

On Toury's laws of how translators translate

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Gideon Toury has proposed two exemplary laws of the way translators produce translations: the law of increasing standardization, and the law of interference from the source text. Much subsequent investigation has tended not to be on the level of laws but is instead framed in terms of translation universals, particularly when involving the use of comparable corpora. However, when comparing Toury's proposed laws with Baker's compilation of four possible translation universals, we find that the latter elaborate only the first of the laws and do not regularly concern interference from the source text. This one-sidedness of the "universal" agenda enhances the justification of comparable corpora but poses serious problems for any kind of causal explanation of the findings. It would thus seem advisable to return to the duality of Toury's exemplary propositions. At the same time, the probabilistic nature of Toury's laws, which would become strong or weak depending on variable sociocultural conditions, means that they cannot properly be universal on the solely linguistic level, where they would in fact appear to contradict each other. In keeping with this, we propose that the tendency to standardize and the tendency to channel interference are both risk-averse strategies, and that their status as possible laws thus depends on the relative absence of rewards for translators who take risks. It follows that future possible laws might be found in the dynamics of risk management.

Key words: Toury, laws of translation, universals of translation, risk management, Translation Studies

A personal and dispensable prelude

This text will offer a reading of Gideon Toury's proposed laws of how translators translate. Those laws will be compared with four universals of translation compiled by Mona Baker. The reading and the comparison will be with a view to a possible unification of all the proposals. That is an intellectual activity, as dry as old bones, and not for the faint-hearted. Yet it has a personal and political context.

In 1992-93 Gideon Toury and I coincided at the *Sonderforschungsbereich* in literary translation in Göttingen, Germany. He would come to our house for Sunday

lunch, and we would try to make conversation. He was writing *Descriptive Translation Studies and beyond* at the time, so I would ask about the book. “Let’s not talk shop”, came the reply, understandably enough (Professor Toury came to lunch to get away from the book). So I would try to talk politics, and ask about the Middle East: “Wiser minds than mine have not been able to solve that problem”, was the response. With work and politics thus out of bounds, we must have managed to discuss food, family, whatever. In fact, there must have been some degree of success in the exchanges, since my daughter, then aged two, conferred on Professor Toury the honor of being the only adult ever invited into her cubby house (a privilege never extended to myself). Two-year-olds tend to know a good man when they see one.

We all separate our private and professional lives to some extent. Gideon Toury makes special use of the separations. His work as a translation theorist is mostly kept apart from his work as a literary translator, just as his place in Israeli culture is rarely mentioned to academic circles outside of Israel. My frustration at not being able to discuss his work-in-progress was partly annoyance with that kind of separation. When I eventually did see *Descriptive Translation Studies and beyond*, as a printed book, some of that frustration came out in my *Method in Translation History*, which I was writing at the time. I did not have an abundance of good things to say about Toury at that stage, at least academically. My book was then refused by Benjamins (quite independently of Toury) and accepted for publication by Mona Baker at St Jerome Publishing. Those were perhaps the consequences of the initial separation of personas: irritation led to publication in one place rather than the other. Subsequent events, however, have obliged me to think beyond the masks.

In 2002 Gideon Toury and Miriam Shlesinger were ousted from the boards of journals edited by Mona Baker. That was how the Egyptian Mona Baker chose to apply a boycott of the Israeli state and all those employed by that state. Part of Gideon Toury’s very brief response to that action was predictably in keeping with his separation of personas: “I was appointed as a scholar and unappointed as an Israeli” (reported in Goldenberg and Woodward 2002). True enough, Toury was there for his merits as a translation scholar, and not because of any particular national provenance. To my knowledge, that point has never been responded to in any adequate way. Mona Baker’s journals do continue to put countries after everyone’s name, as if we were all in quaint national dress. But there has been no real attempt to argue that the world of Translation

Studies (or any other world for that matter) can be neatly divided in accordance with the policies of nation states. All we have is the boycott as an extreme division of people, not just of personas.

Some will see this text, indeed this book, as breaking that boycott. Yet neither the paper nor the book are playing the game of nation-states. There is a far more substantial loyalty at stake. Perhaps since Boëthius, scholarship has partly been its own reward, never fully exchanged for economic or political gain. Certainly since the Renaissance, the European humanities, the *studia humanitatis*, have never been fully reducible to national policies or pay checks, although they have certainly needed both and have occasionally flirted dangerously with both. The call of humanistic learning remains a shared vocation, a long-standing tradition that ideally produces knowledge for the benefit of all, in unrestricted relations of intellectual exchange and debate, beyond whatever we may think about the political or personal provenance of the people we are dealing with. The practice of that vocation is always difficult, requiring countless symbolic separations of people into personas, incurring innumerable petty frustrations along the way. Yet that shared creation of knowledge is the only aspiration that makes our efforts worthwhile; it is the only cause worth defending in this place; it is the only standard by which our scholarly relations and strategies should be measured; it is the calling shared by the most noble group of people, over centuries and across countries, to whom we can be loyal. The following lines have been written in that belief.

