singular – in the pronouns, in noun declensions, in prepositions and other particle declensions and in conjugations: in other words, there is no way to compose a conative utterance in the singular which would be gender-neutral. At the same time, the actual number of feminine markers often far exceeds what might have been classified as 'obligatory' in the transition from English to Hebrew. Thus, in a single translated sonnet as many as 16(!) such markers may occur (no. 18). As a result, eleven of Schwartz's sonnets have become love poems to a woman, in marked contrast to the original intention. This was hardly to recur after 1923, except in isolated cases, even though the "Sonnets to a Young Man" are still the more popular among the translators, and not as a mere reflection of their forming the majority of the original Cycle ( $126/154 = 82\%$ ).

Schwartz's behaviour in this respect is not difficult to explain, in light of the prevailing norms of the period. During the first third of the 20th century, much of Hebrew poetry, original and translated alike, 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 a similar 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.<sup>5</sup> However, in Hebrew, this type of compromise is not only much more striking than in any one of the traditions I know of, owing to the specific grammatical gap between the two languages, but it continues to prevail long after having been abandoned by members of other cultures, because of the belated start of modern, secular Hebrew literature and the specific course it has taken.

In this particular case, Schwartz's compromise can be characterized as involving *voluntary censorship*, taking into account possible sanctions by the target culture. However, the need to strike a compromise was not confined to this thematic level, where a global decision should be assumed to have been made which was then locally realized in dozens of instances in the texts themselves. As indicated more than once, 'compromise' is a key-word in any attempt to account for both translations and translation practices. In Schwartz's case, this compromise, as was still customary with literary translations into Hebrew, tends to lean much more

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5. 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, and even a particular English edition of Shakespeare.

heavily on *target* models and norms than to take upon itself the reconstruction of source-text features, even the most relevant ones from that text's point of view. In other words, acceptability is given precedence in most domains. The price in terms of adequacy is considerable, even though it is never really a 'total loss'. In sum, Schwartz's translational products are not close reproductions of the Shakespearean model, but they are no longer pure Golden Poems either.

Chief among the domains where Schwartz's peculiar compromise reveals itself is the *formal* one. The Shakespearean sonnet is known to differ widely from the Italian model, which had been at the base of the Hebrew sonnet down through the ages. Schwartz himself was raised on the Italian model in its Hebrew variety, until he first encountered Shakespeare's sonnets. As the first person to translate these texts into Hebrew, there was also the possibility of introducing a novelty into the Hebrew literary repertoire in the form of a deviant version of a genre. Then again, in principle, he could have opted for subjecting himself to whatever model prevailed in Hebrew literature itself. In view of the secondary status of English literature as a source for Hebrew translations at that time, a rather *conservative* approach might have been expected vis-à-vis the formal traits of the Shakespearean sonnet, which was indeed the case. At the same time, Schwartz did not confine himself entirely to the traditional option either, but chose a kind of a middle course. This decision – and a non-automatic act of decision-making was certainly involved – may be taken as an indication of the as yet undefined, if not insecure status which Shakespeare's sonnets must have had for Schwartz in his capacity as a *translator*.

As far as the **rhyme scheme** is concerned, for example, the Shakespearean sonnet follows the AB CDCD EFEF GG pattern; that is, it includes three quatrains, each one of them with a different alternate rhyme. The Hebrew (Italian-like) sonnet, in turn, 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 (12 texts x 14 lines), only 8 (4.76%), amounting to two quatrains out of 56 (3.57%), follow the Shakespearean rhyme scheme. All the rest abide by the scheme established on the Hebrew principles, 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 **metre**, Schwartz adopts not merely the *iamb*, but the iambic *pentameter*. Thus far, however, the Shakespearean model tallies with the tradition

established by the Hebrew Golden Poem, a tradition which had prevailed ever since Hebrew poetry first appropriated a tonic-syllabic metrical system (Hrushovski 1971a). In fact, while almost all Hebrew translators of Shakespeare's sonnets followed the iambic pattern, some of them expanded the original pentameter into a *hexameter*, mainly in order to partly compensate for Hebrew's inability to fit all of the lexico-semantic material of the English texts into a Procrustean bed, since the Hebrew word tends to be substantially longer in terms of the number of syllables. (See also Chapter 7, Section 3.)

While Schwartz did adopt the **pentametric** norm as a point of departure, the truth of the matter is that he too inserted hexametric lines into each one of his translations. In the most extreme cases, he went so far as to include as many as eight (nos. 17, 32, 71) or, in one case (no. 27), even ten 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 the pentameter as the basic metrical norm is no more than a *statistical* phenomenon. In fact, however, a six-foot verse for Schwartz is neither a basic nor even a secondary norm. It is simply 'tolerated' behaviour (see Chapter 4, Section 4), 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 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 7). As it turns out, Schwartz appears to have struck another 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**, 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 finally and penultimately 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 which were penultimately stressed in both pronunciations of Hebrew, the Ashkenazi and the Sephardic as it was used in Italy (Pagis 1973:660), thus failing to obey another norm in the domain of rhyme.

From the standpoint of the **language** of the translations, Schwartz is very close to the poetry of the Enlightenment, whose norms were still active in Hebrew



literature, especially in the translated sector, although they were gradually being relegated to a peripheral position and superseded by the poetic norms of the so-called Revival Period. His adherence to the Enlightenment norms may be accounted for, at least in part, by his place of residence, where he was cut off from the mainstream of Hebrew literature and the major course it had been taking. Schwartz's adherence to the older norms may have been further enhanced by the status assigned to the *original* texts as 'classics', although that stance was much more typical of the translational attitude towards Shakespeare's *tragedies*,<sup>6</sup> and, at any rate, epigonism was typical of most of the Hebrew writers and translators who lived and worked 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 guide and a major constraint. Clearly, they were not a result of confronting that formulation equipped with the full range of possibilities afforded by the Hebrew language. In fact, Schwartz approached those texts with a relatively *limited* repertoire of TL options, put at his disposal by the prevalent linguistic and rhetorical models of the time. It is mainly this repertoire, taken as a more or less closed list of potential translation 'solutions', that dictated his 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 Hebrew canonical sources**, mainly the Bible, and to string them together. In fact, quite often, the choice of one such fragment affected the manner in which the translation went on. The emerging text was, as it were, forced to follow through on it, rather than getting back to the wording of the original text and restarting from there.

One consequence is that one would be hard put to define the dominant **translation unit** – i.e., the original text segment with which the translator tended to work – since the role of the TL, 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 translation unit is fairly large**. In fact, if the 'no leftover' condition is adhered to, the unit pertinent to his behaviour as a translator is often found to **transcend the boundaries of the poetic line**.

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6. "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).

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 might have represented through the reflection of source-text features was tempered by habitual and established practices sufficient to ensure that the TL text as a whole did not deviate unduly from prevalent practices. 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 both original writing and 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, in 1929, one of the ten sonnets translated by Schwartz for *Ha-*tkufa** (no. 66) appeared in a second Hebrew translation. The principles ingrained in this new translation indicate the possibility of an alternative point of departure for translating the sonnets. 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 generation, the so-called Revival Period, from which it was to emerge victorious (see Z. Shavit 1982).<sup>7</sup> The translation itself is unsigned, but it has been attributed to Avraham Shlonsky (1900–1973), the main spokesman of Hebrew modernism and the founder of a new, and highly influential 'school' of literary translators. In all likelihood, just as other Shakespearean translations by Shlonsky were not made directly from the English, neither was this one, which is, in a sense, a small step backwards.

By and large, this translation is in keeping with the poetics of Hebrew modernism,<sup>8</sup> whose norms – in both original writing and translation – were then still on the periphery of Hebrew literature, struggling to push their way to the centre. Among its most conspicuous characteristics, the following deserve to be mentioned.

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7. The fact itself that a Shakespearean sonnet appeared in a militant periodical of modernism, and in a strategic position at that, in the centre of its front page, may seem unusual. However, the whole issue in question was an exception to the normal customs 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 from Jack London, Bernard Shaw, Upton Sinclair, John Ruskin, August Strindberg, Walt Whitman and John Bunyan).

8. 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, as it is not the text as such which is at stake here but rather the alternative translational option it suggests.

1. In complete 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 given to the closing couplet. One needs to count the lines in order to discover that the poem is a sonnet. Needless to add, no mention is made of the position 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. Also, 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 Shakespeare’s sonnets, done by members of the modernist school or its successors, whether the texts were published one by one or in small groups.<sup>9</sup>
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 total separation between poems whose rhyme scheme was based on final-stress metre and those whose rhyme scheme was based on penultimate-stress metre (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 fully representative of modernism. Actually, in this respect, the translation is more or less representative of the norms occupying the *centre* of the system at the time, those norms that came under the attack of modernism. In fact, as translators, Shlonsky and his contemporaries-cum-followers were rather slow and cautious in adopting the new ways, which again goes to testify to the *conservatory* power of translation.

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9. 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 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. (For another example of Avinoam’s translational activity see Chapter 7, Section 2.)

This isolated translation, then, represents a constellation of features which was in a position to supply an alternative point of departure for Hebrew translations of Shakespeare's sonnets. 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 a kind of a translation that was to gain canonization in one or two decades.

#### 4. 1941 onwards: leaving the Golden Poem behind

As 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 draw on more than mere quantity, especially when we bear in mind that when groups of these translations were published together, they retained neither their consecutiveness nor even their order. It appears that the complete lack of a 'cycle' notion in the Hebrew tradition of the sonnet is at least as much of a factor here: the only notion this tradition made use of was the fifteen-sonnet unit where the last sonnet is made up of the first lines of the previous fourteen, and even this notion was used quite scarcely. In the late nineteen 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 Hebrew version of the entire collection of Shakespeare's sonnets. However, the book version did not come into being *ex nihilo*. Thus, Shalom too began by publishing 16 of the "Dark Lady" Sonnets in two groups (nos. 127–134 and 137–144), each one in its original order and with explicit mention of their respective numbers, in the important periodical of art and literature *Gazit*. These were followed, two years later, by the entire "Dark Lady" Cycle in a special booklet of 22 pages (1941), and in another two years Shalom published the first 17 of the "Sonnets to a Young Man" in the *Moznayim* monthly for literature. 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 a long time; in a sense, until this very day. (It was reprinted with minor modifications in a slightly revised version in 1990.) When one of Shakespeare's sonnets was referred to in a secondary Hebrew source, it was almost always quoted in Shalom's version, even if there were already several versions to choose from (see Section 5 below). This translation also became the main contributor to anthologies of poems in existing translations to this very day. True, already in 1977 a second full translation of the Cycle was published, but the position reflected by this translation constituted a marked regression, especially with respect to the

kind of language favoured by the translator. At any rate, this newer version never superseded Shalom's translation.

The canonization of Shalom's version cannot be exhaustively explained by the mere fact that for some 35 years it constituted the only full translation of the sonnets. After all, it could easily have fallen into oblivion much earlier or never caught on at all. In order to explain this status, we also need to take into account the striking conformity of Shalom's translation to the norms dominating Hebrew literature at the time. Thus, while Shalom's version did represent a further move away from the old-time tradition of the 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 epicentre 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 Hebrew **neologisms**. 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 their profusion, would have been inconceivable in previous periods, although 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 legitimization. (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 source 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 system towards a growing concern for translation adequacy.

Another concomitant outcome was an overall **reduction of the translation unit**. This often shrunk to the size of one 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 into Hebrew. As the quantity and variety of translations from the 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 various 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 exclusive.

### 5. A mixed picture again

In the next decades, dozens of Hebrew occasional translations of sonnets appeared, either in isolation or in small groups, mostly in newspapers, periodicals, selections of translations and thematic anthologies. At first sight, this may seem a regression to the period when the fact that each sonnet was part of a Cycle was 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 identical to the situation when such a cycle does exist and when every new attempt at translating a sonnet can be, and often is, regarded in the light of an existing canon. This is all the more true considering that, from now on, most of the translators no longer suppressed the number of the corresponding source text in the Cycle and as a rule, used this number as the translation's sole title.

These scattered translations constitute a very mixed lot indeed. In essence, they reflect parts of different, and competing, sets of linguistic, literary and translational norms – old and new, central and peripheral. As a whole, they point to a state of *flux* between various norm systems, and a situation which is far from creating any clear picture. This is so even in the case of poet-translators whose original works (or other translations) represented the latest trends in Hebrew poetry, such as Meir Wieseltier or Menachem Ben (Braun), or academics, either with some background in Translation Studies (e.g. Shimon Sandbank) or without any (e.g. Ziva Shamir).

As already mentioned, another complete translation of the sonnets appeared in 1977, after parts of the Cycle had been published over several years in various periodicals. This translation was done by Editor Ephraim Broide (1912–1994). 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 Hebrew translation except among an ever-diminishing group of veterans (of whom Broide was 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 now are even more obsolete than those Shalom adopted three decades earlier, which may well be (part of) the explanation why Broide's sonnets never superseded Shalom's. Interestingly enough, the regression in sonnet translation is highly compatible with the changes that occurred in how Shakespeare *the playwright* was being translated. Thus, it is not surprising that the 'classical' norm has always prevailed in his translated *tragedies* (see note 6 above), and not so in his *comedies*.

The reviews of Broide's translation shed further light on the norms under which he worked, and especially on their status in the Hebrew culture of the late nineteen 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 classes, whose contours seem to be so well defined, often cite the same features, even the very same lines of the translations to prove and illustrate their contentions, which are, as we recall, opposed to each other.

## 6. One step backwards and two steps forward

Some eight years later, a third Hebrew version of the Sonnet Cycle appeared. It was made by a totally obscure person named Emmanuel Ginzburg, and was published by a small Jewish firm in Brooklyn, New York, Svet Publishers. By that time, Hebrew publishing in the US had almost ceased to exist and the number of American readers of the language was rapidly diminishing too, the growing numbers of Israeli immigrants notwithstanding. The little we know about Ginzburg comes from a brief self-characterization in the book itself (p. 176), and in view of the concept of translation manifested by the texts there is only one possible conclusion; namely, that even though the year of publication is nominally 1985, and in spite of the fact that the translator was a contemporary of both Shalom and Broide, this version represents a much earlier historical stage; almost a whole century earlier, in fact.

Ginzburg was born in 1907 in Baku (Azerbaijan) and grew up under the Soviet regime. He received his doctorate in technology and worked as an engineer in Moscow until 1980, when he was granted a permit to leave for the US. A rather old man by now, he settled down in New York. The book of sonnets itself (which followed the publication of two small books of Hebrew poetry) was very modest: a mimeographed typescript rather than a proper (i.e., typeset) book, and the sonnets are totally unpunctuated, a clear indication that attempts were made to keep production costs very low. In all likelihood, the print run was very small too

and the copies were probably intended mainly for family and friends. There was clearly no pretence to introduce the text into the blood stream of contemporary Hebrew culture.

The Sonnet Cycle itself is accompanied by four paratexts, three in verse and one in prose: Thanksgivings to the original author and his texts (p. 1), Dedication to the translator's wife (p. 2), Prologue (pp. 3–9) presenting Shakespeare's Sonnets as a secular 'Song of Songs' and hence of high significance to Jewish culture (a typical kind of text in the Enlightenment period), and an Epilogue (pp. 164–169). It is not only the number of these paratexts and the position expressed by them that mark the book as something of an anachronism, but also the language Ginzburg used.

The concept of translation as reflected by the sonnets themselves is no less old-fashioned. In places, Ginzburg's sonnets look even older than Schwarz's. At least in part, this can be attributed to the long period the translator had been out of touch with Hebrew (which he claims to have learnt as a child), not to mention the way it had been evolving throughout the 20th century and the literature that was produced in it. Finally, there is more than a hint (p. 165) that Ginzburg may have relied heavily on Samuil Marshak's Russian translation of the Sonnets. As we have seen, both the use of a mediating language for the translation of English texts and the use of Russian as a mediating language had become dated a long time earlier. The only 'modern' trait is perhaps the fact that the love poems addressed to the young man were left the way they were.

Copies of Ginzburg's book are very difficult to find even in big University libraries, and most of the potential readership, which lives in Israel, never saw the least trace of its existence.<sup>10</sup> It certainly left no mark on Hebrew literature, whose centre had moved to Israel, unlike the fourth full version of the sonnets done only seven years later by Shimon Sandbank (b. 1933), a professor of comparative literature at the Hebrew University in Jerusalem and a prolific translator from English and German.

Sandbank's career as a translator of Shakespeare's sonnets started in 1968 (sonnets no. 65 and 130), and in the next quarter of a century more and more translations of his were published in a variety of newspapers, periodicals and anthologies. The entire Cycle was finally published in book form in 1992.

Representing as it does not only one more step away from the Golden Poem, but also towards a Shakespearean-like sonnet in Hebrew, Sandbank's version may

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10. A search in WorldCat (<http://www.worldcat.org/>) has yielded 3 copies in British libraries and 19 in American ones. I wish to thank Dr. Aminadav Dykman of the Hebrew University in Jerusalem, who kindly sent me a scanned version of a copy which Ginzburg himself donated to the Library of Harvard University (Heb 44900.820) to commemorate his grandson's graduation. The dedication was signed (in both English and Hebrew) on 27.3.1987.



well come to mark a new beginning. Implied is a rather unified set of norms, which – unlike the ones adopted by Broide – may eventually replace the norms still marked by the canonization of Shalom's translation.<sup>11</sup> Thus, Sandbank chose to lean heavily on the linguistic experience accumulated by modern Hebrew poetry itself, 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 sonnet structure. On top of it, the kind of Hebrew he used is variegated and very flexible, not really a language which would have been labelled 'classical'. The result is more or less well-formed Hebrew poems through which major aspects of the originals can 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 had been publishing isolated translations for decades. Several of his translations appeared in more than one version – offering a unique opportunity of tracing the evolution of one translator's attitude towards the Shakespearean sonnet towards the end of the century. Several sonnets were reprinted after 1992, and these reprinted versions tell us a lot about the on-going crystallization of the norms adopted by Sandbank.

## 7. A glimpse into the future

Isolated sonnets as well as small groups of them are still published from time to time, with no clear backbone: in terms of the norms their translators subscribe to they are a very **heterogeneous** lot and most of them are accepted by some and rejected by others (normally, criticism is heavier than praise). The same holds for three additional full translations of the Cycle, published in 2000: one by a rather marginal figure, Ben-Tsiyon Ben-Moshe (b. 1944), another by the poet Arye Stav (b. 1939). In the very same year, there was also an illustrated selection of sonnets translated by Ariel Zinder (b. 1973; i.e., coming from a totally new generation!) under the title *From Shakespeare with Love*. Finally, a couple of months ago (2010), a further full translation was published, this time on the internet (by Ziva Shamir, a professor of Hebrew literature). All these versions, and others which will no doubt follow, will have to be left to others to deal with.

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11. In fact, not many months later, a revised edition of Broide's translation was also published. His revisions, however, were all made within the old paradigm, which means that they now look even older than they looked when they first saw the light of day. Consequently, they stand a slim chance indeed of constituting a true alternative to either Shalom's version or to Sandbank's, which is now struggling for priority.

## CHAPTER 9

# 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, the very recourse to indirect translation<sup>1</sup> is not devoid of significance. Unlike individual instances of translators turning to existing translations as their immediate sources, which may indeed indicate no more than simple linguistic inability, or even a sheer whim, the recurrence of this practice, especially if regular patterns can be detected, should be taken as evidence of the forces which have shaped the culture in question, including 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 an aberration, as has long been the prevailing attitude. What such an approach reflects is a fallacious projection of a current norm, ascribing uppermost value to the original text, onto the plane of theoretical premises. What mediated translation should be taken to be is a *syndromic* basis for descriptive-explanatory studies – a configuration of interrelated symptoms which should be laid bare if that kind of behaviour is to be understood rather than merely shunned. The way to unravel these symptoms and actually regard them as symptomatic of something involves a conscious attempt to supply convincing answers to a set of questions like the ones listed in Chapter 4, Section 2. In translating from what 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

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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.

von Stackelberg 1984). Rather, it presents a convenient means of moving from observable facts to their underlying motivations, precisely because its 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 practised with any amount of regularity can purport to ignore this fact and waive the need 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 to be tackled in isolation, but as a **junction** where systemic relationships and historically determined norms intersect and correlate.

In this chapter, second-hand translation will be addressed as it has been practised in Hebrew literature during the last 250 years. Special emphasis will be put on a symptomatic reversal of roles of two languages/cultures, German and English, as mediating and mediated agents, 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 inspection of historical processes which are normally rather diffuse.<sup>2</sup> The accelerated pace of its efforts to catch up with the modern world, along with its longstanding status as an epigone vis-à-vis various (and changing) cultures, render the contours of its development considerably sharper, and its phases easier to discern.

