

## European Translation Studies, equivalence, and *une science qui dérange*

Anthony Pym

Published as “European Translation Studies, *une science qui dérange*, and Why Equivalence Needn’t Be a Dirty Word”, *TTR* 8/1 (1995), 153-176. This appears to be a pre-print version.

I take the term “Translation Studies” as an accepted and acceptable name for the general ruck of writings, debates and research on aspects of translation. “European” here means whatever is talked about in a disintegrated Europe, which might as well extend to Israel, Australia, Texas or Canada. “Equivalence” is the name of a concept that has organized much talk about translation at least since the 1970s. And the expression *une science qui dérange* - roughly “a science that upsets” - is one of the ways Pierre Bourdieu describes his sociology in an interview published in 1980. To put these terms together is obviously to pose chestnuts like whether Translation Studies can legitimately be described as a “science.” Less naively, however, the collocation proposes a specific comparison. It asks if Translation Studies, with or without equivalence, is a science like Bourdieu’s sociology. Is it in a position to upset anyone? If so, whom? And if not, why not?

### **Whom might Bourdieu upset?**

Much of what Bourdieu says about sociology should sound strangely familiar to scholars of translation:

“Sociology has the sad privilege of being constantly confronted with the question of its scientificity. People are a thousand times less demanding with respect to history or ethnography, not to mention geography, philology or archeology. Constantly questioned, sociologists constantly ask questions of both themselves and others. All this makes sociology look like an imperialistic science. How can a science that is scarcely beginning to stand on its feet be so presumptuous as to put other sciences under its microscope? Of course I believe sociology is a science. In fact, the questions it asks of other sciences are those that are particularly problematic for sociology itself. Sociology is a critical science because it is itself in a *critical* position.” (1980: 19-20, my translation)

The question of scientificity - often expressed as scholarliness - has been similarly problematic for Translation Studies, and perhaps for reasons similarly associated with its status as a relative newcomer. Of course, our location as an interdiscipline does not yet involve taking other sciences as entire objects of study. But the potential is perhaps built into the fact we draw on and question the approaches of the more established disciplines. It is also implicit in gestures like Susan Bassnett’s proposal to make comparative literature a subdiscipline of Translation Studies. Although Bassnett’s move precludes a definition of non-translation and seems likely to have as little actual effect

as its model - Barthes' hierarchical inversion of semiotics and linguistics -, could it perhaps be an expression of something upsetting?

Beyond bravura, Bourdieu gives good reasons why a science might upset people:

“Because it reveals things that are hidden and sometimes *suppressed*, like the fact that success at school correlates not with “intelligence” but with social background and more exactly with the cultural capital inherited from one’s family. These are truths that technocrats and epistemocrats - that is, a good number of those who read and finance sociology - do not like to hear.” (20, italics in the text)

Is Translation Studies involved in the revelation of such truths, suppressed as in psychoanalysis (Bourdieu’s term is *refoulées*) because socially disturbing? Are we in a position to upset those who read and finance our activities? Or do we lack the scientific machinery, the questionnaires, the statistics and schemata that enable sociology to claim quantitative truths that are apparently neutral and objective? But for Bourdieu, who opposes his sociology to that of the “social engineers” who would control individuals, the value of such scientific procedures is itself quite relative. Sociological findings are by no means discovered from a position of simple objectivity or neutrality:

“The possibilities of helping to produce truth depend on two main factors: the interest one has in having the truth known (or inversely, in hiding it from others and from oneself) and the capacity one has to produce it. As Bachelard put it, there can only be a science of that which is hidden. Sociologists are best armed to dis-cover what is hidden when they have the best scientific weapons, when they best use the concepts, methods and techniques developed by their predecessors [...]. And they are most “critical” when their conscious or unconscious intention is most *subversive*, when they are most interested in revealing that which is censored or suppressed in the social world.” (22-23, italics in the text)

Sociologists should use all the scientific weapons available to them, since they are from the outset involved in a conflict situation. Yet the use of such arms does not exclude subjective investment on the part of the sociologist, who has an interest not only in revealing hidden things but also in appearing to be objectively disinterested about this process. Further, although subjective investment by no means invalidates the knowledge produced, recognition of its motivating role requires that sociology undertake the sociology of sociologists, periodically turning its questions in on itself, objectifying rather than hiding the truth of its subjectivity. Sociologists become one of the objects of their own scientific research.