Introduction

In Gideon Toury's seminal *Description Translation Studies and beyond* (1995), the "beyond" part is a search for laws of translatory behavior. That last chapter has perhaps enjoyed rather less success than many other parts of the book; it would seem the least cited part of Toury's structured research program. Some reasons for this no doubt lie in the way that chapter is written, as a series of exploratory reformulations, each different and none definitive. Part of our work here will be to analyze why that style was necessary in 1995 and might still be valid today. At the same time, however, the quest underlying Toury's proposed laws has enjoyed considerable success elsewhere, notably under the label of a search for "universals" of translation, surreptitiously allied with specific research tools such as translation corpora. Here we thus propose to compare

Toury's original formulations of potential laws with some of the later possible universals of translation. The purpose of the exercise is not just to seek the reasons for relative success or failure. We are more particularly interested in whether anything really new has been added to Toury's insights, and also in whether there is a way to unify all the proposed laws and universals. Such a unification could be in the interests of developing a unified discipline. So as to allay the suspense: at the end we will claim that there is one underlying cause, which we shall call risk-aversion. And to tone down the fanfares: we will also claim that this underlying cause is properly neither a law nor a universal, although it might still be of considerable intellectual interest.

Toury's proposed laws

Much of Toury can be picked up and used in research projects, or fruitfully discussed in research seminars. The concept of norms, for example, seems easy enough to understand and apply; it could be the concept that launched a hundred research projects. Indeed, it is too deceptively easy to apply. But laws? Of the kind Moses would have brought down from the mountain-top? The formulations and reformulations of Toury's two candidate laws are hard to grasp, difficult to follow, and ultimately frustrating for the student in search of lapidary statements that can be written in notebooks and cited in prefaces. And so one reads and re-reads, moving from insight to perplexity. At one moment the two laws sound trivially obvious, even cheeky. One posits *grosso modo* that translations have less internal linguistic variation than non-translations. That sounds like nothing more than the age-old complaint that translators over-translate (cf. Mounin 1963). The other law suggests, and again we impose our own words, that translations tend to carry over structures of their source texts. That is little better than another long-standing complaint: translators produce foreign-sounding language. Toury would seem to have picked up the bad things that critics perennially say about bad translations, and he has promptly turned them into unavoidable qualities of all translations. You have to stand back to see it, but much of Toury's intellectual *modus operandi* is based on precisely that: take what everyone says, and argue that the opposite, or almost opposite, might be equally valid, if not more valid. *Enfant terrible* of his age, to an extent that we sometimes fail to appreciate, he formed our discipline by turning opinion on its head.

It gets worse. Read in those simplistic terms, Toury's two laws surely contradict each other. One law says that a translation is like all other translations (they all share "flatter language" and so on), and the second law says that translations are like source texts (they all have "interference"). If all translations are like other translations, how can they also be like their very individual and different source texts? Which is it to be? One suspects that, with one law looking forward and the other looking backward, this Janus image of translations can either not fail to be right, or must always be at least half wrong. Whatever we find, surely it can be attributed to either one side or the other? Let us now turn to the formulations themselves, to see if they resolve these naïve doubts.

The law of growing standardization

The first law refers to "growing standardization" and is given in several formulations. Perhaps the one most retained is the "conversion of textemes into repertoremes" (1995: 267). I think this means a source-text feature in some way specific to that text will tend to be replaced by a feature from the stock held in waiting in the target-language genre. I know this happens. When I put occasional Australianisms into academic texts, thus creating expressions that are rarer than a blue-arsed fly, they either just disappear in translations or are turned into something absolutely standard (if indeed the copyeditors do not eliminate them first). My textemes are converted into repertoremes, much to my chagrin. This tendency is thus also called the "law of conversion" (274), explained as follows: "textual relations obtaining in the original are often modified [...] in favour of (more) habitual options offered by a target culture" (268). Then the same thing in different terms: "items tend to be selected on a level which is lower than the one where textual relations have been established in the source text" (269), where "lower" means on a less text-specific level. The general phenomenon is also exemplified (with an experimental example) in terms of procedures that seem rather more specific: "disambiguation and greater simplification" (270). That makes sense, since if one reduces the reader's options by taming ambiguity, the effect must be greater standardization. However, the door is thereby opened to all forms of "simplification". Are all reductions of complexity to be included under this law? This would be suggested by further alternative expressions such as "reduced rates of structuration (that

is, simplification, or flattening)” (273). The proposed law is thus expressed in terms of an open-ended string of adjectives: when compared with non-translations, translations are simpler, flatter, less structured, less ambiguous, less specific to a given text, more habitual, and so on.

Why this string of adjectives, instead of staying with just one formulation (of which several good candidates are given)? The technical reason surely lies in the degree of generality involved. The law can be applied at the level of many different linguistic variables. To restrict the formulation would involve limiting the law to the variables best suited to the terms or best exemplified in the discussion.