## 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 ‘rest of the world’ without investing major efforts in translating as a convenient means of trying their hand and tools on things already marked ‘literary’, and 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 difficulty of Hebrew literature, if not its inherent inability, to express with its own meagre, and

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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).

rather fossilized means everything that had been – let alone could have been – formulated in other languages.

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 alive 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 pretence. This strategy was adopted as far back as 1755–56, by one of the main figures of the *Haskala* movement, the philosopher Moses Mendelssohn, in *Kohelet Musar* (literally, ‘Preacher of Morals’), the very first periodical of the movement: of the two issues of *Kohelet Musar* 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 almost impossible to translate into Hebrew, that language could hardly be rivalled when it came to *literary* translation (in the sense that will be discussed in Excursus B). By harping relentlessly on the ability of Hebrew to do precisely what held so many difficulties in store, the proponents of this stance succeeded in creating an ideological climate which was highly favourable for translation. This, in turn, made it possible to pursue the programme 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, highly congruous move of far-reaching consequences: positing *acceptability* as a major constraint, to the almost complete forfeiture of translation adequacy – a kind of Hebraic *belle infidèle*, if you wish. This move, which no doubt contributed immensely to mitigating the problematics of literary translation into Hebrew in the fossilized shape it manifested at the time, might have drawn on the example set by the neighbouring literatures, although their own concept of translation had already undergone radical change by then. There were examples enough of ‘old-fashioned’ approaches to this activity and its results which were still around.

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 limited 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* (grammar and syntax) to be filled with pre-existing materials, but also as a reservoir of actual *forms*, to be used as more or less fixed expressions or directly imitated.

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 assigned to it – was regarded as unconditionally applicable to whatever the problem was thought to be. On top of all this, the preferable mode of usage was to remove rather long expressions 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. (And see what Lotman (1976) had to say about the implications of “recoding ... a text with a large alphabet into a text with a small one”.)

Literary translation into Hebrew during the Enlightenment period, 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* activities on most other planes, which were therefore highly resistant to change.<sup>3</sup> One result was a **marked blurring of the borderline between original writings and translations**; not, of course, in terms of the activity whereby translations came into being, which was well known, but certainly in *functional* terms, on account of the respective positions of the ensuing texts as well as the practices themselves in the target culture. Indeed, no necessity was felt to even mark a text as 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, were mentioned only in passing: in short formulas, in fine print, in parentheses

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

or a footnote, or in the table of contents alone. Not infrequently, even if the original author's name *was* given, the *text* or the *language* which served him was not specified. Finally, and probably most importantly, translations which were in fact *fragmentary* were not seldom presented as being complete, sometimes even being rounded off (e.g. have lines omitted from their end or lines added) to better achieve that end. All this amounts to strong evidence of the fact that the text and its features, textuality and completeness, were at best secondary, making any striving for adequacy almost impossible to realize.

## 2.2 The symptomatic status of indirect translation

As implied by the non-markedness of translations, translating was regarded as a convenient technique for introducing entities into a culture which was in desperate need of them, having had no real continuity for a long time. Under such circumstances, tolerance of indirect translation seems almost self-explanatory. After all, for the mere application of translation procedures on the way to the establishment of 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 felt gap) could well have been encountered by the *Haskala* proponents themselves in translated form to begin with (and let us not forget that many of them had no more than one or two languages at their disposal!).

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 culture it belonged to, i.e., the *mediating* culture, with no regard for the position its own original had in the initial source literature. In other words, what was *nominally* second-hand translation was *functionally* – that is, in terms of the organization of the 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 itself 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 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, mostly under the influence of neighbouring cultures, 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 this activity, can be viewed as a reflection 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.

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 when the history of translation and its role in culture as a whole are subjected to research.

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, have long defied 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 indeed is presented as one – establishing the fact that it is *mediated*, let alone the identification of the mediating language and text, is no less problematic. This is due mainly to the considerable uniformity of translations from different languages, in terms of their linguistic make-up (see Section 2.1). Among other things, owing to the overall dominance of a norm which yielded the establishment of correspondence between TL and SL segments at the phrase, clause, or even sentence level, very few clear signs of *negative transfer* can be detected in the Hebrew texts, which might have served not only as evidence of the fact of mediation itself, but also as a clue to the actual mediating language as well as identifying the text that was actually adopted as a source.

In many cases, the only practicable way out is to bring data about the individual figures involved in the act – translators, writers, editors, and the like – to bear on the texts themselves: where they lived, what kind of education they had, who their friends were, knowledge of what 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 intercorrespondences,

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 one combination or another of the ultimate original and translation(s) thereof.

### 2.3 German culture as a supplier

Since it was in Germany that the Hebrew Enlightenment made its debut, there can be 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 evolution**, and picked out items which had become more or less canonized. Indeed, many of the texts selected for translation were once located near the epicentre of the German system, but most of them had since been relegated to a more peripheral position.<sup>4</sup>

Superficially, this might have been described as simply ‘playing it safe’; that is, as a way of averting any doubts regarding the appropriateness of the models and norms realized in the texts, and hence likely to be accepted into the recipient culture. This might have become a crucial issue, had the replica-literature (to extend one of the terms used by Uriel Weinreich in his classic discussion of ‘Languages in Contact’ [1953]) 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 into Hebrew thus had to pass the test of **proven recognition 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 came

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4. A typical case in point would be Christian Fürchtegott Gellert’s role in the creation of the Hebrew Enlightenment fable. My hypothesis would be that one important factor was, in all likelihood, the fact that Gellert’s fables were included massively in later readers and anthologies which Jewish proponents of the *Haskala* used to teach themselves German and literature (see Toury 1993; 2000).



into being – 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 that literature 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 directly from a non-German original, the position of its German translation(s) in German culture was often crucial at least for its selection for translation.

#### 2.4 Translating English texts via German

One of the main literatures whose very presentation to the Hebrew reader 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 of previous ones, and had German not played such a decisive role in establishing modern *Hebrew* literature, English literature might 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 declaration that, when it came to translating literature, Hebrew could hardly be rivalled.

At first sight, the choice of text may seem rather odd, because the excerpt was not taken from a German work, as could have been expected. However, the choice was not so unlikely as it might appear. Even though the text selected for translation was indeed English in origin, its selection had very little to do with its Englishness (referred to implicitly 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 relationship which, in that period, 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 historical circumstances. For the Hebrew translation that featured in *Kohelet Musar* comprised the first 66 lines of *The Complaint, or Night Thoughts on Life, Death and Immortality* by Edward Young (1683–1765).

As is well known, the German scene in 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). Some German critics preferred Young to Milton, and Madame Klopstock thought the British king ought to make him Archbishop of Canterbury. 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 *Aufklärungszeit*.

Moreover, Mendelssohn is known to have shown keen interest in the problems of translating works by writers such as Young into German. Thus, at about the same time (1757), he published in an important German journal of literary criticism, *Bibliothek der schönen Wissenschaften und der freyen Künste*, 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 the principles 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 was at least closely consulted during the translation into Hebrew.<sup>5</sup>

And this is what really makes such an out-of-the-way translation so significant. After all, unlike many of the future translators, Mendelssohn could easily have based his Hebrew version directly on the English, 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 the 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. This would not remain an isolated incident in the history of 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, some literature in Hebrew was 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 there 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,

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5. These markers are listed and explained in a Hebrew article of mine entitled “The Beginning of Modern Translation into Hebrew: Another Look” (Touy 1998a).

Hebrew literature emanating from Britain was never anything but peripheral, and – unlike the literature produced in Germany, and then in Poland and Russia, which was distributed, read and reviewed in major parts of the Jewish world – 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 culture, 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, amounted to virtually nil. In fact, the only British-Hebrew translations that ever got anywhere near the centre of the system were those initiated by it to start with, most notably Eduard Salkinsohn's translations of Shakespeare's *Othello* (1874) and *Romeo and Juliet* (1879). Indeed, though nominally British, the Hebrew 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 Britain, 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 very few of them contributed to the shaping of its centre, or any of its sectors.<sup>6</sup> 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.<sup>7</sup>

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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. See e.g. Delabastita and D'hulst (1993); being a collection of articles on Shakespeare's translations in the Romantic Age, this book also includes invaluable references to many previous studies.

As shown by Almagor in his 1975 bibliographical survey (see Chapter 8), 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 Hebrew culture's 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 should 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. Owing 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 culture of the time 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, they were mostly published in rather marginal periodicals. Small wonder, then, that the great majority of the few translations which were 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 knowledge of only 200 translated lines in published sources, 101(!) of them drawn from three different translations of Hamlet's monologue "To be or not to be";<sup>8</sup> and every single one of these translations may well have

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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: the 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 Hamlet's monologue. Made by Naphtali Poper Krasensohn and published in *Kochbe Jizchak* ('Isaac'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 from *Hamlet*, embedded in a story by Peretz Smolenskin, including the "To be or not to be" monologue.

been mediated, mostly by German. In most cases, this suspicion is not too 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 translation into other languages), it was evidently less than successful – clear evidence of the peripherality of Shakespeare in 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 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 ever-changing literatures, new voids were constantly encountered, and, at the same time, a variety of 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 on the *linguistic* plane as well, which, in view of the new tasks, could no longer be regarded as adequate, not even by a stretch of ‘wishful thinking’. For instance, not being spoken, on the one hand, and lacking in appropriate literary traditions, on the other, Hebrew hardly had any semantically 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 number 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. As of the 1820s, Russian gradually became not only the closest available system, but also the most legitimately usable culture. There is therefore 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 10, Section 5). For this was a case not of mere 'influence', but of ever-growing *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. This Russian influence was stronger, and had much greater and longer-lasting effects, than German had had in the previous (and much shorter) period of the Enlightenment.

Consequently, the new paradigm which took shape in Hebrew literature, especially from 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*.<sup>9</sup> 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, formulated in keeping with 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 relative positions 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

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9. And see Even-Zohar's 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).

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 his German origin nor his being born a Jew. (In fact, his conversion to Christianity may well have acted as an *obstacle* to 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 poems about nature and love (Z. Shavit 1976), probably acted as a contributing factor, but this need too was prompted by Russian literature, mainly of earlier 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 10.) 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 that did find their way into it (including a great majority of Shakespeare's works, where a gradual increase in numbers finally occurred from the 1870s on) 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 centre of Hebrew literature to Eretz Yisrael (Palestine) and the physical abolition of Hebrew cultural 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 various sectors of light fiction (*ibid.*: 117–118).

The main contributing factors for that growth seem to have been the following:

- the British mandate over Palestine (1917–1948), which made English an official language and rendered it the first foreign language studied in its schools;
- the growing centrality of English vis-à-vis most other western cultures;
- the establishment of a secondary cultural centre in the United States, due to the 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 who emigrated to Palestine;
- and, finally, the changes which the concept of translation itself had been undergoing towards growing emphasis on adequacy, which inevitably encouraged the reduction of tolerance for indirect translation.

### 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 Russian. This was the ultimate result of the dominance of a Russo-Hebrew model, especially in its second phase of crystallization, effected in Palestine by writers who hardly had 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 become part of the domestic culture. A special kind of ‘Hebrew for Russian-like texts’ thus 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 (1899–1964), 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 devoid of any 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 Russo-Hebrew model, which had risen to native-like status, and newly introduced Anglo-American norms. This struggle was to be settled in favour of Anglo-American models. To the extent that indirect translation was still practised, English now became a main, if not the 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 came about.

Between the 1930s and the 1960s, translation of German literature into Hebrew 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 (Tourey 1977: 118–120). The ban was gradually lifted in the late 1960s, but direct contacts between the two literatures were hardly resumed, particularly when it came to the selection of texts for translation. In fact, it was only quite recently that a substantial number of German authors and texts were picked for translation, and for a substantial part of the new period texts in German were not translated before their English translations had gained fame and success in the English-speaking world, mainly in the US. To a certain extent, this remains valid today.<sup>10</sup>

10. For instance, towards the end of 2010, Hans Fallada's German novel *Jeder stirbt für sich allein* [Everybody dies alone], which was then 63 years old (having first appeared in 1947), was translated into Hebrew for the first time. The decision to translate it was clearly based on the success of its first English translation, made a few months earlier (in 2009). Nor was this dependence concealed in any way. On the contrary, the success of the English version was recruited in the promotion of the Israeli version. Moreover, the title of the Hebrew translation was a literal translation of the English one (*Alone in Berlin*), evidently as a means of enhancing and fixing the connection between the two. An earlier book by Fallada, *Kleiner Mann – was nun?*

Thus, the decision, in the early 1970s, to revise the translations of two of Hermann Hesse's novels, *Narziß und Goldmund* and *Peter Camenzind*, made (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).<sup>11</sup> 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.

Post-war German writers such as Günter Grass and Heinrich Böll were still introduced to the Hebrew reader following their success abroad, mainly in the US, and on the basis of this success. This trend only began changing in the eighties, a rather hesitant but highly significant change. A certain number of translations of German texts were also made via their English translations, even though mainly on the cultural periphery: namely, in children's literature and certain sections of popular writing.<sup>12</sup>

A few examples of mediated translations of children's books (in parentheses, the dates of the Hebrew translation): two of Lisa Tetzner's books (1956 and 1958); four books by Friedrich Feld (Rosenfeld) (1967–1968); *14 höllenschwarze Kisten* by Gina Ruck-Pauquët (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

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(1932), was translated into Hebrew (in 1987) on a local initiative. However, it did not enjoy much success and the amount of interest it aroused was very small. Interestingly enough, a new impression of that translation appeared on the market just a few weeks after *Alone in Berlin* first hit the best-seller list. In terms of the concept of translation reflected by the two books and the norms which directed their translation, the 1987 book is relatively old-fashioned, whereas the 2010 one is very much a contemporary translation.

11. 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".)

12. Several revisions of older translations, including the two Hesse revisions mentioned earlier, were also made with no recourse to the German texts, probably with some dependence on English mediating texts. 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.

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. When 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<sup>e</sup>l/ rather than /zim<sup>e</sup>l/. 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 might well have been a perfect mirror-image of the one which prevailed 200 years earlier. Paradoxically, because of 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.<sup>13</sup>

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

## Literary organization and translation strategies

### A text is sifted through a mediating model

We now move from an extensive survey of a relatively large domain to an intensive analysis of a single translation in its context. The present chapter will thus present, 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 children's literature. This case is offered as a typical if extreme example of the behaviour of one culture in one of its evolutionary phases vis-à-vis a single text imported from another language/literature.

On the face of it, confining a study to just one pair of texts represents a rather modest aspiration. The enterprise, however, is actually far more daring – and with far greater implications. In what follows, an attempt will be made to demonstrate, by introducing a constantly growing enveloping context, that:

- a. decisions made by an individual while s/he is translating a single text are far from erratic. Rather, even though by no means all-embracing, they tend to be highly patterned;
- b. the observed regularities of behaviour are attributable to some governing principles;
- c. the strongest of these principles originate in the target system, the one where – semiotically speaking – the translation event is initiated and whose needs it is designed to satisfy; and, finally,
- d. 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, the claim is that – even though seemingly always possible – any other translational strategy would have been way out of line, and hence much more surprising.

The text we will be handling is “Gan-Eden ha-taxton” [literally: “Lower Paradise”], a Hebrew translation of a version of a German fairy tale entitled “Das Schlaraffenland”. (Both texts are reproduced in the Appendix to this chapter.)

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 (1892–1930), the daughter of one of Sigmund Freud’s sisters (see Murken 1981), and published in Ludwigsburg in 1921.<sup>1</sup> Within a few months the entire collection was translated into Hebrew 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 lesser personage than Chaim Nahman Bialik (1873–1934), the so-called ‘national poet’ of modern Hebrew literature,<sup>2</sup> that lent the booklet whatever position it had in the recipient culture, rather than anything in or about the original. However, “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 hinder Weihnachten” [three miles behind Christmas] (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 Polívka 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 to an open-ended series of exaggerations and lies based on an inversion of reality.

In her interlingual 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 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.

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 particular 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.<sup>3</sup> The same holds for many of the individual omissions – items which were hardly admissible in a German text for such an audience at that point in time.<sup>4</sup> 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 source. After all, being pseudo-narrative in nature, it is the very *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

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

True, the added rhymes have not been made conspicuous, so that, to the cursory eye, "Gan-Eden ha-taxton" looks like all the rest of the tales. There can be no

3. For the effects of the existence of these 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. Here 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 rhymemes 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 2 5(6) | 3 0. Thus, even though the line pair is 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'.

doubt, however, that rhyming does constitute a central organizational principle of the Hebrew text, and hence a major constraint on its verbal formulation. There are in fact several cases 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" [one can lick and eat, which is a delight]. 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 as well as 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) licking<sup>6</sup> 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.

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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 shared this literary tradition 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 in the book; 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 an epic situation and tightening the 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 with global implications. One of the most salient among those 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<sup>7</sup> 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., it is clearly an epilogue. What this textual segment boils down to is the establishment of a *fictional narrator*; this is a major deviation indeed, this time not only from the immediate source text, but from its underlying model too which yields pseudo-narratives. Moreover, the (oral) narrator thus established does not make do with the explicit, and framing, announcement that he has 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, not only from the source text but 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” [listen, I would like to tell you about ...] – is immediately followed, and overshadowed, by the succession of lies 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:

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



*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-behakitim im ba-x(a)lom. iver ra'ahu ve-elav lo karav, kite'a ba bi-šarav, gidem patax dlatav, ve-ilem siper li zot bi-sfatav. mi xereš yasur halom, yešev ve-yišma ve-yakšiv dom.*

[Lower Paradise who knows? – 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 at night or in daytime, was I awake or dreaming. 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 account of 'Lower Paradise'. The latter now comprises hardly three quarters of the text, a crucial change indeed in its very 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 (*ich*), 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.<sup>8</sup> This ultimate ending is printed as a separate passage. It is also the only exception to the rhyme requirement. These are marks of special prominence, indeed! 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 actually being told.

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 and its producer simply have 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

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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.

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 11.

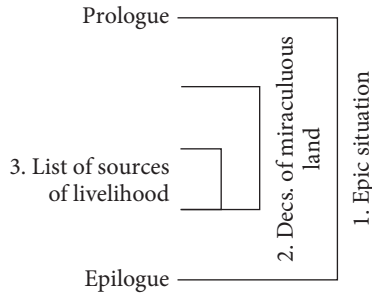


Figure 11. The hypotactic structure of “Gan-Eden ha-taxton”

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 bemila ve-ot be-ot* [word for word and letter for letter].

### 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 (a negative motivation for translational deviations). 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 more or less verbatim)? The approach he opted for is certainly at odds with Levý’s ‘minimax strategy’ (1967: 1179), which Bialik himself adopted much more closely with respect to the rest of Freud’s *Märchen*.

As 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, to which she alluded by mentioning Bechstein’s name on the cover. 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 the German model. At the same time, the text itself had to be transferred into Hebrew, if only because Bialik had committed himself to translating the book in its entirety.

However, there was a lot more to this necessity, given Bialik’s own 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 among them 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 [*Social Aspects of an Earthly Paradise*], the 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 backdrop of the use of the Hebrew expression “Gan-Eden ha-taxton”, mainly 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 according to 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 regarded as lacking it.

#### 4. A mediating model and its justification

It should have become clear by now that the structural changes introduced during the translation of “Das Schlaraffenland” were far from unstructured, let alone a mere accident. 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 admissibility into literature. However, a ‘positive’ question can also be raised here, namely, with respect to a possible regularity in the added and replacing elements themselves. Can any principle be shown to have governed their selection? To account for this question, let us have another look at the epic situation and its two constitutive parts.

As a compositional element, the added epilogue readily ties up with folk literature: it is there that narrators not only receive, but are expected to actually demand remuneration for their performance (Jason 1971: 87), and it is there that formulas for expressing this demand 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 kind of folklore 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 above) with a Russian prologue like the following one:

*Slepoj podglijadyvaet,*  
*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 seems to be much less institutionalized, and hence much less salient.<sup>9</sup>

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 Freud's adaptation):

*Wer sich also aufturn, 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 linguistic formulation of this sentence does not really conform to the Hebrew one, on top of which it does not cover *all* the expressions used in Bialik's 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. Finally, there are many additional regularities in the Hebrew text which have their counterparts in Russian traditions; (methodologically speaking) one global hypothesis which applies to many heterogeneous phenomena is to be preferred over a number of disparate explanations accounting for one detail each.

The hypothesized Russian source can be made more explicit still, a possibility which, of course, further increases the plausibility of our 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

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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).