This is not the place to explain all of Bourdieu. I cite these passages merely to indicate a few of the points where comparison might help us to relativize our own problems, particularly those that concern ideals of objectivity and neutrality. The citations should nevertheless illustrate a few of Bourdieu’s more general formulas like the need to “subjectify the objective” (apparently disinterested scientific truths are in the interest of a certain subjectivity) and, inversely, the need to “objectify the subjective” (the very material of such truths is the subjectivity of people involved in social practices, of which sociology itself is one).

How might these statements apply to Translation Studies?

## In and out of equivalence

In 1979 Werner Koller identified a “legitimation crisis” in European Translation Studies:

“This science [*Wissenschaft*] has perhaps been more or less established as an independent discipline in some universities. But the relations and understandings between it and other fields of scientific inquiry is in no way unequivocal, settled or unproblematic. We have not (yet) overcome the legitimation crisis concerning the relation between translation science and translation practice, nor that between translation science and other scientific disciplines such as contrastive linguistics and stylistics, comparative literature, and computational linguistics.” (1979: 10; my translation)

This seems to imply that Translation Studies was already doing the right things. It just had to be formally legitimated, especially with respect to its neighboring disciplines. But did anyone imagine this could be done without conflict? Given the limitations on university budgets, student enrollments and publication possibilities, the emergence of a new discipline surely implied some degree of submergence for others. Particularly where translation was traditionally taught in departments of language and literature, the association of an independent Translation Studies with the independent teaching of translation was bound to create tension and confrontation. Opposition on the level of pragmatic interests could not help but question the scientific legitimacy of Translation Studies. Subjective interests thus motivated arguments for and against scientificity, and did so in a way that was far more demanding than might have been the case for many of the more established disciplines.

Koller’s response to this problem, elaborated in his *Einführung* of 1979, was to bring together and systematize the work that had already been done. In practice, he assumed the essential solution had already been found; it just had to be cleaned up, put on show and developed. A major part of this solution was the concept of equivalence, which brought with it the authority of structuralist linguistics, at that time still the most prestigious of the human sciences (Barthes did indeed place it above semiotics). Further, the theorists of equivalence could be presented as technical engineers interested in the better control of translation as a social practice. Their aim was the regulation and improvement of standards (cf. Reiss 1971). Equivalence thus became a piece of scientific capital, stretching out into a general paradigm with a certain amount of institutional power. It provided the foundation for research programs supposedly useful for both machine-translation research and the training of translators, responding to the rising social and political demand for controllable cross-cultural communication, particularly in what was then the European Community. Translation Studies was made to look like a science worthy of institutional support. It was also made to look like applied linguistics. As such, the equivalence paradigm enjoyed a moderate degree of success in advancing the cause of moderately independent research programs and

translator-training institutes. The concept was institutionalized.

The 1980s saw this linguistic concept of equivalence challenged in at least two ways.

For the historico-descriptivism of Toury (1980), equivalence was something automatically produced by all ostensible translations no matter what their linguistic or aesthetic quality. Thus defined, the concept could not be used prescriptively or in order to improve standards. Would-be social engineers could make no use of it to improve social communication. Worse, it could not support concrete institutionalization in the fields of machine translation or translator training. For Toury, the confidence of linguistic experts should thus logically give way to detailed descriptive work on actual translations in their historical context. If equivalence had upset no one, Toury's extension of it at least had the potential to upset prescriptive linguists.

For the target-side functionalism of Vermeer, on the other hand, equivalence was only one possible goal for a translation, which could serve many other social purposes (cf. 1989: 120 & passim). The determinant on translation was not the source text, as had been assumed by the theorists of equivalence, but the intended function or *Skopos* of the translation as a text in its own right. This was also potentially upsetting, at least for linguists and teachers of translation who had never looked beyond source-text criteria.