A more probable reason, in fact a down-to-earth version of the former, is the nature of the laws as an *invitation au voyage*. Far from being laws that have to be obeyed in order to escape punishment, these are ideas to be pursued, played with, experimented upon, and thereby extended into an open-ended beyond. The law is not engraved in stone; it is instead penciled on students’ translations of Rocky (270), the simple textual experiment that Toury would like to see duplicated by others, to “shed more light on the issue at hand” (270).

The law of interference

Less seems to be said about the law of interference, which starts from a very simple formulation: “In translation, phenomena pertaining to the make-up of the source text tend to be transferred to the target text” (275). That seems painfully obvious. Where else is a translation going to get its features from? The simplicity is nevertheless soon complicated when we realize that the “make-up” can also come from “the normal, codified practices of the target system” (275). Only then does it make much sense to say that the transfer may be negative (when the translation deviates from what is normal in the target-system) or positive (when it does not). That means that even when the results of interference are invisible to the reader (since positive transfer appears normal in the target system), there is still interference. At this point most readers pause to reflect, or should do so.

One paragraph ago I risked a cheap pun on *stone/Rocky*, with a tenuous link to the earlier mention of Moses. It is a *texteme* (no repertoire would deign include it). Let us imagine a translator who tries to carry that bad pun over to the target text, perhaps

inventing something based on the sounds of the English name (several possibilities are offered by *tablete/rocoso* in Spanish, or by *pierre/rocher* in French, where Rocky could become the Saint Peter of Toury's pugilistic church). In that case, the texteme would be rendered as a texteme, and there would be interference of the negative kind. The translator would *not* have obeyed the law of growing standardization; they would have obeyed the law of interference. However, if the translator opted to render the opposition *engraved/penciled*, or even *law-giver/student*, the result could be quite standard in the target culture and yet there would still be interference from the source text. In that case, the translator would have acted in accordance with the law of standardization and the law of interference, both at the same time.

That is one way we might understand and possibly agree with Toury's claim that the two laws "are largely interconnected" (267), for as long as the interference is positive. However, the argument is sophistic, to say the least. A better way would seem to lie in the term "make-up", in the phrase "phenomena pertaining to the make-up of the source text tend to be transferred to the target text" (275). What is this make-up? Cosmetics? Looking at some of the examples, it seems more reasonable to see it as a set of segmentational and macrostructural features. Toury mentions, for instance, that the law of interference might apply in a German translation of a haiku, but not in a translation of a video-recorder manual (279), since the latter might be made to conform to the way Germans write instruction manuals. That makes sense. Between European languages, at least, there is a default norm by which one translates sentence for sentence, paragraph for paragraph, text section for text section, and that whole default behavior might be described as "interference". This understanding of Toury even makes good sense of the claim that non-interference "necessitates special conditions and/or special efforts on the translator's part" (275). You usually need a very good reason to change a paragraph break, and it does take a special effort. I note in passing that this is even more true when we work with translation memories, where the segmentation imposed by the software makes rather more impositions than authorial segmentation ever did (cf. Dragsted 2004).

That said, I remain perplexed by both these possible readings of Toury (the sophistry of "positive interference", and the possibility that one law applies best to macrostructures). I sense there is something here that I have not properly understood,

and that illumination will come soon. Do Toury's laws ultimately contradict each other? For the moment we leave the question in the air.

Baker's proposed universals

Mona Baker has been a prime proponent of using corpus linguistics to study translations. Since her early programmatic texts (e.g. Baker 1993) one of the aims of the exercise has been to describe the "principles of translational behavior", also known as potential "universals of translation". In a slightly later text we learn that a particular kind of corpus linguistics, comparing translations with non-translations in the same language, can "allow us to capture patterns which are either restricted to translated text or which occur with significantly higher or lower frequency in translated text than they do in originals" (Baker 1995: 235), although on this occasion there is no mention of universals as such. However, in the following year (Baker 1996) the author gives a brief checklist of the universals she feels might be identified. Although that text makes no reference to Toury and carries no mention of "laws", the list of universals might ring some bells. Here we summarize the list:

1. *Explicitation* is "an overall tendency to spell things out rather than leave them implicit" (1996: 180). The phenomenon is seen as including text length, as well as "optional *that* in reported speech" (181), to which we return soon.
2. *Simplification* is "tentatively defined" as "the tendency to simplify the language used in translation" (181). This includes phenomena such as using shorter sentences in the translation, preferring finite instead of infinite structures, resolving ambiguity, removing unusual punctuation (from Malmkjær's text published in 1997) and producing texts with a lower lexical density and a lower type-token ratio (183). In features such as the preference for non-finite structures, "there is a clear overlap with explicitation" (182).
3. *Normalization/conservatism* is glossed as "a tendency to exaggerate features of the target language and to conform to its typical patterns" (183), as seen in the use of normalized punctuation (Malmkjær 1997, Vanderauwera 1985), finishing incomplete sentences, and avoiding ungrammatical structures.