'story' in between. This is how Roman Jakobson describes this variety in one of his numerous classic articles, "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*, a strong recommendation for Bialik to adopt it for his problematic text.

It now becomes possible to make a positive claim too: namely, 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 field, that of Russian folklore, but actually constitutes an alternative model taken from a recognizable source. In fact, "Gan-Eden ha-taxton" seems to satisfy the requirements of the Russian anecdote completely, in terms of both the jesting quality it possesses and the plot it lacks, except that it is written in Hebrew. 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 the organizational principles underlying the German text, the latter also triggered the adoption of 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 a 'first-hand' translation: the German source text was sifted, so to speak, through the model of the Russian anecdote, resulting in a 'Russian' text in the Hebrew language.

Obviously, my claim is not, and could never have been, that the Russian mediating model was any *better* than the one which underlay the original German text. 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 might have done just a few decades earlier. In fact, the Russian model

was 'legitimately usable'<sup>10</sup> 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;<sup>11</sup> norm-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, 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* sector of his literary production, and of Hebrew literature as a whole, that Bialik introduced this novelty – a clear demonstration of a central principle of systemic evolution which had already been formulated by the Russian Formalists. This was not an isolated case, even in Bialik's own literary career.<sup>12</sup>

This doesn't conclude our account, though. For 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 the years 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 bring this text-type to life again (see Govrin 1968).

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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 nineteen 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 (Tourey 1977: 138–140, 141–142 and 262, respectively).

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

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 in Hebrew literature itself may 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 it played a significant role in their evolution.<sup>13</sup>

## 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 matched up with the recipient body as much as possible. 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 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 its 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

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13. See, e.g., examples of internal 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.



enriched by a number of loan-translations from Yiddish which had been accepted into written Hebrew, including, of course, the phrase *Gan-Eden ha-taxton* [lower paradise] itself.<sup>14</sup>

Literal translations 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 – which was at the root of the 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 of the Hebrew language 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 anecdote compatible.

And here is where the most striking regularity occurs: the two diametrically opposed alternatives prove to be distributed throughout the Hebrew text 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 geographical space between the two rhyming countries *Mitsrayim* [Egypt] and *Aram Naharayim* [Mesopotamia], approximately representing the Land of Israel in its golden ages and thus suggesting a 'Promised Land' rather than the 'Land of Israel' in any kind of reality, acting as an interpretation of [Lower] Paradise. 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 from canonized sources – still forms about three quarters of the text and constitutes its core.

Needless to say, this massive recourse to fixed expressions 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 Hebraity of the translation (and/or yield a required rhyme-partner), and not in order to reconstruct or

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

represent any feature of the source text.<sup>15</sup> 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 in 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 the ones built around the pair of (near-)antonyms *lokek* and *rokek* (see supra) 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 Hebraity (see Chapter 7 above). Clearly, the need to establish parallelisms further increased the number of textual-linguistic additions as well as some other types of translational shifts.

Given 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 the period, even when one and the same person was responsible for both. It is also no wonder that this norm continued to be highly active in this sector 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 set Hebrew phrases

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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), Lebzelteln → [*mamtakim*] *maše 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 is due 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).

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 comprising *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 canonized by inclusion in Bialik’s collection of writings for children (1933: 170–174). There it featured as a *poem*, no less.<sup>16</sup> For that purpose, it was rearranged in short verses and in stanzas on the basis of the original rhyme scheme and division into paragraphs, respectively (see note 5). This collection as a whole 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 the ‘collective memory’ and became a historical rather than literary fact, until uncovered by Uriel Ofek in the early nineteen 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 the number one expert on Hebrew children’s literature, Uriel Ofek (Bialik 1983: 204–205). Moreover, a year later, the entire book *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 “Gan-Eden ha-taxton” was again given the status of a translation, if only to a select audience. 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 as a whole has become much more secondary in Hebrew literature, and only a very small proportion of this activity is now being done by Israeli writers, let alone by those of the first rank. Chances are that “Gan-Eden ha-taxton” as a translation will remain peripheral, a kind of neutral piece of knowledge held by the (very few) initiated, rather than becoming again an active member of a living literature.

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16. When Bialik included “Gan-Eden ha-taxton” among his poems for children, he further enhanced the subjected status of the inscription mentioned above (Section 2) 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.

## 7. Was there any alternative?

An intriguing question is whether Bialik had any other choice. Could he have opted for a different strategy and produced a very different kind of text?

As translation is a decision process by its very nature, the obvious answer would be ‘yes’. What is significant, however, is precisely that the advantage of this prerogative was not taken – and that, in retrospect, Bialik’s preferences turn out to be so easy to explain, 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 (i.e., equally accessible and legitimate), given the constraints imposed by the target culture. Rather, they tend to be hierarchically ordered: some are more available (and hence more expected).

As we have already argued, a translator may also decide to work against the established 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 TL text. At least, the risk is always there. Most translators, in most situations, regard this price as too high and are quite reluctant to pay it, and therefore the overall 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 in those years 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-s’arav, gidem patax dlatav, ve-ilem siper li zot bi-sfatav. mi xeres yasur halom, yešev ve-yišma ve-yakšiv dom.

batim be-Gan-Eden ze’irim-ze’irim, tsaxim mi-šayiš u-mi-šeleg behirim, gagotthem lo teven v(e)-lo kaš, ki im tsapixiyot bi-dvaš; kirot ha-batim ve-daltotthem, xalonehem u-trisehem, min ha-misderon ve-ad ha-ulam, min ha-ma’ake ve-ad ha-sulam – mantakim ma’ase ofe kulam. etsav motsi’im bigde tsiv’onim, minim mi-minim šonim, šeš u-txelet veargaman, ma’ase yede oman; ve-ugot be-karkom u-gluska’ot, rakot xamot ve-na’ot, gdolot šam be-xol ha-ša’ot, ve-xol še-ken le-Purim u-le-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-šar 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-atselem 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 vax(a)tsi ha-Gera.

amen ve-amen.

(Freud 1922)

## EXCURSUS B

### ‘Translation of literary texts’ vs. ‘literary translation’

Let us start with a brief summary of some of the points made so far. These will be used as a stepping stone in an attempt to draw a general distinction between the mere translation of texts of a particular kind and a kind of translation texts may undergo. This distinction, which is of general validity, will then be exemplified in the field of literature.

As we have observed, when a translation is looked at from the point of view of the culture which hosts it, it can be assumed that it was designed to fulfil certain needs of that culture. It does so by introducing into the culture a version of something which has already been in existence in another culture, which is deemed worthy of introduction into it. The introduced entity itself, the way it is incorporated into the new culture, is never a complete novelty, never completely alien to the host culture. After all, much as translation entails the retention of some aspects of the source text ‘invariant under transformation’, it also involves adjustments to the requirements of the target system. It is clear, then, that whatever is produced in a translation event (and act) is always **something which hasn’t been there before**: even in cases where the same source text is retranslated into the very same language, the resulting entity – that which would actually enter the culture – will definitely *not* have been there before.

In the simplest of cases, what is imported into the receiving culture is just a *text*. In more complex cases, however, *models* (i.e., sets of rules for the generation of any number of texts pertaining to a type which is recognized and acknowledged by the culture), even *modes of language use*, may be imported, with groups of texts which either embody (in the source culture) recurring patterns or are translated in a way which brings them close together. The novelty thus imported is not a mere reflection of what the source text as such has to offer, in view of the tradition it is part of, or even in comparison to possibilities of the recipient culture, however striking the incongruity between the two might be. Rather, the novelty of an imported entity is established in terms of the structural organization of the target culture as such. In other words, it is a function of what that culture is willing (or allowed) to accept vs. what it feels obliged to submit to modification, sometimes so much as totally reject.

For that reason, questions such as whether the translational transference of an English limerick into another culture/language may result, e.g., in a four-liner, in a non-anapestic entity or in a text which is devoid of a punch line, or even all three shifts together, are serious questions rather than mere teasers.<sup>1</sup> There is nothing in principle to *exclude* such possibilities in real-life translational behaviour. What is more, there are often circumstances in the host culture which may reduce the acceptability of a limerick which is transferred into it in an insufficiently modified manner, the result being regarded as an *irregular* textual entity, if not a non-text. Consequently, the recipient culture may prefer to accept precisely the four-line and/or the non-anapestic option. 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. Many of them still are in this position.<sup>2</sup>

By the same token, these are precisely the reasons why, for a long time, the translation of a Shakespearean sonnet into Hebrew was [more] likely to yield an Italian-like sonnet (see Chapter 8), or even a non-sonnet. The implications of such cases for making *theoretical* arguments are thus unquestionable. It is precisely in this respect that the *Schlaraffenland* example, which was dealt with in great detail in Chapter 10, was so illuminating. In this case, the finding was unequivocal: an unacceptable literary model was rejected, to be replaced by an alternative one which, albeit non-habitual, was deemed appropriate enough in terms of both the reconstruction of the source text as well as acceptability in the target culture. Calling this strategy ‘adaptation’, *Umdichtung* or anything allusive of that sort would not solve anything, even if the distinction between ‘translation’ and ‘adaptation’ were pertinent to the historical circumstances (which it hardly is, in this case). As I see it, instead of clarifying the issue, the use of such labels is likely to blur it. For what we are after is not a set of labels, however convenient their use may be. The whole

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1. I have elaborated on this particular example in my 1999 article “How Come the Translation of a Limerick Can Have Four Lines (Or Can It)?”.

2. 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 produced 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 people-in-the-recipient-culture would refer to the resulting entities as ‘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 *xamšir* [a five-line-poem] from the nineteen fifties on); another culture may retain the original name even if it adopts the model to a much lesser extent.

issue is rather one of 'why' and 'wherefore'. What we should be asking is therefore what might have been the reason that one text out of ten comprising an integral book, whose standing as such was respected, was regarded as requiring a different mode of translation, and why it was this particular mode that was selected.

It is numerous observations of this kind, made over a time span of some forty years, and attempts to account for them all *within* Translation Studies rather than dismissing them as simply non-pertinent (as is the attitude of many scholars who wish to proceed from an a priori, idealized concept of translation), that have led me to readdress high-level notions such as 'literary translation'. As is often the case (see, e.g., the discussion of the notion of 'translation problem' in Chapter 2), the first step involves the recognition that the phrase actually represents different concepts emerging in different contexts and serving various objectives.

### 1. Two senses of 'literary translation'

At a certain point in my activity as a translation scholar (e.g., in Toury 1984a) I came to the conclusion that the term 'literary translation', as it had come to be used in professional discourse on translation, 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, this sense refers to *any translation* of such texts; in a modified version – one where the focus is on the retention (or, better, reconstruction) of the source text's internal web of relationships (e.g., Even-Zohar 1971; Snell-Hornby 1987:93) – it takes such a text as a unique instance of performance, rather than its mere realization in language;
- b. The translation of a text – in principle, *any text*, of any type whatsoever<sup>3</sup> – in such a way that the product is acceptable as a literary text in the *recipient culture*.

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3. 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' – because of 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.



The surface manifestations of these two senses may of course concur. There may indeed be circumstances under which a close reconstruction of a text's web of relationships in an adequate fashion would override any problems of acceptability which may initially exist; 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 considerably weak vis-à-vis the source system, and hence willing to use the latter as a source of enrichment, in terms which transcend the individual text; or, finally, when a translator has gained a position in the culture which allows him/her to deviate from sanctioned patterns and get away with it, sometimes to the point of introducing *changes* into the culture, which are adopted and followed by others. The point about the two senses of 'literary translation' is thus not that they can never concur, but that there is no inherent need for them to do so. In other words, that they are different in *essence*.

Thus, neither the literariness of the source text, nor even the careful embodiment of its web of relationships in a TL textual entity, is enough to secure a position for the end product in the recipient literature, much less a position which would simply reflect the one the original enjoyed in its own cultural environment. In fact, 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 the source text as such, the model underlying it or the entire tradition reflected by it, all of them cases of interference on various levels.

As already indicated, the fundamental distinction between the mere 'translation of texts of a particular kind' and 'a kind of translation designed to fulfil 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 history book in TL and serve as a source of information *about* the legal state of affairs exhibited by 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 translated versions. However, only 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), beyond the melody that both versions should be sung in.<sup>4</sup>

Similar things can be said of the translation of a text such as the Hebrew Bible, which became canonized as a *religious* text: replacing the original in this

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4. And see the revealing discussion of the French and English versions of the Canadian anthem in Harris 1983. One may also wish to consult the website devoted to national anthems: [http://en.wikipedia.org/wiki/List\\_of\\_national\\_anthems](http://en.wikipedia.org/wiki/List_of_national_anthems).

particular function is only one of a long list of options open to the target text and its producer. Moreover, even if an attempt is being made to establish a religious text, there might still be a difference between, e.g., functioning as a *Jewish* and as 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, but rather that mode whose product was designed to be a religious text, taking into account what would count as one in the culture in question.<sup>5</sup> (This may be a strong interpretation of Nida's famous concept [1964; 1969] of 'dynamic equivalence'.)

Literature is just an example, 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, and certainly of 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 which should therefore be realizable by any literature. Rather, literature is first and foremost a kind of 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 [1924]) called 'literary facts' is a *systemic constellation* – a network of ad hoc relationships into which these phenomena 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 or linguistic formulation. It is the systemic position, and the cultural-semiotic functions which it gives rise to, that make the difference, not any surface realization. (Of course, there is also a mechanism which may help

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5. What translation would actually come to be canonized in the target culture is a different question altogether, which should be addressed separately: acceptance is never a mere reflection of acceptability and acceptability doesn't always ensure acceptance! More about this in Section 3 below.

keep pieces of older literature no longer regarded as literary under contemporary observation, as ‘have been’ – that is, historically literary.)

It is quite clear that only rarely will two different cultures fully concur on such matters; otherwise, why should they be seen as different to begin with? And since the (functional) identity of a phenomenon is governed first and foremost by the internal organization of the system which hosts it,<sup>6</sup> 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 bear on an act of translation to various extents too, which makes literary translation a *graded* notion, a matter of more/less rather than 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 it stands in inverse proportion to the latter.

‘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 specificity of the conditions they impose on the act (adding to other features of the event), while demonstrating *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 (‘genre’). (In this case, at least partial interference of the model underlying the source text is to be expected in the target text.)
- **A textually-dominated mode of 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 recognizable 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.<sup>7</sup>

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6. “[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).

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

Subjugation to target literary models and norms may thus involve the *suppression* of some of the source-text features, on occasion even those that 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 metre 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 easily occupy central positions within the translation (when looked upon as a text in its own right). They may even count among the **markers of the translation's own literariness**, despite their having had no basis in the original.

### 3. 'Literary translation' and target-orientedness

Unfortunately, the 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 (e.g. 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 have an existence 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 an act itself – and for its own purposes**.

Thus, it is not *acceptance* (or reception) which is the key notion here, but *acceptability*. In other words, what may be said to operate in literary translation (in sense (b)) is not any *fact* about the reception – or rejection – of its product (which is not yet there). Only *assumptions* (or expectations) can be operative here, namely, as to the *chances* a text will have 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,<sup>8</sup> translators can be more or less aware of the factors which govern

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8. Anthony Pym (e.g., 1993–1994) has often 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.

the prospects of texts and textual-linguistic phenomena to be accepted into 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 sigil of acceptability. Where in the target culture the results of these efforts will then be located is a totally different matter, which may indeed form part of a research programme in reception, literary or otherwise. While never to be totally ignored, the reception/rejection of translations is definitely *marginal* to the present line of reasoning, except for the effects it may have on *later* translations, that is, its implications for acts of translation which will be performed in the *future*.

Thus, not only can translations which have been carried out according to strict requirements of literary acceptability *not* be accepted into the target literature after all, 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 would never have been the case, had ‘acceptance’ been a mere extension of initial ‘acceptability’. The circumstances under which a culture tends to accept interference on one level or another (resulting from the translator’s failure to – or a decision not to – adapt the translation to target system requirements) constitute another interesting research area. In this connection, it should be emphasized that the acceptability of interference on one level does not necessarily entail its acceptability on any other level – a fact which may have interesting implications for policy planning in translation (see Toury 1998b, 2002).

The difference in focus between translating SL texts which *are* literary, on the one hand, and performing translation with the intention of *producing* proper TL 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 TL material will always 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*. This particularly concerns so-called ‘non-obligatory’ shifts (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. Of course, this does not mean that, in the course of its application, s/he won't 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 taking care to go all the way, one would necessarily arrive at the opposite end. The question is only what approach would be best suited for what one is after, especially as individual research projects would rarely go 'all the way'.

By now, it should have become very clear that the kind of approach I have been advocating since the mid-nineteen seventies does *not* "have much in common with conventional studies in Comparative Literature" (Snell-Hornby 1988: 24–25). Nor is this solely because there is no reason to limit the validity of this approach to the study of *literary* translation alone, in no matter what sense. (And see the detailed account of the English warning on German trains in Chapter 6, Section 2, where the very same theoretical framework and methods were adopted. Clearly, this is one of the least literary texts one could think of!)

Thus, even in cases where literary translation (in sense (b)) *is* the object of study, it would be simplistic to claim that the difference between "conventional studies in Comparative Literature" and target-oriented Translation Studies lies in the mere fact "that the [latter] deal with translations rather than original works", as Snell-Hornby further had it (*ibid.*). After all, even in the initial phases of a full-fledged study, when it is indeed *textual entities* which are taken up and analysed, the locus of study is never the text as an entity in itself, whether a mere TL utterance or even a replacement and/or representation in TL of another text, pertaining to another language/literature/culture. The locus is rather what the texts can reveal as concerns the *processes* which yield them: the options the translators had at their disposal, the choices they made and the constraints under which those choices were affected. Analysis then seeks to extract 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, and especially if and when those shared factors can be tied up with the organizing principle which governed the establishment of the corpus.

Two of the most obvious principles for grouping texts together for study purposes, especially in the literary domain, are the fact that the texts are (assumed to be) translations (a) of one and the same text, or (b), of texts composed by the same author. Incidentally, these criteria often prove of limited consequence when regarded from a target vantage point. The main thing normally yielded by a body of texts thus constituted is a series of more or less *independent* observations regarding the 'legitimate rights' of the source text or of the author's personal style,

whether they were honoured or not-so-honoured. Not uninteresting, to be sure, not even unimportant, but hardly enough, if what one wishes to achieve is an account of phenomena occurring in translations as *translational* features rather than mere blunders; that is, an account using ‘positive’ rather than ‘negative’ terms.

As we have seen throughout, a great deal of *contextualizing* would have to be done, if interim results are to gain in significance, which is really what target-orientedness is mostly 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 interim results have proven insufficient? Why not start contextualizing the texts and their phenomena right away?

All this, and much more, would render any study of this kind, let alone the general framework in which it is performed, highly *unconventional* indeed, in terms of Comparative Literature. In fact, 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 nineteen seventies. More than one comparatist who found him/herself attracted to it was sneered at, sometimes so much as stripped of the honourable title ‘comparatist’... By contrast, it does plant this approach at the very heart of a Translation Studies.

#### 4. The gap between the two senses of ‘literary translation’ as a function of cultural distance

One point that the *Schlaraffenland* example (Chapter 10) has indicated 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, this is 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, even if they were willing to subscribe to a version thereof for translation *practice* and *teaching*, such as Vermeer’s highly influential *Skopos* *theorie* (e.g. 1983) or Holz-Mänttäri’s more idiosyncratic *Handlungstheorie* (1984). In fact, at times, I have even been 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 1991), Chinese (Chang 2008, 2011) or even French culture (e.g., Lambert 1988, 1992; D'hulst 1982, 1987; Lambert, D'hulst and van Bragt 1985)...

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 which is always involved in such 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-linguistic 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 have simply never been 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.<sup>9</sup> Let us therefore take a quick look at one famous case where the starting point indeed involved a huge gap coupled with almost zero previous contact between the two cultures.

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

– END-RHYMES – phonetic, or sometimes visual – with the possible addition of INTERNAL RHYMES, such as in:<sup>10</sup>

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

9. See, e.g., Snell-Hornby's discussion of the German translation of Salman Rushdie's *Midnight's Children* (1987:96–99) and Nord's discussion of exemplary passages from Lewis Carroll's *Alice in Wonderland* (1991:99–102).

10. The numbers in brackets refer to the Appendix, where 27 texts purporting to be translations of one and the same 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 taken to serve as another line, also 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 have been using as my data base (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.]