As revolutionary as they could have been, neither of these approaches denied that a translator could set out to produce one kind of equivalence or another. Nor did they deny scientific objectivity as an essential goal for Translation Studies. They simply refused to base their scientificity on equivalence, choosing to employ different kinds of weapons. Toury's science has invoked systems, hypotheses, empirical testing, and the search for probabilistic laws. Vermeer's has developed a rich assortment of technical-sounding names for various aspects of translation, combining discursive precision with elitist exclusion. One of the curious outcomes is that, whereas Toury has helped to develop a mode of corpus-based research where "a translation is any target-language utterance which is presented or regarded as such" (1985: 20), Vermeer's institutionalization means that prospective students at Heidelberg are told that the institute's term "Translation" does not correspond to "the translation that unthinkingly duplicates linguistic forms and structures" ("das unreflektiert Sprachformen und -strukturen nachvollziehende 'Übersetzen' und 'Dolmetschen'.") (1992: 2). For historico-descriptivists, translation is anything people commonly think it is (social practice cannot be wrong). For Heidelberg, it is precisely *not* what people commonly think it is, especially if they imagine it is a matter of producing equivalents (social practice can be correctively engineered). In the first case, science is empirical investigation; it goes out into the world and can advance on the basis of the material it analyzes. In the second, science is a matter of knowing what others have to find out; students come to you and advance on the basis of your scientific expertise; and if social

agents don't always know what a translation really is, they too can become your students. More important, neither of these approaches needed a strong concept of equivalence, which soon seemed unable to objectify anything of interest about translation. Having become either too large (for Toury *et al.*) or too small (for Vermeer *et al.*), the concept gradually lost its status as scientific capital.

What really happened here? The details are no doubt best told by the scholars involved. But one cannot help but notice that the general trend was entirely in keeping with the movement of linguistics towards discourse analysis, the development of reception aesthetics, the sociological interest in action theory, and the general critique of structuralist abstraction. Translation Studies was no hotbed of intellectual originality. More interesting, however, are the points of contact with the previous paradigm.

The new approaches of the 1980s effectively dis-covered a previously suppressed (or insufficiently recognized) truth, namely the social and historical relativity of linguistic equivalence. Many of the linguistic categories that had previously been considered objective could now be seen as largely subjective constructs. Beyond the restricted field of specialized terminology, theorists could no longer be sure that under conditions X, Y and Z, ST1 was necessarily equivalent to TT1 and not TT2. Such a relation could only be probabilistic (for Toury) or subordinate to wider target-side considerations (for Vermeer). There would always be at least residual doubt about general claims to equivalence.

This doubt should probably have undone the entire equivalence paradigm. It should have revealed the subjective interest that the theorists of equivalence - and their institutional supporters - had had in pretending to found a science of translation. We might say, with Bourdieu, that the objective could have been subjectified. The 1980s approaches could have done this. But they didn't. And no one was particularly upset. Why?

### **The critique of equivalence**

Almost ten years after Koller's *Einführung*, Mary Snell-Hornby's 1988 "integrated approach" sought to bring together and systematize the work that had been done, claiming to integrate just about everything *except* the concept of equivalence. As in Koller, the underlying assumption was that the compatibility was there; it just needed to be "integrated." The package was once again made to look appropriately scientific, this time privileging American panaceas like prototypes and scenes-and-frames. Yet one of the most remarkable aspects of this integrative exercise was the long list of effectively excluded approaches. Snell-Hornby's peremptory style dismissed two thousand years of translation theory as an inconclusive "heated discussion" opposing word to sense (9) (one finds the same inconclusiveness in theories of God, or love, and yet we keep

talking); she dispatched historico-descriptivism because it had avoided evaluation (26) (but hadn't it discovered anything?); and she especially regarded equivalence as being "unsuitable as a basic concept in translation theory" (22). None of these previous approaches, said Snell-Hornby, "have provided any substantial help in furthering Translation Studies" (26). Really? This of course implied that Snell-Hornby knew which direction Translation Studies should head in. And that the others didn't know. So misled souls should either succumb to her direction-giving integration or disappear altogether. The interesting thing about Snell-Hornby is that, unlike Toury or Vermeer, she tried to indicate where the equivalence paradigm had gone wrong. This is where Translation Studies could have become truly upsetting.