4. *Leveling out* is described as “the tendency of translated text to gravitate towards the centre of a continuum” (184). For example, “the individual texts in an English translation corpus are more like each other in terms such as lexical density, type-token ratio and mean sentence length than the individual texts in a comparable corpus of original English” (184). Similarly, very oral texts become more written, and written texts become more oral (Shlesinger 1989).

Before we attempt to assess these four proposals, a few notes should be made on the way they are presented. In Toury, the one law goes through a series of dialectic reformations, drawing out possible consequences, indicating a direction for thought rather than a quotable product. In Baker, on the other hand, we have one simple noun phrase for each proposal, in most cases just restating the name of the potential universal (“simplification” is “the tendency to simplify”, with a simplicity worthy of Grice). What in Toury is an intellectual process becomes in Baker a readily understandable and reproducible product. Toury takes 13 pages to describe two laws; Baker needs just five pages for four universals—there can be no doubt where the greater cost-benefit lies. Many readers do not understand Toury’s laws; everyone understands Baker. That is perhaps why there are frequent citations of things like “Baker’s concept of simplification” (cf. Laviosa in this volume). Baker can be cited with confidence; Toury is a minefield. In sum, Baker’s is a great and necessary divulgation, absolutely vital whenever we attempt to address a wider audience (in a process that can be taken further; Pym 2003 gives five potential laws in just one page—great value for money!).

Of course, divulgation is not authorship, and Baker is curiously reluctant to cite sources here (she cites sample experiments, but not conceptual antecedents). “Explicitation” carries no reference to the “explicitation hypothesis” formulated by Blum-Kulka (1986) with respect to cohesion markers, nor to the work of Klaudy (cf. 1993), which extended the term to a very wide field. As noted above, Toury is not cited in the article at all (although he is indeed cited in the article Baker published in *Target*). Nor did he have to be. After all, his English-language discourse was about laws. Then again, the vocabulary of translation universals had certainly been developed by Toury in Hebrew (cf. Toury 1977) and by other authors working with Hebrew, similarly not mentioned here on the conceptual level. The intellectual work is in Blum-Kulka (1986),

of course, but also in Blum-Kulka and Levenston (1983), Shlesinger (1989), Zeller Mayer (1990), and Weissbrod (1992), at least.

With respect to the actual universals proposed by Baker, we might be concerned about two aspects. First, all four propositions appear to be saying much the same thing. Second, they all seem to elaborate Toury's law of standardization, without touching his proposed law of interference. That is, Baker might have taken half of what was available in Toury then divided that half into four. Let us test the arithmetic.

To start with the most obvious, Baker admits that some of the features listed under "explicitation" are also under "simplification". This is recognized with respect to the preference for finite structures (no examples, but we take her word for it). Yet could it not also be the case for the lower lexical density in translations (explicitation, in Blum-Kulka's sense, adds grammar words), and indeed for type-token ratios (explicitation adds common words)? At the end of the day, both explicitation and simplification make texts easier to read, and the line between the two becomes hard to discern. Precisely when is explicitation not also a simplification? Or would it be fairer to say that explicitation is one of many types of simplification?¹

Another relatively obvious mix-up: The tendency to normalize punctuation appears under both "simplification" and "normalization", with the same reference to Malmkjær each time. So what is the essential difference between these two universals?

For that matter, if all translations share the features of explication-simplification-normalization (the three that clearly overlap), then it is likely to follow that translations tend to "gravitate towards the centre of a continuum" (the fourth universal, "leveling out"). The norm is theoretically in the center of a bell curve, after all, and this fourth universal refers to the same linguistic variables as the previous ones (lexical density, type-token ratio, sentence length and, in Shlesinger, explicitation and normalization).

So we have some grounds for suspecting that all these universals are different aspects of the one underlying universal. In fact, the term "universal" here seems to be

¹ The problem here is that implicitation can also be seen as a form of simplification (cf. Pym 2005) when it reduces the complexity of syntactic structures. For Blum-Kulka and Levenston (1983: 119), lexical simplification was thus "making do with *less* words" (cf. Laviosa in this volume). Explicitation would then be a kind of simplification that reduces complexity by spreading semantic load over a wider stretch of text. The problem in Baker at this point would seem to be that no specification is made of what level simplification is supposed to operate on. If, as we suspect, it can be found at all levels, then it should perhaps become the superordinate for explicitation, implicitation, and much more as well. (There is nothing more complicated than a simple definition of simplicity!)

naming no more than the linguistic variables that operationalize then test the one underlying universal (which we should perhaps call a “law”?). The usage is nowhere near the intellectually demanding concept of “universals” in Chomskyan linguistics (cf. the critique in Malmkjær 2005). Then again, if Baker’s four universals do happily say much the same thing, perhaps that is as it should be. After all, if they contradicted each other, they could not be universal at all, could they?