More important still, the added features go against the grain of the principles of the model underlying the Japanese text. One can almost say that it is precisely due to their absence that the original is regarded as an occurrence of the *haiku* ‘genre’; this absence sets this text-type apart from all other poetic types in *Japanese* literature. Some of the features which characterize the English translations, e.g., the rhymes, the titles and the added modifiers, actually represent a complete REVERSAL of the ones permitted by the originals. By contrast, **they are all drawn from the target repertoire of poetic devices**. Thus, their selection was no doubt intended to bring the end products close(r) to what may safely be regarded as ‘poems’ in the English culture. (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 present in the source text and which are part of its underlying model and the entire *haiku* tradition, even features which are among the most salient ones. Most notably, only rarely is an English translation found which has exactly 17 syllables, which is THE *differentia specifica* of a Japanese *haiku*. Obviously, even if English and Japanese conceptions of a 'syllable' concurred (which they do not), nobody in the recipient culture at that time would have been expected to count the number of syllables employed in the English text, much less to look for an 'irregular' number such as 17 in a 5–7–5 distribution. This 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 1940s, *haiku* poems were selected for translation because (preliminary norm!) they were regarded as concurring with a tendency towards *exoticism*, which was rather strong in the target culture of that time. 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)

Some thirty years later, in the second phase of its introduction into the cultures in question, Japanese *haiku* poetry was perceived as an outstanding example of such *suggestive realism* as was then popular, e.g., in Dutch/Flemish culture. Now text selection was dominated by this conception, and the translational 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 simple 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* were mainly submitted to 'literary' translation, except that in the time that has elapsed, the conditions of 'literariness' within the target cultures in question have undergone an immense 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 translations of the “Crow” haiku

- |   |  |
|---|--|
| 1. On a withered branch<br>A Crow is sitting<br>This autumn eve. (Aston 1899)   | 8. On a withered branch<br>A crow is perched<br>In the autumn evening. (Blyth 1949)                    |
| 2. The end of autumn, and some rooks<br>Are perched upon a withered branch.<br>(Chamberlain 1902)   | 9. Autumn evening:<br>A crow perched<br>On a withered bough. (Blyth 1952)                              |
| 3. The autumn gloaming deepens into night;<br>Back ‘gainst the slowly-fading orange light,<br>On withered bough a lonely crow is sitting.<br>(Walsh 1916) | 10. Autumn evening –<br>A crow on a bare branch. (Rexroth 1955)  |
| 4. Lo! A crow sits on a bare bough,<br>‘Tis a dreary autumn evening.<br>(Miyamori 1930)   | 11. On a withered branch<br>A crow has alighted –<br>Nightfall in autumn. (Keene 1955)                 |
| 5. <i>A Crow on a Bare Branch</i><br>A crow is perched on a bare branch;<br>It is an autumn eve. (Miyamori 1932)  | 12. On a withered bough<br>A crow alone is perching;<br>Autumn evening now. (Yasuda 1957)              |
| 6. On a withered branch<br>a crow has settled –<br>autumn nightfall. (Henderson 1933)   | 13. <i>The End of Autumn</i><br>Autumn evening: on a withered bough<br>A solitary crow is sitting now. |
| 7. <i>A Crow on a Bare Branch</i><br>Autumn evening now<br>A crow alone is perching<br>On a leafless bough. (Yasuda 1947)                                 | 14. On a bare branch<br>A rook roosts:<br>Autumn dusk. (Bowans 1964)                                   |
|   | 15. A crow<br>Perched on a withered tree<br>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 bough  
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)



## Studying interim solutions

### Possibilities and implications

One of the main objectives of Translation Studies is to offer as good and as full as possible an account of what acts and processes of translation actually look like. Towards this end, mere lists of factors which may exert influence on a person as s/he is translating and on the results of her/his translational activity is not enough. It is no less important to establish how that person manoeuvres between constraints of different kinds and sources as s/he goes along, and to assess the interdependencies of the various constraints and their relative force, given the circumstances under which the event is taking place.

Whereas the main tool for the establishment of the possibilities at a translator's disposal is speculative reasoning, the study of their actual workings can hardly be divorced from instances of performance. The obvious obstacle here is that acts of translation do not lend themselves to direct observation. Consequently, some *indirect* means of approaching them will always have to be used.

Attempts to rely on *retrospection* while trying to reconstruct processes of internal give-and-take, which assumedly take place as part of translational decision-making, have often fallen short: to the extent that an individual is able to recall what passed through her/his brain in the first place, there are too many factors which may intervene and tamper with the reconstruction even of those parts of which the translator was more or less aware as s/he was busy translating. This in itself is enough to cast doubt on the reliability of translators' retrospective evidence as a source of data, whether offered voluntarily or solicited: one simply cannot be sure just what such evidence represents. Indeed, finalized translations can often be shown to be at odds with the 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.<sup>1</sup> Statements elicited by researchers normally do not fare much better, as shown, for instance, in the overview given by Sandrock (1982: Chapter 2).

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1. And see Chapter 4, Section 4, for a short discussion of the problematics of using normative pronouncements as sources for the establishment of the norms which have actually governed translation.

### 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 data, which are, in a sense, *external* to the act as such: their existence precedes or follows it. Towards that end, a variety of methods has been used, all having in common the one fact that the only entities submitted to scrutiny were the output of the act and its input (not even that part of which that was actually taken in, which cannot be observed). More often than not, an attempt was made to draw whatever conclusions were made by recourse to comparisons of (assumed) target vs. assumed source texts. These studies came in 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 allegedly applied to the same corpus. Obviously, such comparisons do not allow replicability in the true sense of the word, which requires that the different studies be performed in the same theoretical framework, using the same methods.

Above all, 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 achieve is the formulation of *explanatory hypotheses* capable of accounting for the establishment of (more often than not aspects and parts of) translated texts. If one is lucky, these hypotheses may be established in a way which is not at odds with knowledge obtained from other sources, using other methods. Such hypotheses may be more or less plausible, but more often than not they retain the status of mere hypotheses which are 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 itself – a very convenient rationale for the descriptive researcher which, however, is not always that easy to justify, especially when one’s corpus transcends the borders of a homogeneous group whose members can be assumed to have come into being under exactly the same norms.<sup>2</sup>

More problematic still, the use of comparisons as a method entails an inherent weakness that specifically concerns the study of *processes*.<sup>3</sup> As long as it is only

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2. And see the brief discussion of the possible psychological validity of the notion of ‘bitext’, or the coupled pair of ‘replacing + replaced segments’, in Chapter 6.

3. By contrast, studies into the process, especially of a psycholinguistic kind, often ignore the final version of the product, whose establishment forms the very *raison d’être* of the said process. This issue will be taken up in Chapter 14.

pairs of final(ized) *texts* that are available for study, there is no way of knowing how many different persons were actually involved in the establishment of the assumed translation, playing how many different roles. Whatever the number, the common practice has been to collapse them all into one persona and regard that conjoined entity – the bridge between SL input and TL output – 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* (as against the comparability of texts, textual segments, textual-linguistic phenomena, etc.), which is a sine qua non for making any kind of justifiable generalizations.

Very often, hypotheses which were formulated with respect to the translator and his/her actions are thus related to a *hypothetical construct*; i.e., a functional entity mediating between the two texts rather than a person. Even in the case of the most prestigious translators, whose products may well have been tampered with least of all, one can never be sure just how many hands were involved in the establishment of the translation as we have it and who did what; and most cases are not as extreme as this.

A similar claim could be made from the vantage point of the exact way the translation came into being. After all, the process which yielded it may entail different kinds of activity, which may be widely dispersed in terms of both time, space, and the agents responsible for them. 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 proof-reading, has usually been collapsed into one which is taken to represent a single ‘act’ of translation. Obviously, 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 assumed source.

As long as comparisons are mainly 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 a translation. In fact, the proponents of the use of comparative methods in Translation Studies (e.g., van Leuven-Zwart 1989, 1990) often 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 greatly from knowing whether the same attitudes were shared by all those involved in the production or whether a (direct or indirect) normative negotiation took place, maybe even a struggle, and if so – who 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 also 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 sought to break down both fictitious constructs, both the ‘translator’ and the ‘translation process’, into their components and 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 has been devised which are normally applied to individuals, most of them in real time, i.e., as they are performing a more or less uninterrupted act of translation. 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 an obvious exaggeration, to say the least. What they involve is normally little more than an expansion 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 in Chapter 14, when evidence which comes into being in a ‘natural’ way during the translation process itself will have been exhausted. At this point, we focus on the contribution of research on *interim decisions* made by translators on the way to the finalized textual output, as documented in manuscripts, typescripts, corrected galley-proofs and the like.

To the extent that the interim versions have not been destroyed (which, unfortunately, is often the case), they can be taken to substantiate the good old notion of ‘multiple stage translation’ (Voegelin 1954), which seems to have sunk into oblivion, mainly owing to the domination of the myth of the ‘text’ (Toury 2010). This notion is based on the assumption that, as a rule, translators do not attain a result which they are willing to accept (under the norms they have subjected themselves to) in one fell swoop, but rather in *a series of shorter moves*. This enables the translator to practise self-corrective feedback, constantly employing self-monitoring as a procedure. From the point of view of the researcher it may be taken as good evidence of a **decision-making** process (Wilss 1994), which is precisely what we are after; that is, decisions that have not only been *formulated in language*, but also *committed to paper*. To be sure, even this would never amount to everything that has been going on in the translator’s mind as s/he was translating, but there is certainly a lot more information here than the two texts alone. Moreover, and much more importantly, this type of data gets much closer to tapping processes of translation as they actually go on, even though by no means ‘in real time’.

True enough, it is not always easy to lay one's hands on interim phases of the emergence of a translation, let alone all of them. Moreover, even if a number of successive versions *are* available, gaps will often be found to exist between them, testifying to some missing links, either a lost portion or one whose only existence was in the brain. Still, the limited availability of interim solutions notwithstanding, phenomena of this kind have all too seldom been used as evidence. Thus, some thirty 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 situation has changed very little since. In fact, in 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 so much as 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 now, in the computer age, this kind of study would have become obsolete. However, while it is true that less use is now being made of paper, it is also true that there are possibilities of retaining everything that is put into the computer, and this seems to actually promise *more* data than what used to be committed to paper in older 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 translators nowadays would be *willing* to have all their activities tapped, 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 interim decisions either (which is one of the main reasons they got lost).<sup>4</sup>

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4. One program which captures every keystroke has become quite popular in the last decade. I have in mind TRANSLOG (see e.g. Jakobsen and Schou 1999; Jakobsen 2006). This program can be combined with other ways of measuring human behaviour such as Eye-tracking and Electro-Encephalography, to yield much more elaborate research projects. A combination of those three methods is now being tested in a multi-national experimental project called EYE-to-IT (see: [http://www.hf.uio.no/ilos/forskning/forskningsprosjekter/EYE\\_to\\_IT/](http://www.hf.uio.no/ilos/forskning/forskningsprosjekter/EYE_to_IT/)).

## 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 would not even have been asked under other kinds of observation conditions:

- why particular options, or options of a particular type, were promoted to translation solutions to begin with, while others were not;
- why certain options were then rejected (i.e., their status changed into ‘temporary solutions’);
- why other replacements, or replacements of other types, were opted for instead,
- and so on and so forth, towards the one option which remained fixed in the final version.

(Needless to say, the creation of a target text 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 sets of corrected proofs show 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. At the same time, the insights it is likely to offer are also minimal: the act may seem to have proceeded automatically, or else things that did go through the translator’s mind were simply not externalized. 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 *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 the relative force of them as constraints on the act.

The reconstruction of an act of translation can be likened to an archaeologist's attempt to reconstruct the original course of an ancient road which had almost completely disappeared. If there are only two points along the road which are known, especially if there are reasons to regard them as the 'beginning' and the 'end', respectively, and if there is no complementary knowledge from any other source, there will be an infinite number (mathematically speaking) of possible ways of connecting those points in a way which might represent the road. If there are pieces of external knowledge (geographical, historical, whatever), the number of potential routes will decrease and there may even be a possibility of organizing them in a hierarchical order of likelihood. Still, it will be next to impossible to reconstruct the road both in full and in a way which would be acceptable to all. Some might start looking for the shortest, or the quickest alternative, but although this may make a lot of sense when designing a new road or when training others to plan and build one, it will not necessarily be acceptable to the archaeologist. Finally, if a number of intermediate stops along the way are known, or made known (e.g., in archaeological excavations), the number of candidates will fall. And the larger the number of stops, the more convincing the reconstruction will be – even if, in factual terms, it may still be 'incorrect'.

As we have seen right from the start (especially in Chapter 2), there is absolutely no need for this reconstructed distinction between the more and the less problematic (in the sense of PROBLEM<sub>3</sub>) to be identical to the one yielded by an analysis of the source text as such, or even to its initial translatability vis-à-vis a particular TL (PROBLEM<sub>1</sub>). It is well known that translational conventions often become consolidated with respect to certain phenomena, including ones which may well have been marked as a potential problem from the contrastive-linguistic or contrastive-textual standpoint. 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 the act of translation and 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 had evolved with respect to their translation into the TL in question. Be that as it may, the relationships between the different notions of 'problem' would be of the utmost importance for any understanding of the translation act. And its results, one hastens to add.

To ensure a solid basis for making conjectures about the decisions made in the act, and especially about the considerations which may have yielded them, it is vital that, for each point in the text, **the various layers discerned be arranged in their correct order** (the archaeological view again!), which is not always all that easy. On the other hand, as long as the documents, or at least the revisions,

are in longhand, the intervention of different agents (e.g., translator, editor, proof-reader, etc.) is normally quite easy to discern, which is yet another advantage of the method.

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 (or maybe divided into three parts, each one of which was replaced automatically in a serial manner):

1. 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 ( $\approx$  stages of the act), the revisions themselves should be taken to define the coupled pairs on which the comparative analysis would then be performed (see Chapter 6). To be sure, not only will the replacements 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 to real-life processes of translation as well as a logical continuation of our basic research method.

What one would have at one's disposal, as a result of breaking each version down into its constitutive layers and regarding them in the correct order, 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 *SL* segment (to the extent that the latter has remained unchanged, so that the revisions were applied within its boundaries), but also with its *TL alternatives*, at least those that were verbalized.

What should always be kept in mind is the fact that the interim outputs represent **phases in the emergence of a single text** rather than a series of textual segments having a degree of independent status. Even though a revision may 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 submitted to revision. As the number of revisions tends to differ from one (reconstructed) unit to another, no justification can normally be given for reducing one multilayered version to a number of self-contained texts, much less to a number which would correspond to the number of layers it has been found to comprise. Textual *considerations* may have formed part of (some of) the decisions, but no 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 revising activity. The observed revisions offer regularities which are sufficient to warrant some *generalizations* with respect to at least the monitoring of the emerging target text for discourse transfer, partly negative, partly positive, which manifests itself as one of the aspects which Wheen lay special emphasis on – and increasingly so with each successive revision.

- a. The translator started by moving linearly along the sequence *jetzt haben wir den Magen [voll]* and gradually replacing it (stage (1)) by 'now with a belly', which would require a complementation by 'full of X' as a replacement of *[voll] weißer Bohnen und Rindfleisch*. Within this sequence, the word *Magen* was replaced by one of its English equivalents, 'a belly', both nouns being in the singular. This means that the grammatical number used in the assumed 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 Wheen was taking into account as he made his first decisions. Many of these units he would later revise, some of them with recourse to larger units. In this case, it was probably a one word (or a two-morpheme) unit.
- b. After writing down 'a belly', the translator halted, went back a little and crossed out these words. Presumably, before he came to actually writing down the expected complement, 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 grammatical feature represented so-called 'negative' transfer: 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'). The 'short circuit' in English (lack of number concord) must have seemed to him more critical than the German parallel one, and it doesn't really matter whether his impression was correct or incorrect.
- c. Instead of the erased words he now writes down a form in the plural, 'stomachs'. The fact that the noun itself was also changed (*belly* → *stomach*) implies that, while identifying the *grammatical* problem, the translator also became worried by some *semantic* and/or *stylistic* issues. The new noun will be kept for four successive stages and the old one will be picked up again in stage (8). Needless to say, whoever reads the final version only will never be aware that the translator considered, and very seriously so, another replacement. By contrast, the grammatical feature 'plural' will now have been fixed. It is not impossible that the translator might still have returned to this unit from time to time, but he has left no traces of such visits,

- d. Although later on he will be returning to the beginning of the clause (stage (6)), the translator now (stage (3)) turns to supply the missing complement; and he does it in the most expected way: 'full of X' (stage (2)). However, when he gets to filling in the slot X, an instance of transfer is introduced into the emerging English text once again: the order of the members of the compound [*weiße Bohnen*] und [*Rindfleisch*] – a standard dish in the German army in those days (beans and beef). This order is conventional, whether the dish consists of mostly beans or mostly meat. Thus, keeping this order (in 'haricots and beef') certainly involves SL interference in TL, albeit not really of the 'negative' type. This order is retained in stage (3) and then (stage (4)) reversed, probably to adapt the formulation to English habits, and hence bring the translation still closer to meeting its prospective reader's expectations.
- e. The two constituents of the dish change in virtually all the following stages and the final decision is not manifest until stage (11). Thus, whoever reads the translation in its final version will never know that for our translator this must have constituted a major problem. After all, it would have been perfectly possible to move from *weiße Bohnen und Rindfleisch* directly to 'beef and haricot beans'. Even less would the reader of the completed text be able to know that the translator wavered not only between concrete words, but also between looking for replacements on the level of the individual constituents and on the level of the whole compound, and between retaining the 'Germanness' of the dish and replacing it by a more British one (cf. mainly the item 'bullybeef').
- f. In stage (6) he decides to go back to the beginning of the German clause. In the meantime, the draft version was typed. A possible speculation would be that it now occurs to the translator that there might be a possibility of getting closer to the structure of the original sentence. Anyway, he goes back to the German text again, reactivates it and writes down a new English opening: 'now we have', introducing into the text a new instance of transfer. (The inversion of 'now have we' to 'now we have' was probably executed automatically. At any rate, there are no signs of conscious attention to it.) This product is (immediately?) monitored (stage (7)), and no other recourse to the source text occurs in the translation of the segment under observation. 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.



g. 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 translation (12), which is something no one looking at the final version alone could have found out. One possible explanation for this last-minute change is that before approving it for publication, somebody (the translator himself? an editor? a proof-reader?) read the translation *as a piece of English text*, checking it for stylistic acceptability.

Such a study may give rise to the following kinds of general observation, which require a lot more work.

1. To the extent that a number of alternative interim replacements refer to one and the same SL segment, it may be beneficial to regard those alternatives as constituting a ‘translation paradigm.’ This, however, would not be Levenston’s 1965 paradigm, which was intended as a *prospective* tool, but a *retrospective* one, comprising *realized decisions only*.<sup>5</sup> On occasion, the two notions may of course overlap, in part or even in full. The point is, however, that there is

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5. A few years ago, the Israeli linguist David Téné presented a detailed analysis of 16 Hebrew translations of Heinrich Heine’s short poem “Ein Fichtenbaum steht einsam” [The Lonely Fir Tree] (Téné 1996). The most comprehensive, and by far the most non-routine part of the article (Section 3, pp. 18–23) contains the Hebrew realizations of what the author described as a “mini-thesaurus of the key-concepts of the poem”, of which he identified 16. Thus, the fact that a Hebrew item served as a translation replacement at least once was the only criterion for its inclusion in a paradigmatic group. No questions were asked either about the relationships holding between the members of single pairs of replacing + replaced items or about role of time in the changes that occurred. After all, the 16 translations extended over a period of almost 75 years (1923–1996) and some of them may have been related to older versions, whether in an affirmative or polemical way.

The result is that several of the 16 thematic groups presented by Téné also include translational replacements which are not a priori (that is, interlingually) equivalent to the nuclear meaning of the original German item. It is a pity that Téné didn’t go one step further and use his ‘thesaurus’ to draw some of the possible conclusions, which are mostly on the socio-cultural and historical levels. He makes do with claiming the obvious: namely, that an SL item can be replaced by a host of different TL items, some of them semantically closer than others, and that the chances of any given replacement are directly connected with the number of different translations produced.

What Téné does next (1996: Section 4) is typical of a source-oriented approach: he chooses what is for him the ‘best’ Hebrew replacement of every single key concept in isolation (in accord with the concept of translation he subscribes to) and has them concatenated to form what he regards as “the most ‘loyal’ translation” (p. 23), but which is, in fact, just another translational option. He does notice that no translator came up with that solution but never bothers to ask why.

no need for this to occur. In other words, a retrospective paradigm needn't include all the options that a prospective one would have contained; e.g., it is not necessary – I would dare say: not even feasible – that a translator would go through the list and try all the options (is that number final, to begin with?) before making his/her decision. However, it would be interesting to find under what circumstances he will tend to opt for such a procedure as a translational strategy.