Some of the things that Snell-Hornby said about equivalence are perceptive and judicious. She recognized that the paradigm originally overcame traditional disputes between "faithful" and "free" translation strategies (15). She found that in the course of the 1970s the English term "equivalence" had become "increasingly approximative and vague to the point of complete insignificance," and its German counterpart (but what criterion did she have for putting the two terms together?) was "increasingly static and one-dimensional" (21). This difference curiously maps onto the strategies of Toury and Vermeer as outlined above, suggesting that there was in fact no radical rupture between those who talked about equivalence and those who preferred not to (Toury accepted the English-language trend; Vermeer accepted the German-language usage of the term). And then, summing up a very meandering argument, Snell-Hornby finds that "the term *equivalence*, apart from being imprecise and ill-defined (even after a heated debate of over twenty years) presents an illusion of symmetry between languages which hardly exists beyond the level of vague approximations and which distorts the basic problems of translation" (22). Some kind of equivalence could be integrated into its appropriate corner (technical terminology), but the equivalence paradigm should otherwise get out of the way.

There are two quite obvious problems here. First, if the term "equivalence" were really so polysemous - Snell-Hornby elsewhere claimed to have located fifty-eight different types in German uses of the term (1986: 15) - , how could she be so sure it "presents an illusion of symmetry between languages"? The term apparently means nothing except this illusion, and since she gives no citation supporting her claim, the illusion looks like hers. Of little import that the idealized symmetry between languages belonged more to the word/sense debate that, as Snell-Hornby herself recognized, the notion of equivalence was born to overcome (15). Indeed, had she looked a little further, Snell-Hornby might have found theories like that of Seleskovitch, where functional *équivalence* is ubiquitously presumed to be *easier* to attain the more the languages are different. And doesn't Nida's "dynamic equivalence" presuppose substantial linguistic asymmetry? Or again, wasn't Koller's actual proposal based on

studying equivalence on the level of *parole*, leaving to comparative linguistics the entire question of symmetries or dissymmetries between *langues* (1979: 183-4)? So where did Snell-Hornby get the idea that equivalence means “symmetry between languages”? She seems to have presented a limited range of variant usages, picked or projected the illusion that suits her, then assumed that everyone else suffered from the same hallucination.

One can only suppose there was more than logic at stake in this critique of equivalence. An element of power, perhaps? Snell-Hornby’s approach has indeed enjoyed a certain institutionalization, and may yet have more. Curiously, in the midst of an expanding European Translation Studies, a center of very good equivalence-based research at Leipzig has been all but dismantled by west-German academic experts, and the one western translation institute that has recently been threatened - Saarbrücken - is precisely the one most closely aligned with the equivalence paradigm. This is not to mention the numerous east-Europeans who still - heaven forbid! - talk about linguistics and equivalence. The institutional critique of equivalence surreptitiously dovetails into facile presumptions of west-European superiority and progress. Perhaps we should take a good look at the bandwagon before we hop on.<sup>1</sup>

### **Understanding equivalence**

Although the 1980s critiques of equivalence-based prescriptivism opened up new terrain, they did little to understand the logic of the previous paradigm. No attempt was made to objectify the subjective importance of equivalence as a concept. One thing is to argue that substantial equivalence is an illusion, quite another is to understand why anyone should be prepared to believe in it. A further lesson might be learnt here from Bourdieu:

“There is an objective truth of the subjective, even when it contradicts the objective truth that one constructs against it. Illusions are not in themselves illusory. One would betray objectivity if one acted as if social subjects had no representation, no experience of the realities that are constructed by science [...]. Sociology should not forget that in order for social practices to work, social actors must believe they are the ones who make their actions work. Some systems work entirely on belief, and no system—not even economics—can do without the belief that it can work. (32)

Illusions are not illusory. But when Snell-Hornby talks about “the illusion of equivalence” (1988: 13), she does so precisely to suggest that it *is* illusory and should thus be dispensed with. A more understanding approach would be to accept non-relativist and non-linguistic “equivalence beliefs” as part of the way translations are