This is where some niggling worries set in. First, if “leveling out” is truly universal, then a lot of translations will presumably avoid anything extremely explicit, extremely simple, lexically very non-dense, or with a very low type-token ratio, in order to move to the center of the continuum. But if they did that, of course, they would thereby falsify the universals of explicitation, simplification, and parts of what has been shoved under normalization. In the question of universals, you cannot have your cake and eat it too, can you?

Second doubt: The universal of “simplification” includes the shortening of sentences (by no means a universal of Spanish-to-English technical translation, but let’s move on). This must surely contradict the universal of explication, which makes sentences longer, as indeed it might contradict disambiguation and the lowering of lexical density, both also included under “simplification”, not to mention finishing incomplete sentences, classified under “normalization”. If a lot of universals are pushing sentences to be longer, it seems difficult to use sentence length as an indicator of anything as simple as simplification.

Third doubt: Baker incorporates Shlesinger’s findings into the universal of “leveling out” without taking into account that Shlesinger was working on oral translation (interpreting). This raises the question of whether the universals are supposed to apply to all kinds of translation, or just to the written kind that easily gets into corpora. The question is of considerable importance in view of what Shlesinger herself said in that early study, where the “equalizing universal” (Shlesinger’s term) is considered a feature predominant in the interpreting mode itself, and that mode is found to be more powerful a variable than are the tendencies to explicitation or to adoption of target-side norms. More explicitly, Shlesinger effectively proposed that interpreting has its own universal (since she found the spoken or written mode carries more weight than explicitation or normalization), and thus that not all universals have the same strength.

To find that argument drafted into a neat list of four equally balanced universals begs many questions.

Now for our second major suspicion (leaving aside the minor doubts). What is the difference between these four universals and Toury's law of standardization? Many elements are mentioned in both places, or can be interpreted as such: normalization ("habitual options" in Toury), simplification, disambiguation, low lexical density ("reduced structuration" for Toury), and low type/token ratio ("flattening"). On the other hand, Toury does not discuss sentence length, hopefully for the reasons we have just mentioned (it is the result of too many different causes). More strangely, Toury's discussion of standardization makes no mention of explicitation, although Blum-Kulka's explicitation hypothesis is certainly mentioned elsewhere in Toury 1995. Why this absence? It might be because of Shlesinger's work on interpreting (Toury directed that research), which did after all suggest that explicitation was not universal to interpreting. The general idea of explicitation might nevertheless be contained, I suggest, within the more general metaphor of "flattening". It could moreover be seen in Toury's reference to translators selecting items "on a level which is lower than the one where textual relations have been established in the source text" (1995: 269), which would concord with Klaudy's view of explicitation being any reference to a less specific or less defined item (also expressed in terms of levels). In all, we might surmise that explicitation, like sentence length, is not mentioned by Toury because he was seeking categories of more general validity. These were supposed to be high-level laws, not just norms.

If Baker's universals can thus be more or less fitted into Toury's law of standardization, what room is left for his law of interference? None, apparently.² This way Baker avoids the problem of having major universals that overtly contradict each other (any internal contradictions are minor in comparison, to be put down to sloppy thought and little else). So is Toury's second law at all necessary? The answer could have to do with explanation.

² Another question is whether this is necessarily so. Laviosa (in this volume) sees Tirkkonen-Condit's Unique Items Hypothesis ("a new universal") as a particular case of negative interference (so is it really new, or just another linguistic variable to test the same law?). The attribution is of interest here because the testing of unique items can be done with comparable corpora (as in Baker), and yet it does locate a particular kind of interference. The trick, of course, is that the comparative linguistics has to be done prior to the corpus work, in the selection of the "unique" items to be tested.

Laws with explanations

If any part of Toury's laws most deserves to endure, it is probably not his descriptions of the linguistic variables (the part we have looked at so far). The laws are far more engaging when they relate linguistic to extra-linguistic variables in a probabilistic manner. In his later work in this area, Toury has emphasized that this is the direction of his interests (e.g. Toury 2004). We should thus attach particular importance to the following correlation hypothesis (which refers back to a similar hypothesis formulated by Even-Zohar in 1978):

[...] the more peripheral [the status of the translation], the more translation will accommodate itself to established models and repertoires (Toury 1995: 271)

That is, the law of standardization is not something that happens in all translating. It tends to happen in cases where the translation (or translation in general) is of relatively minor importance (less "central") within the target culture. The point for us here is that the linguistic variables depend on sociocultural factors. The law is subject to social conditions.