2. What is even more fascinating is that the retrospective paradigm may also include options which do not appear in the prospective one, that is, that the translator could have tried out replacements which do not count as part of the prospective paradigm. On occasion, the *final* replacement itself may be of that kind. There is also a possibility that such a replacement would become habitual in TL, what Schwarze (1981: 165) called “eine übliche, aber nicht semantisch äquivalente Übersetzung” (see Chapter 13).
3. Acts of translation proceed in a dominant *direction*: they are generally forward-bound. However, this progression is not necessarily strictly linear. Unlike interpreting, especially of the ‘simultaneous’ kind, it is characterized by **loops** of various sizes: going back part of the way in order to resume the forward movement from another point.

### 3. Possible implications for Translation Theory

It is clear that this type of study, and the conclusions it gives rise to, 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 act of translation, especially those resulting from sheer speculation or involving an idealized concept of translation.

Consider the following insightful statement offered some 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, the act of translation is accounted for in terms of a ‘monitor model’, which should make it compatible with the nature of our findings vis-à-vis the Remarque example and accommodate comparisons for the sake of verification/refutation.

To be sure, it is somewhat unclear whether Ivir had real-life processes or ideal(ized) ones in his mind and thus whether his formulation was offered as a *theoretical* pronouncement or a *descriptive* one. It is mainly the decision to use verbs in the present tense which gives rise to doubts (begins, is ... not available, [is not] able to ensure, resorts to). In fact, in spite of the lack of modal verbs, the author may also have wished 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 act of translation is not completely unfounded. After all, a case has been found where the kind of reasoning he used seems to apply, 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, even on the basis of a single case (but preferably much more than that) would shed considerable light on its validity. In fact, if a hypothesis which is questioned is to be retained at all, it will normally have to undergo some *modifications*, e.g., by specifying the 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. Owing to a shortage of findings, there is therefore very little to go by on the way to 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, both quantitatively and in terms of the variety of behaviours studied. 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. We will ask ourselves why, towards the end of the next chapter.

## 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 a relatively independent textual segment of considerable length will be presented in some detail. In the process, an attempt will be made to uncover the *constraints* to which the translator subjected himself as he went along and the way he manoeuvred among these constraints, especially in cases where they would have led him in different directions. We will also take a cursory look at the *interdependencies* of various constraints and at their *relative force*, which is as close as one can get to establishing the norms which governed this particular act of translation. In addition to its status as a case in point, this chapter is designed to illustrate the methodological claims concerning the use of interim solutions in the tentative reconstruction of a translation process as they were presented in Chapter 11.

#### 1. The materials under study

The text in question is the Hebrew translation of Hamlet's monologue "To be or not to be" (*Hamlet* III:1) made by Avraham Shlonsky (1900–1973) in 1946, first and foremost for staging at the Habimah theatre. At this point in his career, Shlonsky enjoyed a central position in Hebrew culture. 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 these norms, especially in the translational domain, were regarded – not always with full justification – as his disciples. No wonder, then, that it didn't take long for Shlonsky's *Hamlet* to completely supplant all previous Hebrew versions of the play.<sup>1</sup> Unlike them, Shlonsky's version

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1. H. Y. Bornstein's translation, made in Warsaw, was first published (in instalments) 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).

also enjoyed rapid canonization, both in the theatre and in printed literature – a canonicity which it managed to retain its for several decades. In fact, in spite of a considerable change of mainstream norms and in spite of a number of newer translations, important sectors of the Israeli culture still cling to Shlonsky's text, and not just out of nostalgia either. Rather, as is so often the case (see Chapter 4), norms which had become dated did not disappear completely. Though no longer in the cultural epicentre, they still enjoy a certain prominence, mainly through their association with Shlonsky and his colleagues.

My main source of data is a *manuscript* in Shlonsky's handwriting, which – judging from the quantity and nature of revisions – constitutes the very first draft of the translation. As such, it comes close to representing a single activity, though not necessarily an uninterrupted one. This manuscript (No. 3:41–52) forms part of the Shlonsky Archive at the Kipp Center for Hebrew Literature and Culture at Tel Aviv University.<sup>2</sup> The pertinent pages bear the numbers 5–7, which makes it clear that the monologue was not the first extract of the play to be translated. (Page 6 is reproduced as Figure 12.) This however doesn't imply that the play was necessarily translated in the original order: bunches of pages could well have been numbered separately; e.g., when handed over to the typist. Fortunately, this uncertainty doesn't seem to affect our account of the emergence of the translated monologue.

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 in a text submitted by a translator of such standing, it must follow that a link is missing; possibly a clean(er) copy of the manuscript. This typescript was then revised by the translator, in his own handwriting. Interestingly, the revisions lead in two slightly different directions: one set approximates the text to the version which was finally put on the stage, whereas the other brings the typescript closer to the first book version. To be on the safe side, the two final versions, both dated 1946, were also consulted.

Thus, even though we might not have all the interim phases of the monologue, not even all those that were committed to paper, we do have quite a lot, which

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2. I would like to thank the Katz Institute for the Study of Hebrew Literature, Tel Aviv University, which is now part of the Kipp Center for Hebrew Literature and Culture, and its former director, Prof. Reuven Tsur, for their permission to use the material and reproduce portions of it. So far, I haven't managed to locate p. 8 of the bunch in question, which should have included the last five lines of the monologue.

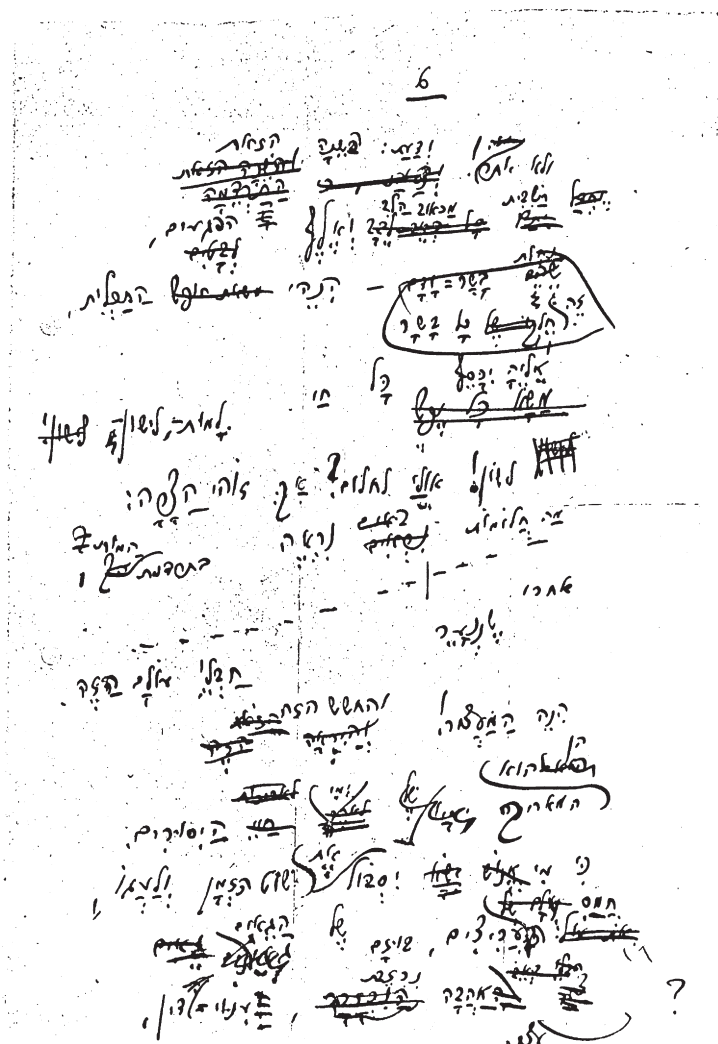


Figure 12. Page 6 of Shlonsky's first draft of the Hamlet monologue

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 given to observations which are made in terms of a reconstructed translation process (see Chapter 11).

As to the source text: throughout most of the discussion, it will be found to have played a rather marginal role in directing the revisions. In fact, we have both external and internal evidence to the effect that Shlonsky didn't proceed from any English text, but rather from a combination of mediating translations in a number of languages (Russian, French, German). There is thus reason to consult any of these texts only if and when this is warranted by the analysis itself. This temporary suspension of the source text, discussed rather extensively in the methodological part of the book, will be taken up again in the concluding section of the present chapter.

## 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 subjected himself to before ever setting 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 production of the text. 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 is further attested to by several markings he made in the margins. It is clear that he felt an occasional urge to check the compatibility of his verses with the metrical scheme.

At the same time, Shlonsky's iambic hexameters are not an exact reflection of Shakespeare's verses, which are *pentametric*. Rather, they represent a compromise between the original format and the need to accommodate the typically greater number of syllables of the average Hebrew word, a strong constraint in any translation of poetry. Thus, it is always very difficult, often practically impossible, to pack the entire informational content occupying ten English syllables into a ten-syllable Hebrew line. 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. It may even have been present in one or more than one of the mediating translations Shlonsky used. Further research may help us clarify this.

No less significant is the fact that no attempt seems to have been made to fully domesticate the metre, e.g. by replacing the iambs by Hebrew *anapests*. This option would certainly have offered a far more convenient way out of the

difficulty of reconstructing the overflowing semantic content. Such a solution would also have been more in line with the nature of the Hebrew language in its 'Sephardic' pronunciation, which was rapidly becoming dominant. Since the anapest is so very characteristic of Hebrew poetry, it would also have counted as no less functionally equivalent to the original metre than the iambic hexameter; indeed, probably more so. On the other hand, it might have been regarded as too much of a deviance from the *formal* properties of the original and of the tradition it represents.

This compromise was not really Shlonsky's. Rather, at that point in time, substituting Hebrew iambic hexameter for English iambic pentameter was well within the norms of poetry translation. All that Shlonsky was required to do was to decide whether to adhere to that sanctioned behaviour or run the risk of producing a 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 English pentameter by one Hebrew hexameter did more than pack more material into the translated text without adding extra lines – a kind of solution which had already become marginal, almost obsolete. It also made it possible to treat the **line** as the ultimate unit of translational consideration, making as many of the necessary adjustments as possible 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 known as the 'Habimah Heritage'. Thus, the translation in question was commissioned by a *theatre*, and was intended first and foremost to be performed. Notwithstanding, what the norm which governed Hebrew theatre of the mid-nineteen forties required was a far cry from a simulation of any kind of language actually spoken. 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 of the 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

(a) *lihyot o lo lihyot? hine ha-še'ela*

or

(b) *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 line 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 in their speakability, on the other, lending each of them both advantages and disadvantages, within the whole array of norms which the translator subjected himself to.

Grammatically speaking, *hine ha-še'ela* (a), 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 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, that 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* (b) 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 in terms 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 would have been concerned about precisely these considerations.

Except for this hesitation, which – in spite of the many revisions made throughout – 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 metrical line. At the same time, this line represents a very close

reflection of the original line too, which may have been taken as another point of strength:<sup>3</sup>

To | be | or | not | to | be, – || that is | the | question  
 li | hyot | o | lo | li | hyot? || hine (hi) | ha- | š(e)ela

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 one. 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)ela*), 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: on occasion, Shlonsky seems to have shown some readiness to pay a certain price in terms of poeticness, at least temporarily, but he was never willing to consider a similar price in the prosody.

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 monologue, recourse to poetic language (according to the prevailing norms, to be sure, and independently of the features which characterize Shakespeare's own language use) is another constraint on 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 TL item is substituted for another, both the replacing and replaced items tend to belong to approximately the same type of language.

3. 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 Popović [1976]:21].)

### 3. Using revisions to uncover constraints

Although the rest of the monologue was not born as easily as its first line, 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 according to the translator's own concept – the one he shared with the culture within and for which he was operating – as to what would constitute a proper text for the *stage*.

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 the translation. For instance, beyond the privileged status of the poetic line as an ultimate unit of translational consideration, there also appears to be a tendency for this unit to be **cut in two**, preferably **symmetrical parts**. Such a tendency is often a concomitant of the decision to use the hexameter, and a larger corpus may even substantiate the hypothesis that the convenience of working on half-line units further enhanced the adoption of this basic prosodic norm. An almost inevitable outcome of the symmetrical division of a hexameter (3 + 3), in turn, is a considerable degree of *monotony*. However, monotony is not necessarily marked as negative, especially when it comes to speakability.

True, here and there the English line itself falls into two equivalent parts. Still, Shlonsky's application of the principle to lines 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's, again in accord with the poetics he epitomized. Consequently, the few cases where this strategy is in keeping with the original itself may be considered a happy coincidence, the more so as Shlonsky did not rely on the English text as his immediate source (although rumour has it that, on occasion, he would consult some speakers of English). As already hinted, the practices of whatever translation(s) he may have used may well have played a role in forming this strategy.

To take an example: Shlonsky opened his translation of line 3 of the monologue with the clause *et kol xitsav* (in the accusative) [all the arrows (of)] – close enough to the source text (or mediating translations) except for the added quantifier *kol* [all] which winds up the first iamb. (The declined noun *xitsav* represents one full iamb. The word can be read in one way only and cannot be manipulated.) Within the confines of Hebrew grammar, this opening predicts the subsequent occurrence of the particle *šel* [of] plus a second noun phrase such as *goral axzar* (to

replace 'outrageous fortune' of the original or a replacement translation thereof into another language).

At this stage it must have occurred to Shlonsky that he had used up two whole iambs but achieved very little, in terms of reconstructing the semantic content and verbal formulation of the line. A version such as

\* *ēt kōl xītsāv šēl gōrāl āxzār*

would have been much too long (9 syllables out of the 12–13 he had at his disposal for the whole hexameter) and problematic from the prosodic point of view (a superfluous unstressed vowel introduced with the grammatical word *šēl*). Complementing the verse to something like

\* *ēt kōl xītsāv ũ-qlā'āv šēl 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 xītsē vē-qāl'ē gōrāl āxzār*

This time, (minor) deviation from the prosodic scheme would not have been the only reason for rejection. Rather, the impossibility of accepting such a formulation stems first and foremost from the use of two conjoint dependent nouns in the construct state (*xītsē vē-qāl'ē*). While many writers and translators might have been willing to use such a construction, it would have counted as a breach of the norms of acceptable style, sometimes as grave as breaking the very rules of normative grammar. At any rate, Shlonsky gave up the whole opening, returned to the beginning of the line (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 actually written down reads as follows:

*xīts.ē 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 surplus word *kōl* (in this phonetic environment pronounced *xōl*), 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 text(s) he drew upon, 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 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', or of any of its replacements in the possible mediating translations, was definitely much lower in Shlonsky's hierarchical order of constraints.) The only problem with *blistra'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 and be understood, not even while declaiming. Consequently (I suppose), a fourth version comes into being:

*xitsē gōrāl āxzār || āvnē mǎrgēmōtāv*

Not only is *avne margemotav* easier to pronounce, but it would also have counted as more poetic still than *avne blistra'otav*; not necessarily on account of the stylistic markedness of the words as such (where there is hardly any difference), but certainly in terms of their position and functions in the line. Such a formulation makes it possible to establish a local sound-pattern of the neutral type.<sup>4</sup> 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*:

*xitsE GORAI AxzAR || AvnE MARGEMOtAv*

In fact, very few of the sounds of the line do not partake of the pattern, whose main, if not only value is ornamental. Such a sound-pattern is bound to be highly noticeable precisely when the monologue is being recited on the stage.

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 dominant Hebrew poetics of the time, in original writing and in translation alike. Of course, phenomena catering to this need can be (and have been) observed in the final version too, but only an ordered comparison of the successive phases of the emergence of the translation 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:

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4. 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).

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: *tsa'ar ha-(a)hava ha-nixzava* → *tsa'ar ah(a)va nixzevet* → *ke'ev ah(a)va nixzevet* → *xavle ah(a)va nixzevet*, and, at the end, *AtSEVET Ah(a)VA nix-ZEVET* (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-šAIITIm* || *ve-xOl habi-ZyOnOT*

#### 4. Conclusions and implications

All in all, we have been able to uncover six major constraints to which Shlonsky subjected himself during his translation and which account not only for the final version of the monologue, but also for Shlonsky's interim decisions and many of his successive revisions:

1. To operate within a one-line 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 by way of sound orchestration.

There can be no doubt that what all these constraints have in common is the fact that they primarily reflect the interests of the *recipient* culture at that particular time.

Expanding the analysis to bigger portions of the monologue will no doubt yield additional constraints. It may also modify the ones we have already come up with and expose the relationships between them.

As already hinted, what we have manage to conclude from our findings should not be taken to imply that Shlonsky – or the culture in and for which he was translating – attached no importance to how the source text's features were

reconstructed, first and foremost its semantic features. On the contrary. An aspiration towards a certain degree of adequacy should therefore be added to the 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 larger number of the remaining criteria, all of them ostensibly target-oriented – even if this involved bypassing alternatives which would have been more adequate. Be that as it may, the result is a translation whose *general* character (rather than this or that detail within it) differs from the source text's – and in a direction which indeed contributes to its acceptability into the target culture, first in the theatrical, then in the literary domain.<sup>5</sup>

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(s) he actually proceeded from as a translator appears to be rather marginal. Intriguing as it may be, the conclusive identification of that text, or those texts, would change very little in our understanding of the process whereby the translation apparently came into being.

Finally, a similar account of the constraints which Shlonsky seems to have adopted in his Hamlet 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, and also because this kind of investigation teaches us not only what the translator did but also some of the things he considered doing (and some things he never did).

Coming back to Ivir's characterization of the process of translation, which I used as an example of a speculative model of the act of translation (Chapter 11, Section 3), it is clear that this time, our reconstruction would supply only *partial* verification, at best. The difference between the two cases, the Hamlet and the Remarque one, may have to do with a multitude of factors:

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5. 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). These translations originated in the same school, and were hence produced within a very similar set of norms.

- the source and target languages and their relationships, in purely linguistic terms as well as in terms of power relations, previous contacts, etc.;
- the difference in text-type (novel vs. drama);
- the difference in status and internal tradition between translation into Hebrew and translation into English;
- Shlonsky's authority vs. Wheen's relative marginality in their respective cultures,

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.<sup>6</sup> 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., Think-Aloud Protocols – and vice versa.

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6. 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 all texts are 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.<sup>1</sup> There is thus at least one issue of paramount theoretical, methodological and practical implications which tends to be overlooked, namely, the treatment which 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, 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 this chapter will be what translation-specific lexemes can imply, how the intended meaning of such items is to be determined, and what implications all this

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1. See, e.g., the Proceedings of an international conference wholly devoted to “Translation and Lexicography”, which brought together many of the ‘big names’ in the two fields, a rarity in itself (Snell-Hornby and Pöhl 1989). A preliminary version of the present chapter was in fact presented as a paper in that conference and published in its Proceedings (pp. 45–53). It was markedly out of line with the remaining contributions and was hardly ever referred to. A different version of the paper, which fared a little better, was included in Tomaszczyk and Lewandowska-Tomaszczyk 1990:287–300.

can have for the possible representation of translation-specific items 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 intended to serve communication in the TL. To judge from the lists of sources which the larger dictionaries often include, this practice has enjoyed some currency mainly in situations where translations have indeed carried particular cultural weight, which is quite understandable, given that every dictionary is just a *selective* documentation of a lexicon.

Thus, it should come as no surprise that of the sources cited in the *New Hebrew Dictionary* (Even-Shoshan 1966–1970), almost ten per cent (!) 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 incessantly, and in many dictionaries of English not a single translation is mentioned among the sources. It is not surprising that in historical dictionaries of these languages, translations are given more weight. After all, most cultures have had periods in their histories when translations fulfilled important functions, among them the introduction of new lexical items, or new uses of older 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.

As was already claimed, the fact that translations are intended utterances in TL should not be taken to imply that each translation represents a straightforward instance of performance within the boundaries of that language, let alone fits in with any of its institutionalized varieties. On the contrary, it is a well-documented fact that in assumed translations, linguistic forms and structures often occur which are rarely, or perhaps even never encountered in utterances originally composed in that 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 keep certain aspects of the corresponding source text invariant, which is a strong target-external constraint on its establishment (recall the Relationship Postulate, Chapter 1, Section 5). This constraint is of course stronger the more the source text is tackled as an organization of lower-level entities rather than as a holistic whole, and source-text interference is indeed an important source of forms which clearly deviate from general TL patterns, even though by no means an exclusive one.

Whether deliberate or accidental, occasional cases of so-called ‘translationese’ can be expected to appear in translated texts. However, it is quite possible for this phenomenon to undergo some degree of institutionalization. Thus, a number of translators may adopt the same norms and behave in much the same way, producing translation replacements of a similar kind. In the long run, translationese that becomes habitualized may even acquire certain markers which would be distinct enough to set it apart from any other mode of TL use, be it translational or non-translational. In fact, the more noticed (and accepted) such a differentiation is, the more justified one would be in regarding translationese as a **distinct variety of TL**. As Pedersen so aptly put it, “translation itself helps to create its medium” (1983:7).<sup>2</sup> There may of course emerge several non-identical varieties of translationese within a given language, a major distinguishing factor being a regular association of each variety with a different SL. 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.

## 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 a language variety used for the formulation of translations is most conspicuous. The obvious implication is that not only habitual TL lexemes have been used, but also items which do not pertain to the generally acknowledged repertoire – mostly new **combinations** of previously existing lexemes (i.e., deviant collocations), but, on occasion, newly coined **words** as well. Most of the word entities will probably

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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.

realize possible TL items, in terms of phonology, morphology and grammar, but even this is not an absolute must. Greater deviations from predominant patterns have been shown to occur.

Extreme examples of translation-specific lexical items would be complete innovations combining e.g. a major part of the source item's phonetics with a possible morphological structure within TL; for instance, the possible Hebrew word *élef* (for German *Elf*) or *tarit* (for German *Torte*), which were not even derived from existing Hebrew roots.<sup>3</sup> 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 Κοιρογυλλιος: To be sure, there is no knowing what the translators had in mind, because this Greek word (if indeed it was a habitual Greek word!) does not occur anywhere else. (See *Encyclopaedia Biblica* VIII [1982]: 251.)

A more common practice would be represented, e.g., by the Hebrew phrase *šexar zangvil* (literally: 'a carbonated soft drink | flavoured with ginger'), which – in the nineteen sixties and seventies – was very rarely encountered in translations from languages other than English, and even less in non-translated texts. It is not that it couldn't have occurred in original texts too; it is only that (a) factually, it was much rarer in them, and (b) many readers would have regarded a text using it, correctly or incorrectly, as a translation from English (especially if it also had some other words and phrases of the same variety of translationese). This cannot be attributed solely to the fact that there hardly arose a need to refer to the object itself, as the drink itself was virtually unknown to speakers of Hebrew texts. As concerns *šexar zangvil* the situation remained unchanged for quite some time. The English-Hebrew phrase went on being used in translated texts with no appreciable change even when the drink had become part of Israeli material culture, and as such was known to contain no alcohol (i.e., not to comprise any *šexar*), and when – in other modes of language use, especially on the bottles themselves and in menus – it was referred to almost exclusively as *šingjer eyl*.

Then again, lexical items occurring in translations, which have a place in the TL 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 exactly in their habitual functions, semantic, stylistic and/or pragmatic.

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, *élef* was taken from such a list.

For instance, in translated utterances (again, mainly from English), the biblical word *na'ara* occurs much more densely and in a much wider range of contexts and functions than it does within 'authentic' Hebrew texts, where it is mainly used to refer to a feminine teenager. It is rather simple to show that, in many cases, translated Hebrew has simply taken up some of the functions of the English 'girl', especially when it served as a constituent in an array of fixed expressions. A small sample will suffice:

*na'arat telefon* [a prostitute hired by telephone], 'call girl'  
*na'arat zohar* [a woman whose lifestyle or appearance is considered glamorous by popular standards] 'glamour girl'  
*na'arat koleğ* [a female student] 'college girl'  
*na'arat mesibot* [a female who likes to party] 'party girl'  
*na'ara ovedet* [a female prostitute] 'working girl'  
*na'arat ša'ar* [a female model whose picture appears on magazine covers], 'cover girl'  
*na'arat tasrit* [a member of a film crew responsible for maintaining the movie's internal continuity], 'script girl'  
*na'arat Bond* [one of the sexy, seductive women featuring in a James Bond movie], 'Bond girl'

In all these examples – and there are dozens of the kind – the persons referred to as '*ne'arot*' [the plural form] are not necessarily teenagers. Indeed, only seldom is this the main semantic feature here, although the feature [+ young] (or [+ inexperienced]) does play a role in fixing the meaning of an expression.<sup>4</sup> In the last few decades, some of the Hebrew phrases containing the word *na'ara* have started penetrating non-translated Hebrew as well. However, the mechanism for producing new phrases of this type in texts other than translations from English has never become truly creative (i.e. unmarked).

Translational items may also fulfil similar functions, even resembling their non-translational counterparts in terms of 'meaning', but the **distributional patterns** they follow are quite different.

To come back to the *na'ara* example: the distribution of this word in texts translated from English is much denser than in original Hebrew texts, not only when it occurs in fixed expressions but also when *na'ara* is used as a stand-alone word. Even 'girlfriend' [a close feminine friend of a person of either gender], which in

4. As could be expected, the American movie bearing the title "Working Girl" (1988) was entitled in Hebrew *na'ara ovedet*. It might be recalled that, within the film, the title character celebrates her thirtieth birthday. Also, she is a secretary, which may be rather misleading for the Hebrew viewer.

original Hebrew utterances normally appears as *xavera*, is now often replaced by *na'ara*. This phenomenon is most conspicuous when the noun is used in a declined form: *na'arati*, *na'arato*.

In the lexical domain, figures and percentages are of enormous significance. Fortunately enough, most phenomena have become much easier to quantify than ever before, first and foremost because of the availability of computerized corpora, which have become a powerful tool for descriptive studies in the last few decades. To be sure, there is nothing revolutionary here in methodological terms. In fact, hardly any new questions that couldn't have been asked before are being asked now. However, the possibility of processing very large quantities of text in a very short time is a huge plus for tackling old topics such as the similarities and differences between translated and original texts.

Thus, using an initial parallel corpus of original vs. translated Hebrew of about 750,000 words each (very small for a computerized corpus, huge in comparison to any manual collection of data), prepared by Noam Ordan,<sup>5</sup> it was possible to ascertain that the stand-alone word *na'ara* occurs almost twice as often in the translated sub-corpus (22 times than in the original one (12 times)).<sup>6</sup>

By the same token, but on the basis of smaller corpora, 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.

One area which is relatively easy to isolate for study purposes is that of Void Pragmatic Connectors. In a manual count of a few thousand of words I have been able to establish (Toury 1977: 79–81) that not only the very occurrence of Hebrew Pragmatic Connectors such as *ho* or *u-vexen* in translations is much higher 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'.

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5. I wish to thank Noam Ordan and Miriam Shlesinger for permission to access the corpus and use the data that can be extracted from it.

6. There is now an abundance of books and articles about the use of computerized corpora in Translation Studies. See, for instance, Baker 1993; Laviosa 2002.

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 nineteen sixties and seventies, it was almost identical to the distribution of 'o(h)' and 'well' in the corresponding originals.

As a check-up on my manual calculations I ran another search on Noam Ordan's comparative corpus which yielded the following results: *u-vexen* – 44 occurrences in the translated corpus as against 19 occurrences in the original one; *ho* – 11 occurrences in the translated corpus, none in the original one.

As I was unable to check the source texts, there is only one thing I can say about the paired couples: statistically speaking, what could be called the 'typical' (or 'average') pair are 'o(h)' + *ho* and 'well' + *u-vexen*. We should however be cautious and add the following reservation: only in marginal cases would our finding be tantamount to the claim that each English 'o(h)' was replaced by *ho* and each English 'well' – by *u-vexen*. It may well be that some instances of these items were replaced by other entities and that some of the *u-vexen*-s had different sources. This is where the time axis comes into the picture. Thus, there is also the possibility that items figuring in both translations and non-translations may nevertheless occur in them *at different points in time*; the translational use may come first (in which case it may turn out to have functioned as an innovatory force) or it may lag behind (in which case translation may well amount to a conservatory force).<sup>7</sup>

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 biblical word *šexar* [an intoxicating drink] 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, *bíra* is now the normal word. In fact, *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.

7. 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, see again Even-Zohar 1990: 45–51 (1978).



### 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 replacement of an individual translator to a particular item in a single text in one particular language. Being a **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 for any unique change of function or use of an existing item (which is often regarded – not always justifiably – as a mere ‘error’). However, in the course of time, such new items and changes of meaning and function may be picked up by a growing number of translators, either independently, responding in a similar way to certain SL stimuli, or even following in each other’s wake. In the process, these items gain in habitualness, if only as translational replacements, and may become part of the norm. To be more precise, as long as its association with a fixed lexical item in a particular language has not been resolved, such a lexeme would still pertain to translation from that language rather than representing ‘communication in translated texts’ in general.

Rather than its (non-existent) overall validity, it is the fact that it has ceased to be a bona fide instance of performance that earns it lexicological attention and merits a lexicographic account. It would simply not do to leave habitual(ized) translation-specific items outside any dictionary aspiring to a degree of exhaustion and precision in terms of discriminative capacity. On the other hand, to require that a lexeme represent TL ‘as a whole’ is a highly dubious condition for granting it lexicographic treatment, which only modes of language use that 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 the total disqualification of translations as sources of lexical, or any other linguistic data, even in cases where they did introduce novelties into TL. This practice would run counter to simple logic: an item of translationese must be part of the language (and only that language) to which the utterances containing it claim to belong.

At the same time, if and when these items are included in a dictionary, the fact that their distribution and use are restricted should not be ignored, let alone obscured. It would simply not do to regard those translations which make it into the lexicographer’s data-base as if they represented TL 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 angle. The restrictions on 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 acquired?**

#### 4. The ‘meaning’ of translation-specific items

One convenient way of approaching items revealing any kind of specificity is to refer this 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 for use 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 – will form 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.<sup>8</sup>

According to John Lyons (1977: Section 9.7), who tried to introduce some order into the use of the pair of notions ‘markedness’ and ‘unmarkedness’ insofar as it applies to the analysis of the lexical structure of a language, there are three interconnected though theoretically “disparate and independent” senses in which a lexeme may be described as ‘marked’ or ‘unmarked’, and hence three different types of markedness: formal, distributional and semantic. Although he never refers to it in any explicit way, it can be assumed that Lyons would have subsumed translation-specificity under **distributional markedness**, the necessary and sufficient condition for which is that the marked member of a pair should 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

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8. 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 14, Section 2.2.

specific effect (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 suppose that this opposition had been completely neutralized). By contrast, items which are marked as translational cannot be used in non-translational contexts without this source of 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 way the utterance in which it is embedded actually came into being. 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 by passing 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 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.<sup>9</sup>

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 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)

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9. A good example would be Hemingway’s use of the English word ‘much’ (and a number of other words) in *For Whom the Bell Tolls* – e.g. “this is much horse” or “Thou art much woman” – in order to evoke an underlying ‘reality’ in another language; in this case, Spanish (e.g., Fenimore 1943). For a systematic presentation of the whole issue of “Poly-lingualism as Reality and Translation as Mimesis” see Sternberg 1981.

It is in this connection that the quite regular tendency of translation-specific lexical items to replace particular items of another 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 TL-intended replacement of a given item of a given SL.**

It is fairly obvious that, in this extreme formulation, such an assertion would only hold for very few, and very extreme cases – 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’ TL 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 means that there would always be, at the very least, a measure of doubt. Naive users of TL 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 making, 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 general TL lexemes, the pairing of these items and their correspondents in texts in other languages emerges as mandatory for the establishment of the information needed for their lexicographic 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 TL and to no other language, it should have become clear that the properties of those items 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 TL patterns resist explanation, at times even defy 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 will first have to be ascertained, 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.<sup>10</sup> The only feasible solution seems to be to remain within DTS and develop 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, and hence between translational and non-translational modes of text production and language use, has been conceived of as a basic systemic (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 we are after. What is of major consequence for lexical studies of translation is the assertion that in a study carried out within DTS, a TL item does not merely imply the existence of a corresponding SL item, but the two determine each other as ‘replacing’ and ‘replaced’.

Proceeding from the target pole, the analyst, hitting upon a TL segment which recurs in translated texts, thus marks it as a tentative lexical item, considering it against the background of TL ‘in general’ and/or the patterns that are

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10. 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 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 what we have been interested in.

bound to emerge from the mapping of the translations on their assumed originals. What s/he then looks for is the SL segments which pair with the tentative TL lexeme, whether similar to it in rank and scope or not, always keeping in mind the ‘no leftover’ condition (see Chapter 6). 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 TL segment as a replacing member. It is the recurrence of these pairs in larger corpora that will supply the lexicographer with the necessary 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 SL 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 only/mainly/also || in translations into TL || 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, like most of the other restrictions treated in a dictionary; for another, it bears on the very meaning of the item (or at least on the information conveyed by it).

The adoption of such a policy has much to offer in terms of increasing the fullness as well as the discriminative capacity of a dictionary. It is therefore bound to put Lexicography on much firmer theoretical and methodological grounds, for it enhances not only its descriptive capacity, 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 in 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 final examples.

1. The Hebrew noun-compound *tazvig mikrim* [literally: paired occurrences], which, for the first 30 years of my life (or so) I have encountered only as a translational replacement of the English noun ‘coincidence’ in the texts of one particular translator, Eli‘ezer Karmi (1918–1991), one of the most prolific translators of the time, and never as a replacement of any other word, in any language, let alone in Hebrew original writings. Only later was this phrase picked up – presumably by readers of Karmi’s translations, some of which enjoyed great popularity – and given some more latitude. It did however remain quite rare, and after Karmi’s death it seems to have practically gone out of active use.
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 colour term *violet*.

These two examples differ considerably in terms of the relationships that tie each one to its habitual counterpart in the respective SL.

Thus, *braun und blau* hardly reveals any sign of formal correspondence with, or negative transfer from *violet*. Rather, an existing German phrase, which used to mean ‘[mit] schwarzblauen Flecken’, ‘finster’, ‘dunkel’ (e.g., Grimm II (1860): 324–325), was adopted as a carrier for a new semantic function, one that the German language had not recognized, 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 SL carrier, and the latter were replaced by some corresponding TL substance. (The first constituent of the phrase, the word *tazvig*, was a complete neologism, derived from the existing Hebrew root *zvg*, which underlies a number of existing Hebrew words (e.g., *zug* [pair, couple], *zugi* [dual], *ziveg* [marry off, mate, couple], *hizdaveg* [copulate]). It was offered as a replacement of the original prefix *co-* with which it shared the semantic feature of ‘duality’, and combined with the productive nominal pattern *taf’il*. 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 *tazvig* – unlike

the compound as a whole – would warrant an entry of its own in a Hebrew dictionary.)

Despite these differences, and despite the fact that they are well-formed in terms of their intended 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 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 what the child observed became “ganz braun”, thus making use of an unmarked German term *braun* for that colour and modifying it. As Suzanne Öhman put it in her study *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 monolingual dictionaries of Hebrew and German (respectively) that do 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’ or of *tseruf miqrim*, just as it would be erroneous to claim that, at the end of the 18th century, *braun und blau* simply ‘used to mean’ ‘violet’. Reducing the representation of the two expressions to such a simplistic formula would mean that the lexicographer would actually be **creating** linguistic realities rather than merely **documenting** them.

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.<sup>11</sup>

But have our requirements 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 programme seem to lie in the practical domain, and in the electronic age even these seem to become less and less valid by the day.

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11. 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.

## Experimentation in Translation Studies

### Achievements, prospects and some pitfalls

In spite of the rapid evolution of Translation Studies towards semi-independence, the majority of the researchers in the field, especially of the older generations, have had their training elsewhere. In fact, *grosso modo*, Holmes' 1972 claim is still a fair account of the situation:

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 major change seems to be that attempts are being made to apply to the study of translation paradigms and models imported from ever new disciplines.

The introduction of experimental methods is no exception: it can hardly qualify as an internal evolution within Translation Studies itself. Not even the *impetus* for applying such methods 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 had encountered elsewhere – areas which would be close enough to their mother disciplines as well as sufficiently underexplored to warrant study with the help of those tools. No wonder, then, that many outsiders chose to apply them to translation, which indeed satisfies both needs. All this notwithstanding, the introduction of experimental methods proved a true landmark, and it certainly looks like they are here to stay, providing the necessary resources – money and (wo)manpower – can be secured.

To my mind, the greatest contribution of experimentation lies in its potential for shedding a different light on the interdependences of all factors which [may] constrain translation and on the effects these interdependencies may have on the process, its products, and the functions they may serve in the recipient culture, and in increasing their predictive capacity. This potential derives from two traits inherent in experimentation: the relative *controllability of variables* and a high

rate of *replicability*, which also ensures easier *comparability*. At the same time, most of these new methods have only just begun to align themselves alongside other achievements of today's Translation Studies, with a view to increasing their relevance to our understanding of just what translation involves. And this is basically what this chapter sets out to do.

### 1. Empirical sciences and empirical methods

Like any other empirical science, Translation Studies can go a long way with no recourse to empirical methods. However, the very possibility of the introduction of such methods to the study of translation (which is only feasible in sciences of the empirical kind) is bound to encourage their adoption especially in a period like ours, when the scientific scene is literally haunted by experimentation. This makes it easy to understand why empirical methods have started being imported to Translation Studies in spite of the fact that, as a non-prescriptive discipline, it is still in its infancy. In fact, I am inclined to take the very spread of experimentation among translation scholars as a promise of an accelerated movement towards a non-prescriptivist approach. After all, nobody would claim with any amount of seriousness that an experiment could show what translation should be, or even what it can be. Only what it actually is or, at most, what it is believed to be, can be subjected to experiments.

Whereas an empirical *study* 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", so that "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 investigated which have already been tackled 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, experimental studies first have to secure the data itself to which the questions will then be addressed. *Elicitation techniques* thus form an integral part of an 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 given a justification in terms of Translation Studies.

Within Empirical Translation Studies, product-, process- and function-oriented approaches differ not only in their focus, but also in terms of the kinds of data they elicit and process. Indeed, the crucial problem lies precisely in choosing

the right products – 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 these that 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 *emergence* of a translated utterance, often to the complete neglect of the final version.

Having said this, let us examine some of the experimental methods which have been suggested with respect to translation. Following the 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 was to submit translated texts to CLOZE TESTS, the good old technique which served to test two complementary issues at the interface of a text and its readership: the *reading comprehension* of subjects and the *readability* of texts (e.g., Key 1959; Nida 1964: 140; Nida and Taber 1969: 169–170). To be sure, it has never been made completely clear what exactly is measured by this method. Even less clear has its explanatory power been in terms of Translation Studies. After all, there can be no guarantee that the application of a cloze test to a text which just ‘happens’ to be a translation will have anything particularly ‘translational’ about it!

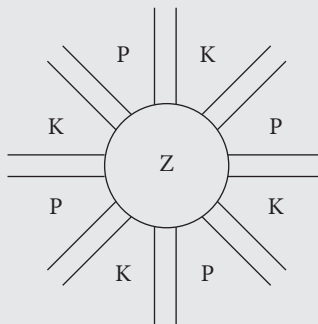
One way out of this deadlock may be to devise comparative studies into the readability of translations into a language vs. original texts of a similar nature in that same language. Studies of this kind are likely to yield interesting insights into similarities and dissimilarities in approaching and processing texts of either kind; and if the assumption 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 translations. (The crucial issue of the possible specificity of translations among TL texts will be taken up again in Section 2.2.)

Combined with other methods, cloze tests may yield even greater insights into the ways addressees process and translators construct translated texts. In this respect, the following multi-stage experiment, which has mini-cloze built into it, 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 13).



**Figure 13.** 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 felt 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* as either ‘like the rays of the sun’, or as ‘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 chose ‘a wheel’.

It would seem, then, that even people who are well aware of native situational equivalents, and use them in comparable native-like contexts, 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 an advanced 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 as well.

Another type of comparative study which could be carried out with the help of the cloze technique concerns *relative readability*, especially of different translations of a text into one and the same language.

A study of this kind was carried out by Tiina Puurtinen (1989a; 1989b) as a corollary of a linguistic analysis where one syntactic feature was isolated whose presence in a Finnish text is believed to reduce its readability. This assumption was indeed verified by a cloze test: a translation which had that feature in it was deemed significantly less readable than another translation, done in the same year, which did not have that feature; 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, Mr. 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 prevailing norms 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 to better 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 three parallel translated excerpts of one short story; one done in 1959 (but according to much older norms), the second one in 1985, and the third one was prepared by the examiner for the experiment itself. Here pains were taken to approximate the linguistic formulation to the one used in contemporary written texts in an attempt 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 method itself.