Much the same happens with Toury's law of interference:

[...] tolerance of interference [...] tend[s] to increase when translation is carried out from a 'major' or highly prestigious language/culture (278)

So the law of interference also depends on sociocultural factors. The actual relation here seems fairly evident, since "interference" effectively means imitating the foreign, and no one imitates something that has no prestige. Let us note in passing that Baker says virtually the same thing, albeit the other way around:

The higher the status of the source text and language, the less the tendency to normalize. (1996: 183)

This is without reference to Toury or Even-Zohar (we do not wish to suggest any plagiarism—Even-Zohar and Toury are hard to read). The main point is that, thanks to these probabilistic formulations, it becomes quite reasonable to have contradictory tendencies on the level of the linguistic variables. If social conditions A apply, then we

might expect more standardization. If conditions B are in evidence, expect interference. And there is no necessary contradiction involved.

Of course, if you only have one coherent universal on the linguistic level (Baker does not address the possibility of interference on the linguistic level), how can you explain that this universal ceases to be universal once the status of the source text changes? One suspects that Baker's probabilistic correlation does not really belong in her theory, and certainly has little place in her method.

That suspicion can be tested. Here we turn to Olohan and Baker (2000), a study of the optional English reporting *that* (as in "She said [that] she would come"). Olohan and Baker find that this connector is more frequent in a corpus of English translations than in a comparable corpus of English non-translations. The higher frequency is hypothetically attributed to "subconscious explicitation" (although one might also see it as an eliminating a marker of orality, in accordance with Shlesinger's equalizing hypothesis). Whatever the alternatives (and there are more), the higher frequency is offered as support for explicitation as a potential universal. The connector is obviously a grammar word, and those were the things that Blum-Kulka's original hypothesis was based on (cf. Pym 2005). The attribution would seem to be justified by the theory. After all, if you only have one kind of underlying universal, you can only have one kind of attribution. However, if the higher frequency of *that* were due to some specific *social* variable like the status of the original texts (the explanatory hypothesis that has intruded into Baker's shortlist), how would we know? The corpora have little to say about those large social variables. More worryingly, if the higher frequency were due to interference (the major tendency absent from Baker's list), how would we know it?

As we have mentioned, Baker (1995: 235), re-affirmed by Olohan (2004: 43), argues that translations can be studied by comparing them with non-translations in the same language, without focusing on source texts or source languages. This means we can describe translational English in opposition to non-translational English, doing all the research on English. The result is perhaps the major methodological advance associated with corpus studies. It has many economic advantages: it cuts out all the bother of learning foreign languages and cultures; it controls numerous tricky variables associated with suspicions of linguistic and cultural relativism. In the English-only research on optional *that*, there is thus strictly no way of knowing about any kind of foreign interference causing the frequencies of the linguistic variable, since in principle

the source texts are not in the corpus. Unfortunately, the likely equivalents of reporting *that* appear to be obligatory rather than optional in virtually all the non-English languages I have asked about. It follows that the higher frequency of *that* in the translations could be a case of straight interference. But since the corpora are of English texts only, Baker's "comparable corpora" will never tell us about it.

Does this explain why Baker's list of universals does not include interference? Does it explain why Toury's laws do? We can only guess. Yet the suspicion must remain that Baker is looking for the only causes that her tool and method will enable her to find (which is why, indeed, the research only concerns the *reporting* function of *that*, because it can easily be located by searching for the verbs). Since source texts do not enter into the method, the tool is selecting the possible explanations, and apparently the possible universals as well. The tail is wagging the dog.

A related problem here concerns the kinds of explanations that can be offered on the level of linguistic analysis alone. For Chesterman (in this volume), when frequencies of reporting *that* are attributed to the "universal" of explicitation, the move does constitute a kind of explanation. For us, the procedure merely involves formulating a hypothesis of extendibility and giving it a name ("explicitation"). The name is there for as long as the data justify it, and the data are grouped together for as long as the name is useful, so the relation between the two levels is ultimately tautological, as in any naming operation. There is a big difference, for us, between that kind of conceptual work and the more intellectually satisfying explanation that comes from relating two levels, both with data, both with names, and with some kind of hypothesis linking the two. We might thus seek to explain the linguistic variables in terms of social variables, or cognitive variables, or indeed any other level offering creative explanations. Corpus studies do this occasionally when referring to cognitive hypotheses as possible explanations, albeit without causal reasoning or probabilistic dependencies. Toury, however, does this consistently in his probabilistic laws, notably when he refers to the cultural position of translations and the relative prestige of source texts. Those external variables would seem to be social, or cultural, or sociocultural (cf. Pym 2006). But could they also be cognitive or psychological? At this point the search for explanations must indeed move beyond the narrow descriptive level.