Finally, the *relative readability of translations and their sources* can also be tackled. This was actually one of the first suggestions made in the literature, and yet, to the best of my knowledge, the cloze method has never been applied in this way, at least not in any controlled manner. True, this is scarcely a great loss, given that the whole orientation of those 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., Key 1959; Van Hauwermeieren 1972)? Simply rephrasing it as a proposition of the type “the readability of a translation *is* equal to that of the original” would not do the trick. This would constitute, at best, a descriptive statement, applicable to one or another *individual case*, but it could hardly serve as 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 with a similar 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, because of the obvious, even though by no means simple correlations between explicitness and readability, the cloze method may 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 prevail, 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 these are marginal? etc., etc.<sup>1</sup> 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 the way to formulating conditioned laws.

Of course, the use of cloze to study the relative readability of translations and their sources involves some inherent difficulties. 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 in itself is not among the tasks of the translation scholar), the data collected would hardly lend itself to comparison.<sup>2</sup> It would be even more difficult to justify investigation into the relative readability of the *translations of one text into different languages*, which should, in principle, offer a possibility of discerning between what is universal and what is specific, or norm-governed, in translational behaviour.

Despite these difficulties, were I to foresee the future evolution of Translation Studies, I would not be surprised to find the cloze technique reintroduced into the field and tried out in each of these types of study.

## 2.2 The use of questionnaires

For many years, the most common way of studying aspects of translated texts (or, rather, addressees’ responses to them) in an empirical way consisted in devising QUESTIONNAIRES, and having groups of subjects – hopefully big enough<sup>3</sup> 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 just one, and to bring a few elements of another one 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 (and hence the justification for using those features as criteria in the assessment of translations).

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1. In recent years we have learnt a lot about some of these issues, but what we now know is not nearly enough.

2. 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.

3. Insufficient sample 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.



The experiment consisted in rating five Finnish translation variants<sup>4</sup> of one English text by 30 native speakers of Finnish on an intuitive basis. The rank order of the texts was to be established using 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 ten paired comparisons were computed into a group-to-text ratio, and the 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 allegedly carried out by the subjects – is involuntarily governed by rules which concur with those textlinguistic features that a ‘professional’ assessor would rely upon in a 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.

My interest here lies mainly in the *experiment’s design*, not in the results which were obtained. Therefore I won’t say anything about the textlinguistic features or about their status as ‘criteria’ for assessment 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* as such.

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.<sup>5</sup> Of course, there is nothing basically wrong with rating translations in terms of their acceptability. After all, I have insisted on it myself. The point is only that the kind of acceptability thus assessed is not altogether clear, much less controlled: is it the translations’ acceptability as *texts* in TL – or only as *translations* in(to) it?

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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.

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.

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 TL utterances and as translations into TL.<sup>6</sup>

Incidentally, as has been shown by many studies, the status of ‘acceptability as a translation’ holds true especially for cultures where translations play a role in shaping the very centre 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 a mere possibility, but it is something that really happens, which may indeed have crucial implications for the way the text would be interpreted; and if interpretation, why not assessment?

This experiment was carried out by a student of Shoshana Blum-Kulka’s at the Hebrew University (Jerusalem)<sup>7</sup> and replicated 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. This they all knew, but none of them was told which were which. Rather, it was precisely their ability to *identify* utterances – short pieces of dialogue, in this case – as either original (authentic or literary) or translated that was tested, along with how they would justify their decisions.

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

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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.

7. Shoshana Fishler. “Register and Translation Perception of Native Speakers”. A seminar 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.

or textual, but pragmatic ones as well. The perception of these shifts, say the authors, 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 said 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 had in common, 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.

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 a neutral factor in the assessment. Thus, it is perfectly possible that, in addition to being read as either utterances in TL 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 students 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 human sciences, 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, it still proved insufficient. What she did was divide her subjects evenly 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 the 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 hypothesis that was being put to the test; namely, that impressionistic assessment of assumed translations is involuntarily governed by a definable set of textlinguistic features. A solution to 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 of the subjects into two groups. So Tirkkonen-Condit applied Cronbach's alpha coefficient to measure the agreement among the subjects of each group, and 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 language 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 taking part in the experiment, but mainly because no one as yet has any clear idea as to which variables could be of [higher and lower] relevance 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 have been established, each consisting of members of *both* original groups, which might 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 these small pitfalls, I do not wish to 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.<sup>8</sup>

### 3. Process-oriented empirical studies

Of greater significance still is the application of experimental 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 new vistas.

Of course, it is not as if there have never been speculations about 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 (Holmes 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, Lörcher 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 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' has been achieved through the analysis of those stages in the emergence of individual translations which were realized and fixed in writing (see Chapters 11–12 above). What we have here is a series of *interim* outputs which, if put in a correct order, could be compared not only to the input of the process, but to each other as well, thus yielding insights into certain aspects of the process, e.g., the way translators replace one tentative solution by another, or keep changing the units they are working with, in terms of size and level alike. Even though processes other than pure translating

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8. In this connection, the reader may also wish to study the elaborate, though by no means non-problematic way in which questionnaires were used in Stegeman 1991.

may be involved in the gradual emergence of the translational product, this type of study (which, admittedly, is descriptive proper, not experimental) 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 worked out on speculative grounds only. What these process studies were 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 endorse.

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, so to speak – 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 have also been investigated (see Tommola and Hyönä 1990). However, the method which gained wide popularity in the eighties and nineties is the technique of Think-Aloud Protocols, TAPS for short.

### 3.1 Think-Aloud Protocols

Think-Aloud Protocols have been used for the collection of introspective data on translating in several, slightly different ways. However, here 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 are transcribed, and the running transcripts submitted to meticulous analysis.<sup>9</sup> The claim is that this method

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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 (*continued on next page*)

allows particular analyses of the levels, steps, units of processing, the role and the interaction of the [source] and [target] language,<sup>10</sup> the amount of proceduralization, the origin and course of search processes, and the times used for these processes. (Dechert and Sandrock 1986: 115)

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 countered successfully. In fact, it has even been 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 relevance 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 different types interfere with each other. 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* which become involved in the act.

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* – that is, linguistically formulated translation of another kind – before the required written one; and there is a real possibility that spoken and written translation do not involve exactly

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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.

the same strategies, regardless of whether the differences reflect the two different modes as such or differences in the norms underlying and affecting them.

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 even if it is just being read aloud), which would make his/her task more compatible with the normal application of TAPS.

A remedy would not be found in simply claiming that the results should be taken to refer to just *that* process, which is a kind of hybrid: ‘spoken-written’ translation. To the extent that such a hybrid has any existence in the world of our experience, it is at best marginal, notwithstanding the observation (Kring 1987: 166) that some translators sometimes do betray certain external(ized) symptoms of ‘inner speech’. Its relevance for the establishment of a general ‘psycholinguistic model of translation’ (which is often presented as the ultimate goal of the use of TAPS [e.g., Kring 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 respective end products. 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 Think-Aloud Protocol.

Trotz	der	Tatsache	daß	es bestimmte Unterschiede gibt						
In spite of	the	fact	that	there are certain differences						
Trotz	der	Tatsache,	daß	es zwischen britischem und amerikanischem						
zwischen	britischer	und	amerikanischer	Aussprache	und	Wortschatz,				
between	British	and	American	spelling	and	vocabulary,				
Englisch	bestimmte	Unterschiede	Hinblick	auf	Schreibung	und	Wortschatz			
gibt,	ist	Standard-	Englisch	—	das	geschriebene	Englisch	in	Büchern	und
Standard	English-	the	written	English	of	books	and			
gibt,	ist	Standard-	Englisch	—	das	Schriftenglisch	in	Büchern	und	
Zeitungen,	ist	ziemlich	das	gleiche	in der ganzen	Englisch-sprechenden	Welt.			
newspapers —	is	much	the	same	throughout the	English-speaking	world.			
Zeitschriften —		überall	in der	englisch-sprechenden	Welt	weitgehend	gleich.			

Figure 14. An English text accompanied by two German translations



In Figure 14, 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 share. Thus, the top translation manifests a much larger *number* of segments, coupled with an average small *scale* and low *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 TL replacements show traces of SL *transfer*, some of them even being 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 where the text was used as a translation exercise.<sup>11</sup> 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.<sup>12</sup> In my fervent search for interdependencies I would even venture to hypothesize that the two variables may be *correlated*, in the sense that the need to perform orally may bring experienced

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 same person – may 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. These are two contending concepts of one type of activity, to be sure, rather than two different activities, a translational and a non-translational one!

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.<sup>13</sup>

These are all untested hypotheses which still need exploring. However, I do believe it is about time that the application of TAPS stopped being oriented towards the establishment of a general 'psycholinguistic model' alone, aiming to test smaller-scale, and hence more specific hypotheses as well. Needless to say, any in-depth study of this kind is bound to have positive implications for attaining the ultimate goal too, which is all the more reason to turn to it, for theoretical and applied purposes alike.

Thus, some differences of performance along the 'inexperienced-experienced' continuum have been substantiated on the basis of TAPS as well (e.g., Krings 1988; Tirkkonen-Condit 1989; Jääskeläinen 1989; Lörscher 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 so-called '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 much more conscious processing, so that competence in 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 basically psycholinguistic), or does it involve the internalization of external feedback as well (which may entail switching the focus to socio-cultural factors,

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13. For the interpretation of translational shifts in terms of the oral-literate continuum see, e.g., Zeller Mayer 1987; M. Shlesinger 1989a.

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 be elaborated on in 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 shifted from ‘testing’ to the direct object ‘something’. The point is that many of the experimental methods which have so far been applied to translation could be characterized precisely by a degree of uncertainty as to what they are supposed to do. The questions underlying them were often overly general, bordering on the vague, and those who used them 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, (Hilpinen 1988: 16)

in terms of both “informational completeness” and “error avoidance” (ibid.). 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 tend to regard any transfusion of fresh blood as highly beneficial for a discipline such as ours, which has long been bogged down in re-inventing old wheels. At the same time, it may well be the case that the introduction of experimentation to Translation Studies was somewhat premature. Not 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 (which is no doubt there), 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 effect': the tendency to be biased towards facts that strengthen certain hypotheses and views, while playing down those which are indifferent to them, let alone weaken them.

In the last decade or so, things seem to have undergone some change. Translation scholars have learnt to formulate more and more small-scale, and hence more easily testable hypotheses within the discipline itself (rather than importing them wholesale from without and projecting them onto translational phenomena).<sup>14</sup> It is to be hoped that the impact of empirical studies will be more noticeable too, contributing to the establishment of a full-fledged, multifaceted theory of translation of a high explanatory power, with some predictive capacity as well.

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14. In this connection, see also the important theoretical and methodological discussion offered by Gile 1991.



## A bilingual speaker becomes a translator

### A sketch of a developmental model

A prerequisite for becoming a socio-culturally significant translator is gaining *recognition* in this capacity. Thus, the identity of a person as a translator is *granted* rather than *taken*, which also means that it should first be *earned*. The implication is clear: a central part of the process of becoming recognized as a translator consists in the acquisition of the norms favoured by the culture that would be granting that status. The crucial question is, of course, how the transition from having a measure of bilingualism (a minimum requirement) to being a fully-fledged translator comes about, first and foremost in a ‘natural’ setting, that is, 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 what he called **natural translation**, i.e., “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).<sup>1</sup> Unaware of this proposal, I myself put forward, a few years later, a seemingly similar notion, that of **native translator** (Toury 1980b).

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 notion. For one thing, translation itself 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 programme would be expected to produce utterances which would not differ in essence from those that have come to be associated with

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1. In 2009 Harris launched a blog which is wholly devoted to “Natural Translation, Native Translation, Language Brokering and related topics” (<http://unprofessionaltranslation.blogspot.com>). “Language Brokering” is a relatively new concept normally covering activities “conducted in an immigrant family setting or between family members and the community”. I am far from convinced that there is a need for this added compartmentalization in our field.

translation, or even with ‘good translation’, in the community which those graduates would ultimately be serving. It thus seemed desirable that translation didactics aiming at the attainment of this object would base both goals and methods on translation *as it is*, and not on any prefabricated notion of what it should allegedly be like, which – irrespective of how it is formulated – is bound to be reductionist and to fail to pay due heed to essential components of native-like performance. At that initial stage, a ‘native’ translator was tentatively conceived of as one who has gradually grown into that cultural role, with no formal training for it, i.e., a crude counterpart of the much more common 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 a *societal environment* which entertains certain concepts as to what translation is and how it should be done. Meanwhile, Harris has not been sitting on his hands either: he too 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 and for the need to study it.<sup>2</sup> More important still, he made a few initial attempts to look more closely at certain points along the acquisition of translation skills among a particular group of subjects: 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 completely independently but also designed to serve different purposes, are symptomatic of the welcome change of climate 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.<sup>3</sup>

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2. Harris 1988c is an annotated bibliography on natural translation and related notions until 1987. See also the 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).

3. It is interesting to quote, in this connection, a pioneering formulation by the Bulgarian scholar Alexander Ljudskanov: “Kraft einer bestimmten Intuition und einer Gewohnheit 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).

In an attempt to advance the treatment of this issue, the relationships between the two seemingly similar notions will now be examined, together with 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, I will address the notions as they have been put forward thus far and 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. I will be the first to admit that the actual testing of the model is still wanting.

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 practised natural translation, were presented and analysed. One objective 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).<sup>4</sup> 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 occurs.
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) outside it as well (extrafamily transduction).

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4. 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).



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 analysed in the article the circumstances are simply not included in the account, and those that are referred to, in the case of one or another individual, are often incompatible with each other. The point, however, is that the authors have neglected even to raise the issue, as if it were of no consequence. What they did, in fact, was to collapse the whole array of factors which may figure in the development of natural translation into one single variable, that of *time*: first and foremost nominal age, maybe years spent in translating as well.

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 seem to represent such an incongruity, which undermines the identification of biological and bilingual age and casts serious doubt on the very selection of the former as an all-embracing factor.

In view of all this, I dare say that the establishment of a well-rounded developmental model, even for very young child-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 defence hardly requires the heavy guns of strictly *developmental* evidence.

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*, from oldest to youngest. Developmentally speaking, what they are mainly able to show in this mode of presentation is that translation at an 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 as “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 (on which cf. also Albert and Obler 1978: 218–219 and the studies referred to there). The qualitative leap would simply be impossible to account for. On the other hand, the identification of translating *as an actual skill* with mere bilingualism seems an unwarranted oversimplification.<sup>5</sup> After all, it offers no answer to the crucial question of what it is that brings forth the *unfolding* of the skill (that is, its realization in actual behaviour) and the way (or ways?) it then *develops*.

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5. 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 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).

The supreme test of a skill (as distinct from the mere predisposition to use it) is no doubt in the *performance*, and it would seem rather far-fetched to assume that all bilingual speakers (by virtue of their mere bilingualism, that is), or even all infant bilinguals (by virtue of the added factor of their young age), will indeed *activate* 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 no difference whether such performance would or would not concur with any particular conception of what translation ‘is’.) 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 it in motion – most probably, a certain combination of personality features 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 begins unfolding. It is thus 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 this lack), I would hesitate to subscribe to a second claim they make, 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, at a certain age, perform natural transduction and/or interpersonal autotranslation will have behind them, if they started translating 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 practised the spontaneous varieties will necessarily move beyond intrapersonal autotranslation and ultimately reach the stage of socially relevant transduction, especially if it is required that it also be done in a socially acceptable way.

The rift between the mere presence of a mental predisposition and its unfolding 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. To be sure, it was never motivation and function, but only circumstances and (lack of) special training that formed part of the concept, according to Harris and Sherwood.

In fact, once one agrees to follow the authors 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 *developmental* aspects vis-à-vis 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 redundant, a mere synonym for bilingualism (at least in infants), especially if one also joins Harris and Sherwood in postulating that translating *itself* (rather than the mere disposition for it) "is coextensive with bilingualism" (p. 155).

This is where the notion of **native translator** seems to be much handier, 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, whether in some internally organized succession of phases or otherwise. Rather than being regarded as alien factors to be suppressed and disposed of, at least on the level of expository argument, the importance of the social motivation for, and of the social functions of translating and/or its end products, should be acknowledged, and these factors should be assigned their proper place within the concept itself and the developmental model that goes with it.

Thus, it is my contention that, whereas the **predisposition** 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 translational behaviour and/or its results.<sup>6</sup> What the developmental model we endorse thus involves is a consequential extension of the notion of norm from the translational performance of an individual to the very acquisition and development of the skill.

### 3. The making of a 'native' translator

Translating proper, even when 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 feedback, which is bound to act at least as a kind of post factum product-monitoring device, may come not only from the recipients of the translated utterance, but also from those who have commissioned the act, and sometimes from the originator of the utterance to be translated as well. When realized by actual persons, these roles (in the sociological sense) may, of course, partially overlap. At the same time, they never coincide in full, or else the act loses its motivation and becomes communicatively redundant.<sup>7</sup>

The feedback a translator receives is of course *normative* in its very 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 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 is supposed to remain 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.

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6. 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.)

7. Even according to Harris and Sherwood's account, pretranslation and intrapersonal auto-translation 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.

It should be recalled that the notion of norm involves that of *sanctions*. It is the (real or assumed) presence of the latter which gives the 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 well as obtain the rewards which go with 'proper' behaviour. This attitude holds especially for the novice, who – owing to lack of both sufficient experience and a defined position in the culture – is likely to feel insecure as to what constitutes translating, according to the conception of the societal group in and for which s/he would be starting to fulfil the role of a translator. 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 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, there will always be more than one option. The only way is to have the culturally acknowledged criteria supplied from without, namely, by those who already have, believe they have or are believed to have them. They are powerful enough to impose their will because of the *authority* they have been granted in and by the target culture.

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 externally-motivated monitoring device, to the extent that the translator immediately responds and revises his/her production accordingly. S/he may even rephrase what s/he has already formulated, or, indeed, change his/her entire translational strategy. Little by little, however, translators may start taking into account *potential* responses too, e.g., by imagining an audience of a particular kind. They thus develop an *internal* kind of monitoring mechanism, which can operate on the (interim) product as it is coming into being as well as on the very act of translation.<sup>8</sup>

Socio-culturally speaking, what emerging translators thus undergo is a process of **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 certain **blend of nature and nurture**, of the humanly innate, the individually assimilated

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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.

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 translation competence, it will also be applied to the production of more spontaneous translated utterances, in situations where no sanctions are likely to be activated from without. When analysed, 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 features 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 whatever conception of that mode of text production is pertinent to a particular societal group. As we already know, this conception may differ from one group to the next, in place, time, social class and various other respects.<sup>9</sup> 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, because of those features which are universal to mediation as such, and a great many other components which seem to be relatively easy to transfer or modify.<sup>10</sup>

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9. To take an analogy from a completely different area: a 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 unambiguous: “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?! Needless to say, the bigger the distance between two cultures, the bigger the gaps between their respective medical sectors are likely to be (for which see, e.g. Fadiman 1997), again, in full analogy to translational behaviour. Here too, attempts to manoeuvre between the two sources of norms involve interference as a basic strategy.

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,

The nature of translation competence as it is acquired in real practice may also have much to do with the exact types of tasks one is called upon to perform. Thus, bilinguals undergoing socialization as translators also develop strategies for coping with specific types of issues that are likely to recur during actual translation. In extreme cases, they may develop automatized ways of handling specific phenomena, even accumulate a stack of fixed solutions which are mobilized whenever a particular problem occurs. To be sure, what this often offers is an efficient way of *evading* a problem rather than plunging headlong into it, thus leaving more time and reserving more energy for less proceduralized parts of the task.<sup>11</sup> 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 mastery of one or both of the languages, 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*.

In fact, 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).  
(Touy 1986b:82)

including institutes for translator training. Unfortunately, it is often the case that, when going out into the 'real world', graduates of translation programmes 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.)

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 sensitized to new aspects of the task which require conscious decision-making" (Jääskeläinen and Tirkkonen-Condit 1991: 106).



To the extent that transfer is indeed disfavoured, the relevant 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 distinguishable from normal TL production. In fact, the interference they entail may not become evident until the translation is confronted with its source. However, when the translator has assimilated the anti-transfer norm and made it into an *internal* monitoring device, it may well be activated not only post factum, i.e., 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 TL, a process of *generalization* may be said to ensue whereby the internalized monitoring device is gradually applied to transfer *as a strategy* rather than its surface manifestations. (The fact that such monitoring is indeed operative has been demonstrated, for 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 11.)

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 developmental data of individual translators (except maybe introspection as to how I became a translator myself). Such data are simply unavailable, certainly not in any ordered form. Instead, I resorted to the comparative use of translational performances 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 related to the number of segments into which an SL utterance is decomposed for the purpose of its translation (which is known to decrease with monitored experience, all other factors remaining unchanged) and with the average scale and rank of the segments themselves (both of which increase considerably).<sup>12</sup> Transfer – whether negative or positive – 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 is at stake here is *adaptability*,

12. See also the passage analysed in Chapter 14, Section 3.1.

that is, an ability to adjust to changing circumstances 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 (depth being substituted for breadth). Different blends of specialization and adaptability are no doubt the rule here, and the implications of different blends make another issue for empirical studies.

All in all, the more advanced the developmental stage one is in, 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 acting *against* normative pressure (if so one desires) with no more 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* of norms for others to follow, even in the target culture as a whole. More than anything else, it is probably the *prestige* that goes with socially acknowledged professionalism that is at the bottom of such a licence. At the same time, there is no real contradiction between becoming a 'professional' and being 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 in society at large.

Be that as it may, in contradistinction to Harris' natural translation, where a distressing gap opened 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 socially acknowledged and culturally relevant translator.

#### 4. How is a developmental model to be validated?

The centrality of norms for accomplished translators seems to have 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, or even the acquisition of a habitus.

The data available in the literature, including the invaluable additions in Harris and Sherwood (1978), are of 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 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 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, let alone submit this to comparison.

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 interrelationships and the way they 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 studying the evolution of translation capacities than it has been found to be in 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 is somehow related to that instruction and to 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 other 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 programmes often have to undergo a painful process of *unlearning* much of what they have been taught, and adjusting, at least in part, to prevalent norms of socio-cultural appropriateness, among them norms that their teacher may have tried to obliterate or change.

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 an opportunity to abstract *their own guiding principles and routines* from actual instances of behaviour, with the help of responses to their performance which would be 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 outside that context.

What I have in mind is not a close imitation, in its entirety, of the process whereby a bilingual speaker becomes a 'native' translator. This 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, owing to the accelerated pace so typical of training programmes. Then again, a syllabus is simply much more focused, 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 follows from 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 those that count, are teachers, who, moreover, tend to adhere to very similar 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 be more intuitive, coming (as some of them no doubt would) from less professional agents. 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, 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, the students themselves should be encouraged to respond to their peers' performance, even to assess it outright. And students' assessments should be taken seriously in spite of their little experience, even taken as a basis for the final grade of at least some of their peers' assignments; and it should be made clear to everybody just how important peers' evaluation would be, so that they regard 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 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 these products will be received and function at the target end – and that their job is first and foremost to manoeuvre among these heterogeneous constraints. As a result, for every gain something will necessarily have to be given up. In other words, the 'We Know Better' stance, underlying the implementation of normative conditioning in translation teaching, should be replaced by an alternative slogan, namely: 'Everything Has Its Price'. Students would thus be trained to consider for themselves what stands to be gained by a certain decision made, what would 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 recipient culture.

Of course, the solutions arrived at in this way will often resemble those advocated by an external authority. After all, many teachers are acknowledged translators in their respective 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 yield ground 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 allegedly be adopted – is to have the students (and teachers!) analyse 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 related to their (intended or realized) positions in the target culture, and this with no immediate evaluative purpose in mind.

Such an 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 way of experimenting with models 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 old suggestion to train translators so as to violate translational norms (Toury 1980b), my only venture into translation didactics, should be understood: not as an end in itself but rather as a means of opening the students' eyes and minds to the multiplicity of modes of translation, all of which may be legitimate, in one context or another (that is, according to a certain set of norms), and helping them wend their own way through the dark forest.



## PART FOUR

# Beyond Descriptive Studies

## Towards some laws of translational behaviour

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 been invested in attempts to establish such laws. However, as soon as one accepts the applicability of the scientific method to the complex of problems clustered around translation, there is no reason why the formulation of laws should not be set up as an ideal and mark the horizon here too. Moreover, the quest for laws would seem appropriate not only within the 'pure' branches of the discipline, but also from the point of view of its applied extensions, to the extent that the underlying rationale favours basing them on knowledge of the real world 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

One convenient point of departure for such a discussion may be to bring the issue into relief by 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.



### 1.1 Lists of possibilities do not constitute laws

First of all, the mere enumeration of whatever is initially possible in events, acts, texts or any other phenomena which may be regarded as translational would yield no satisfactory laws.

To take a relatively simple example: 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 done by Levy himself (1967: 1173), or by Leppihalme (1994) with respect to the possible behaviour of allusions under translation.

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* statements: they present both the options and the logic of selection in a highly systematized form. The main reason why they do not, at least 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<sub>1</sub> itself, as it is determined from the vantage point of the source text.

Lists of initial possibilities thus appear to be rather *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-entities are, and the solution-entities will be, incorporated; 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, even in cases where it is similar to what goes on in other cultures.

All in all, lists of this kind are equipped to deal with *options* rather than *choices* involving actual *decisions*. This renders them necessary, but insufficient: they are all too elementary, in terms of a theory of translation.

### 1.2 Directives are not laws either

On the other hand, directives cannot serve as theoretical laws either. True enough, unlike mere lists of possibilities, normative formulations are often supplied with modifying factors of one kind or another. However, this is not enough to grant them any theoretical status, let alone the status of a law, if only because they are not in a position to predict anything: everything is possible, given the relevant conditions.

To return to the living metaphor approached as a translation PROBLEM<sub>1</sub>: what would fall under 'directives' is not only flat prescriptions like the following dogma:

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 (I):

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)

Formulations using modals – mainly verbs such as 'should' and 'must', or their antonyms – abound in our field.<sup>1</sup> 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. Quite often they are even masked as downright theoretical formulations, as if they were inherently binding rather than as a reflection of particular socio-cultural attitudes. 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,

1. In this connection, see again Table 1 at the end of Part One, where the differences between Translation Theory, DTS, and the applied extensions of the discipline are summarized and exemplified by the use of typical verbs.

the kind of behaviour it tries to promote can always be either *accepted* or *rejected* in actual practice, either followed or ignored.

Thus, while stating what a translator ‘had better do, or else’ (in the opinion of whoever accepts the directive), there is absolutely no assurance that a normative pronouncement such as this one 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 reflect no existing behaviour would not unconditionally create any new behavioural patterns either.<sup>2</sup>

The schism between actual and recommended 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:<sup>3</sup>

**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 actual translators’ behaviour? And what is one to do, should that behaviour be found to *refute* the sweeping totality of the maxim?

The absence of a recommended mode of behaviour on the list of initial possibilities would of course call for action on a *theoretical* level: namely, having the list submitted to revision – unless it could be shown that an option is initially impossible despite its having been presented as a directive. Such a falsification would

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2. 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 programme have to forget some of what they have been taught, if they wish to fulfil their task in a socially relevant manner.

3. 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.

involve theoretical reasoning again, albeit of a different kind. However, recourse to theoretical considerations 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, precisely because it has been favoured by real ‘persons-in-the-culture’.

The series of both ‘existing’ and ‘recommended’ options thus emerge as subsets of the list of ‘possibilities’, yet each one of them embodies a different constitutive principle: the first one empirical, the second one postulated. The position of either subset with respect to the entire range of possibilities is thus straightforward. By contrast, the relations of the two subsets to each other cannot be regarded as given. Establishing them requires conscious research efforts, so that they always appear as pertinent to particular cases only. Any attempt to draw a generalization with no prior attention to cases of other kinds would result in a fallacy.

Of course, the position of an ‘existing’ option in the repertoire of translators could be taken as a crucial factor in rendering that option a ‘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 in the first place.<sup>4</sup> 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 professionals’ within a culture, as once suggested by Chesterman (1993). Even less can

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4. A seeming exception is a 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 a point of suppressing any evaluation. However, his personal preferences often show nevertheless.

these changes be said to represent an ordered – and reasoned – transition from ‘is’ to ‘ought’, as Chesterman further suggests (following Searle 1964).<sup>5</sup>

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), generalizations are seldom an outcome of the regularities observed. And even less often is their justification made on the basis of those regularities, statistically or in any other way. More often than not, formulated generalizations, and especially 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 allows the researcher to look down on the translators whose behaviour is supposedly under observation and claim better knowledge.

This patronizing attitude also accounts for the fact that recommendations are often put forward as being far more valid than actually warranted by the underlying study and its corpus. Seldom is an attempt made to even justify such an extension of application, e.g. from translation into language/culture A to translation at large, irrespective of the target language/culture. (And see my critical review of Kjær’s book [Tourey 1989], which is still one of the best descriptive studies on translated metaphors.) It is not easy to see how it could be justified either, without further studies in the same theoretical framework, using the same methods, on different corpora, and then comparing the results in an attempt to go beyond the norms which pertain to the cultural (sub)system directly represented by the initial corpus or indeed 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, 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

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5. 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 somebody’s view.

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 is placed at their centre rather than any variable in itself. Establishing individual variables is, of course, a precondition for the possibility of stating their relations, but fulfilling this condition is not nearly enough. In fact, as I see it, translation theory will not be released from the corner it has been driven into until it loses its indifference vis-à-vis the manifold factors which have the power to affect translation, on the one hand, while resisting all temptation to replace this indifference by mere normative, often arbitrarily constituted formulations, on the other.

My belief is that each relational law, when uncovered and accurately formulated, will assume a **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.) Consequently, the desired theory – a set of laws of this type striving at maximum coherence – will have a **probabilistic** form, which would put it in line with recent developments in other sciences of man. (With respect to language and linguistic behaviour see especially Halliday 1991.)

Obviously, no conditioned law is conceivable unless the **conditions** lend themselves to specification. On the other hand, it would be impossible to formulate so much as one single law without an actual specification of the conditions which may affect it. Thus, unlike the directives which have been populating discourse on translation, the quest for laws would have to take into full consideration regularities of behaviour obtained by an evergrowing (and ever more variegated) series of studies into well-defined corpora. As should have become clear, this is the main justification for my insistence on the vital role of descriptive-explanatory inquiries into translation in the evolution of the discipline as a whole, and on the pivotal position of DTS as a branch within it. As I have tried to demonstrate throughout, what we are in need of is studies which are not just *observational*, but *experimental* as well, where some of the variables are far easier to control and the studies themselves have a much higher level of replicability.

This is not to say that every individual study is, let alone should be, carried out with a view to revising the 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 frowned upon, driving every other scholar to indulge in theorizing, 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 and clearer relationships between the tasks of researchers and theoreticians.

At the same time, the findings of well-executed studies are always in a position to bear on the theory in whose framework they were performed, thus contributing to the theory’s verification/refutation/modification – 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 again and render it even more intricate; and so on and so forth, towards an increasing 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 are supposed to.

In this process of ongoing refinement it will become necessary to do more than just identify and 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 become necessary to weigh the individual factors, and the bearing each one of them has 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 of  $Y$  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** which are more than just guesswork.

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 and therefore were not taken into consideration, 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 so typical of the relationship between theory, methodology and empirical research.

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 focused, 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 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 possible candidates for laws in their raw form and trace parts of the process of their conditionalization: the law of GROWING STANDARDIZATION and the law of INTERFERENCE. These two possible laws, furthermore, will be found to be largely interconnected.

### 3. On the way to 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 translation, in various cultures, have been able to come up with: namely, the law of growing standardization, which has also been presented as the law of the conversion of *textemes* to *repertoremes*.

#### 3.1 The law of growing standardization

In its most general (i.e., unconditioned) form, the law of growing standardization may be tentatively formulated as follows:

**in translation, source-text textemes tend to be converted into target-language (or target-culture) repertoremes.**

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 processes of *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 culture. A repertoire may also be



accounted for as the range of *choices* which makes cultural functions realizable through real products and practices (see e.g. Even-Zohar 1990:40–43). Any sign which forms part of a repertoire, irrespective of rank and scope, is definable as a **repertoreme**.

When a repertoreme is retrieved from the repertoire it is part of and put to actual use (i.e., inserted in a particular *utterance* or *text*), it enters into an ad hoc network of relations, peculiar to that act/text. These relations lend the repertoreme unique *textual functions* which render it a **texteme**. One and the same item – or, more precisely, different aspects of it – can, of course, partake of various textual structures, so that 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 account, our law can be rewritten 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 here is the claim that the dissolution of textual relations, which has long been realized as inevitable during an act of translation, is by no means temporary, and is far from characterizing only the initial phase of the process. Rather, the disintegration of the patterns exhibited by a source text (or reconstructed by its reader/translator) is hardly ever reparable in full: traces thereof can still be observed after the final phase of recomposition has been completed: sometimes in the TL text as such, but certainly in a comparison of assumed translations vs. their sources. This is one of many reasons why translations so often manifest greater standardization than their sources: being unique, textual relationships are more difficult to reconstruct than institutionalized ones.

Consider the verbal expression ‘drifting up’, an English repertoreme whose initial collocability with either ‘lawn’ or ‘vines’ is rather low. In the following text, Scott Fitzgerald’s *The Great Gatsby*, this phrase was made part of a figurative network implying a race over hurdles (see the italicized portions), thus being given the status of a (relatively) living metaphor, which is based on its repertoremic function (denotative / dictionary meaning) but not identical to it:

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 the metaphorized 'drifting up' in this ad hoc use was translated into three different languages. There was no connection between the different translations, such that similar decisions were made independently:

TT<sub>1</sub>: (German): **emporrunkte**

TT<sub>2</sub>: (French): comme **emportée**

TT<sub>3</sub>: (Hebrew): (kedei) **letapes** [= (in order) to climb]

All three translations 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.**

Consider, for instance, the "Rocky" passage used by Anderson et al. (1977) to illustrate two different possible interpretations ('wrestling' or 'jail') 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 the University of Heidelberg (Germany) and presented by Kurt Kohn (see Neubert et al. 1995), is no doubt much simpler than the source text, which – devoid of background information – allows for both readings – in full accordance with the law:

TT<sub>1</sub>: "Ringkampf" [wrestling match]

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.

TT<sub>2</sub>: "Gefängnis" [jail]

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 jet zt *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 textually 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 – might well shed more light on the issue at hand. Thus, for reasons beyond the scope of this discussion, the jail option seems to be the *unmarked* one 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 tilt the balance in favour of the wrestling match 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 not be clear-cut, in terms of that disambiguation; these would present a mixture 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 have the capacity of influencing translation behaviour, and may hence be rendered as ‘conditions’ in a more elaborate, multiconditional formulation of the law of the conversion of textemes into repertoremes. (Please note that not for one moment have I implied that the translator necessarily makes these decisions in full consciousness!)

This fact notwithstanding, the law can also be conditionalized by features which are 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 that culture 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, translators 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 the US 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).<sup>6</sup>

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 culture: a position whose exposure forms an important part of any individual study. Since descriptive research has amply demonstrated that this law is 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

**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 focused studies are obviously needed to determine what other factors might reinforce (or attenuate) the operation of our tentative law as well as establish the relative force of these factors themselves and their interdependencies.

Thus, it stands to reason that there are some basic *cognitive* factors which enhance the validity of our conversion law: for instance, limitations of memory and/or other difficulties in drawing generalizations and 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

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6. 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 degree of acceptability (which was actually connected with the establishment of different concepts of acceptability as a text in the target language/culture and as 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 stage, in all cultures either, nor even with respect to all types of translation and/or the 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.

[1978]) and to compare them to each other. Consequently, it would seem that special socio-cultural circumstances are not sufficient to counteract the bonds of the conversion law. Special cognitive efforts seem to be required too, if one is to approximate the reconstruction in TL material of the network of relationships constituting an SL 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 status implies.

It is clear that in translation, repertoires may also be converted into textemes, inasmuch as the end product is accepted as a *text* to begin with (which implies the existence of an internal network of relations which lend [some of] the text's features the status of textemes). Textemes occurring in a translation can of course result from either *reconstruction* (of source-text relations and functions) or *construction* (of new, ad hoc webs and functions).

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 appears instead. Both translation and source text are thus highly intricate entities in their systemic structure and rich in elements of textemic status, yet their constitutive principles are very different.

Clearly, 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.<sup>7</sup> 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.

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7. And compare Jones' brief discussion of a TL replacement "which adds an extra level of structuration" which did not exist in the source text (1989: 192–193).

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 this should not be from the view point of the *end product* alone (as a possible explanatory hypothesis of the shifts which may be revealed), 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** (also referred to as simplification, or flattening) vis-à-vis their sources; this 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, owing 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 the discipline too. After all, these laws are extensions of norms, and once a law (and a norm) have been uncovered and formulated in language, 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 we would hardly have been led to find it 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 goal – with full awareness, however, of the deviation from prevalent patterns of behaviour, and hence ready to take the consequences.<sup>8</sup>

A decision to consciously adopt ‘compensation’ as a strategy (e.g., Hervey and Higgins 1992: 34–40) would thus count as a manipulation of the tension between textemic and repertoremic status, and the principle itself could be imparted to translation trainees with relative ease.

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8. And see my account of how I came to the idea of activating the findings of my studies of the use of Hebrew conjoint phrases of near-synonyms in an attempt to achieve a particular effect in one particular translation (Chapter 7, Section 5). By the same token, after I went public with the examples of the use of the pragmatic void connectors *u-vexen* and *ho* in Hebrew translations from English (Chapter 6, Section 3), many of my former students who then became translators virtually stopped using these items, which led to a change of reality.

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 5, 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 codified repertoires would ensue, with a possible bearing on the acceptability, even readability of the target text, either as a TL text or even as a translation into TL. Otherwise it would be realized that the law of converting textemes into repertoremes has had the upper hand again, despite the conscious wish (or 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 has been bothering me for years, the so-called law of interference. This law is not totally unconnected with the law of conversion, which should be taken as a first welcome step towards the establishment of translation theory as an intricate system of *interconnected* 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 force themselves on the translators and 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., an increase in the frequency of features which do exist in the target system and can be used anyway). 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 domains which are regarded as less disturbing) necessitates special conditions and/or special efforts on the translator's part.

It seems that, basically, what I have called 'discourse transfer' (see Excursus C) has to do with the mental processes involved in any act of 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 purely a matter of cognition, all translators, in all cultures, would have been expected to behave in just one way while translating anything and everything from any language to any language. Similarly, all cultures, in whatever phase of development, would have been expected to *accept* interference, at least in the same way / to the same extent, and there would not have been much 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 – are a matter of the event as a whole, and not just of the cognitive act. In other words, they 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 caused 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 when moving from language to language. On the other hand, attempts



to produce translations which would be close representations of their sources are in constant tension with the attempts to produce in a target culture and language native-like texts, especially if these are designed to perform for the target audience a job which is comparable 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,<sup>9</sup>**

no matter (for the time being) what it was that brought about this 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 seems to be only *one* procedure which yields both, interference as such will always be present. It may just be more or less easy to discern.

As we have seen at the very beginning of our discussion in the case of the train notice example (Chapter 6, Section 2), even with regard to brief, relatively simple texts, which are, moreover, codified entities in their respective cultures, translational behaviour should not be expected to proceed automatically; and most texts in our world – originals and translations alike – are not of that kind anyway. On the contrary, 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.<sup>10</sup>

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 *professionalism*, so that

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9. 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.**

10. And see again the experiment reported in Chapter 14, Section 2.1, where advanced students of translation were found to replace the German simile *strahlenförmig* by the English expressions ‘like the rays of the sun’ or ‘like rays of a star’. This tendency reveals a high degree 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.

**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; however, they did operate within different socio-cultural settings and hence had different norms as guidelines for their behaviour as translators.

The alleged undesirability of interference is thus not ‘natural’ in any sense. Rather, if and when manifestations of that phenomenon are rejected, its undesirability is always a function of a cluster of **socio-cultural factors**, which may therefore be said to serve as conditions for 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 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, because of cognitive as well as behavioural factors. These mechanisms are often resorted to post factum, after the act of translation itself has come to its end, 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 reviser is not even required to know the SL, and even if s/he knows it, there is nothing to guarantee that s/he will indeed fall 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 target culture, and uses them as a monitoring device.

However, even the correlation of interference, and tolerance of it, within the overall normative structure of a particular target culture, is still in need of submission 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 equally 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 – tends 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 some other sense.**

Needless to say, notions such as ‘majority’ and ‘minority’, ‘strength’ and ‘weakness’, are relative rather than fixed, let alone inherent features of languages and cultures.

The sweeping generality of the correlation just established between sociocultural 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, at the beginning of the 21st 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 interference, leading to differences in recourse to it between, say, instructions for the use of an electric appliance and a lyrical poem; and this not only, not even mainly, on the (lower) linguistic levels, but on the (highest) level of the textual model as well.

For instance, the claim may well be made 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 original Japanese model is reconstructed (or mimicked) to capacity. To be sure, this has not always been the case, which means that a lot more work is needed 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 discriminative 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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