A possible unification

Malmkjær (2005), echoing something of Blum-Kulka (1986), favors a kind of cognitive explanation for explicitation. She turns to Grice's Cooperation Principle, on the basis of which we might say that translators provide more "communicative clues" than non-translators; they are somehow more "cooperative" in communication. We could go further and propose that translators tend not to flout whatever Gricean maxims are operative in the target locale (the maxims are culturally variable, but the Cooperation Principle is not). We might thus be able to cover a whole range of something like "communication assistance strategies", basically stating that translators tend to avoid extremes of all kinds (cf. Shlesinger 1989). That kind of thinking might be able to explain translation features in a very powerful way, based not on narrow linguistic descriptions, nor just on a theory of translation, but on a theory of the fundamental reason why people enter into communication with each other. That view also fits in neatly with claims that translators are basically nurturers, helpers, assistants, self-sacrificing mediators who tend to work in situations where receivers need added cognitive assistance (i.e. easier texts). In line with that view, Simeoni (1998) broadly sees translators as being in subservient positions, with less power than their communication partners. We might therefore surmise that translators are somehow disposed to work so that others do not have to do excessive linguistic work. There is just one problem with this magnificent all-embracing explanation: the linguistic features are not universal to all kinds of translation. As in Baker, that view is only explaining the "standardization" part of translation processes. It fails to account for interference as the other aspect of what happens in translation.

Let me give an example of why it is important not to abandon the concept of interference. The little that we know about how translators work with translation memories (the market leader is currently Trados WorkBench) suggests that the technology reinforces some of the standardizing tendencies but reduces others (Gow 2003, Dragsted 2004, Bowker 2005). Greater consistency at the level of terminology and phraseology fits in with everything we have placed under the rubric of "standardization" (that is why companies use the memories). At the same time, however, the segmentation patterns (the textual "make-up" indeed) tend to come

straight from the source text as parsed by the software. When we compare translations done with memories to those done without, the ones done with the memory display a significantly higher level of syntactic interference. We might suppose (and we are going to check) that the resulting texts are harder to use than are translations done without the technology. In other words, what you win on the swings (greater standardization) you lose on the roundabouts (syntactic interference). Toury's two laws are both in evidence, at the same time, on different levels. Is there a way of explaining both tendencies at the same time?

The relations between risk, standardization and interference can be formalized. In the present circumstances, however, a narrative illustration should suffice:

You are translating and you are in doubt. For this particular source-text segment here you can think of two or three possible renditions, and they all seem more or less correct. No time to check any purpose-built corpus or parallel texts; no justification for checking with a client or expert, since basic correctness seems not to be in doubt. You have to decide, quickly. You are going to take a risk, as we all do when communicating. You are going to bet that this option, rather than the other two, will be usable. But which? You might decide to trust your experience of the target field and locale, and you put what sounds most normal. You still have doubts (too much risk?): choose an item at a more general level (a "lower" level in Toury's terminology). So far, everything you have been doing to deal with risk has been within the general tendency to standardize. But then, the translation memory is also giving you a rendition, there on the screen, quite independently of your personal experience or recollection. That translation comes with the authority of the job itself (memories are mostly provided by clients), or from the experience of a previous translator (presumably paid by a client), or from your own experience in less doubtful situations (also presumably paid for). Whatever the case, if you go for that option, the risk is to some degree moved away from your immediate domain. Either someone else is to blame (if things go wrong), or you can point to those who granted you success in the previous situation. Either way, the translation memory gives you a relatively low-risk option, and translators tend to select it. In a mini-experiment by Bowker (2005), translators opted for the memory-based rendition even when it was clearly erroneous. When in doubt, trust whatever seems authoritative. This general strategy gives results that clearly come under the banner of "interference".

We thus have two general ways of dealing with doubt: either say what seems normal or safe (standardization), or say what someone else can be responsible for (interference). Both sets of strategies are ways translators reduce their personal risk burden. In accordance with both laws, at the same time, many translators do not tend to take on communicative risk in their own name.

One might object, of course, that the use of translation memories is an aberration, a distortion of the true dynamics of translation. The same objection could be raised to Shlesinger's early study (1989), where the oral mode of rendition was found to be more powerful than the tendency to standardization. Then again, are the laws or universals only to be considered valid for translators working with pen and paper, with no time constraints, and with no professional risks involved? For that matter, what we find in the case of translation memories curiously resembles the strategies used in pre-print medieval translation. When rendering scientific texts from Arabic into Latin, translators would reduce the guesswork by adopting extreme literalism and numerous calques. The result might not make much sense, but it would not be entirely wrong. Further, throughout the medieval period, the ideological hierarchy of languages meant that translation was often used as a way of developing the target language, actively using interference to impose new lexical items and syntactic structures on the receiver. For that matter, much the same thing is happening today in localization projects into the more peripheral languages, and not only because of the use of translation-memory tools. The languages themselves are placed in an ideological hierarchy with respect to technology, and translation downwards on the hierarchy tends to operate through interference strategies of various kinds. For all these reasons, the relative predominance of interference is not to be considered an aberration introduced by minor communication technologies. Quite the reverse: The undue attention given to standardization strategies is more likely to be the result of bias in favor of written translation (particularly literary), in the second half of the twentieth century, and between languages not generally considered in need of structural development through translation (in a moment of ideologically "equal languages"). Perhaps because of the special history of Hebrew, and its importance for much of the research commented on here, Toury gave more specific weight to interference than did most of those who have since worked on universals.

Where the laws or universals might lie

Our proposed unification has reached this point: Translators tend to standardize language or to channel interference because these are two main ways of reducing or transferring communicative risk. This proposition is compatible with several other hypotheses concerning conditioning factors. For instance, if the source text or culture is authoritative or prestigious, it makes sense to allow that authority or prestige to absorb risk (thus producing interference). Or again, if the source-text syntax in a particular passage is especially difficult to interpret, it makes sense to invest effort in those places to reduce the risk of misinterpretation, thus producing standardization of various kinds (cf. Whittaker 2004). And if translation occupies a relatively peripheral place in the target culture, such that any potential receivers are likely to be very sure of themselves and seek no substantial influence from the foreign, then the safest bet for any translator is to use the linguistic repertoires and norms of the target culture (thus producing standardization). There are no doubt many more conditioning factors in this vein, and they can be expected to overlap and reinforce each other. For example, a cultural locale that feels itself to be prestigious and strong will also tend to hold translation in a peripheral position, and will thus have at least two reasons for rewarding standardization and not tolerating interference. The result would be what Venuti (1995) has termed “fluent” translation, considered typical of literary translation in the United States.

In our view, however, all those conditions only correlate with translation strategies to the extent that they affect the risks that translators are likely to take or avoid. The conditions do this by offering different reward structures. If translators are going to be rewarded (financially, symbolically or socially) for taking risks, then they are likely to take risks, rather than transfer them. Translators may then have an interest in breaking all the maxims, norms, laws or universals that theorists throw at them (Toury himself has envisaged the virtues of training translators to break norms). If we are translating for advertising purposes, for example, then the insipid language of standardized translations may be unrewarded or even penalized, and gains will be found by taking the risks of invention or, in some circumstances, extreme interference from the foreign. Similarly, in a locale of low-risk translations, a translator like Venuti can

seek unusual gains by adopting foreignizing strategies and reaping the rewards in a neighboring field, as an academic theorist of translation. (If not, how could his characterization of Anglo-American translation possibly account for translators like himself?)

In a vast number of cases, however, the translator's activity is not subject to any reward structure that can justify a risk-taking disposition. As Leonardo Bruni complained in 1405, authors always get the praise for what is good in a translation, and translators just get the blame for what is wrong (Bruni 1928: 102-104). With this kind of reward structure, translators will logically tend to be risk-averse, and risk aversion may develop into a deceptively universal behavioral disposition. Of course, once risk aversion is embedded, it tends to reproduce itself: risk-takers will go into other cross-cultural professions (the high-flyers of international marketing), and those that deal with translators will tend to assume risk aversion and thus structure tasks and rewards accordingly. Yet there is no fatality. With another kind of reward structure, we would hope to find other kinds of communicative strategies.

If there are large-scale explanatory laws or universals, we should not expect to find them on the language level alone (e.g. translations tend to have more or less X than non-translations). Nor are we wholly content with a simple correlation between linguistic variables and sociocultural variables like "prestige" or "position" (e.g. translations tend to have more X in the presence of social condition Y). We see the analysis of risk dynamics and of risk-management as a crucial link, leading to the formulation of relations that have a stronger human causation (e.g. translators will tend to take risk X in the presence of reward structure Y).

In terms of the unification we are seeking here, the closest we can come to a law-like formulation would thus be as follows: Translators will tend to avoid risk by standardizing language and/or channeling interference, if and when there are no rewards for them to do otherwise. That sounds fairly obvious, even empty. But the reward, for us, is not in the universal formulation itself. We are more interested in achieving cooperation between cultures, and in defining risk as non-cooperation. That formalization, however, will have to wait for another place.

One final consideration, to return to our point of departure: A text that is clear and readily applicable avoids many communicative risks and can thus find rewards in the short term. Baker's compilation of universals might be one such text, and the

rewards certainly came. On the other hand, a text that is dialectically abstruse and resolutely non-superficial runs a severe risk of not being understood in the short term, as indeed may have been the case with Toury's laws (I have needed more than twelve years to grasp an inkling about why there are two apparently contradictory laws.) In terms of risk allocation, if nothing else, Baker's work is simply not in the same league as Toury's. In taking that special kind of communicative risk, however, Toury implicitly sought a risk-taking readership, quite unlike those who run out and apply Baker's universals. Some of my students get to the end of the chapter of laws and exclaim, "Is that all? Is that what I get for so many hours of hard reading?" They feel their investment has not paid off; they took too high a risk and seem to have lost. Such communication will always be precarious and more than occasionally frustrating. Yet the compensation is not necessarily in the book itself, nor in its immediate application. In consciously leaving fundamental problems for the future to solve (the chapter on laws finishes very much *in media res*), Toury has the potential to produce rewards over vast stretches of space and time, wherever and whenever the loyalty to scholarship survives.

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