---

<sup>1</sup> It is nevertheless to Professor Snell-Hornby’s credit that, as foundation president of the recently formed “European Society for Translation Studies”, she has spoken against the dismantling of Saarbrücken. The European Society is perhaps more than an exclusive “integration” exercise. The civil organization of translation scholars seems to be greater than any particular theory. We shall return to this point in a minute

received as translations (Pym 1993), and even to incorporate such illusions (never mentioned as such, since equivalence is a taboo word) into something like Gutt's definition of a direct translation as an utterance that "creates a *presumption* of complete interpretative resemblance" (1991: 186). Despite apparent regression to the 1970s paradigm, these recent positions seem to be drawing out the critical potential of something like Toury's initial acceptance of equivalence. Rather than force any translator to become an "equivalence-seeker" (Mossop 1983) they are simply objectifying the subjective, recognizing but not necessarily condoning a socially operative illusion that enables translations - and translators - to work. The arguments certainly recuperate a certain idea from the 1970s: translation and non-translation can be distinguished (a point sadly overlooked in Tirkkonen-Condit's 1992 review of Gutt). But both approaches belong to the 1990s in at least one very important respect: they formulate their restrictive definitions of translation within frames wider than translation. Gutt takes this wider frame as relevance theory; I have referred to sociological transfer theory (1992) and have more recently tried to incorporate an ethical dimension into the project by borrowing from negotiation theory. Of course, no great importance should be attached to the numerical decades. But the significance of these wider contexts should become clear in a minute.

The 1970s concept of equivalence was an expression of what translation ideally represented for certain people, notably translators in search of higher social status, readers in search of translated information, European politicians in search of reliable transcultural communication, and academics in search of authoritative science. It expressed certain ideals of translation as a contemporary social practice. A theory that now recognizes little substantial equivalence should nevertheless be able to recognize and objectify the subjective interests that make translation work as a social practice. This means that a Translation Studies adequate to its object cannot just put texts under linguistic microscopes. It must also objectify the subjective beliefs - the current but uncritical term is "norms" - that condition the way translations are received and thus the way translators tend to work. Such subjective beliefs obviously include the illusions that remain operative on the level of theory.

### **Solutions without equivalence**

If we now look a little more critically at the various critiques of equivalence formulated during the 1980s, one kind of belief seems surprisingly common to the different groups concerned. None of the 1980s theorists appears to have believed in any *exclusive* definition of translation. There are certainly many definitions; they all say what a translation looks like and does. Try, for example, Snell-Hornby's description beginning "Translation is a complex act of communication in which..." (1988: 81). Nowhere in the

page or so of text that follows those words is there anything about what translation is *not*. There are no definitions of non-translation. The current definitions are inclusive rather than exclusive; everything can be fitted in; everything is potentially translative; so Translation Studies might as well encompass not just comparative literature but the entire humanities as well, and more, if it would make anyone happier or more powerful.

Equivalence, on the other hand, no matter what definition it figured in during the bad old days, always implied the possibility of non-equivalence, of non-translation or a text that was in some way not fully translational. The 1980s thus saw a shift from exclusive to inclusive definitions, from defining non-translation to defining translation as something that was potentially unlimited.

The shift was without friction. Not everyone agrees that a translation can be a bibliographic reference (as in Gouadec), any text explaining foreign information (as in Holz-Mänttari) or just any text at all (as in theories of radical intertextuality). The change has nevertheless been institutionally successful. It has inspired several good propositions for new disciplinary frames, including “transfer studies” (after Even-Zohar 1980), action theory (for Holz-Mänttari), “rewriting” (Lefevere 1985, 1992), and of course relevance and negotiation theory (mentioned above). This is a general trend. In training institutes, which have been the real growth sector in the late 1980s and into the 1990s, the new perspectives have supported numerous degree and diploma programmes combining translation with cultural studies, intercultural studies, documentation, international relations, commerce, computer science, and so on (quantified in unpublished research by Monique Caminade). Everyone seems to agree on the need for a wider frame. To produce equivalence is nowadays simply not enough, neither for the theorist nor for the pedagogue. There is thus little subjective interest in exclusive definitions of translation. Instead, we find general belief that inclusive definitions are good enough. Why should anyone want to define non-translation anyway? (Don’t Gutt and Pym know that a revolution took place?)

If we now go back to Koller’s “legitimation crisis” of 1979, we find that a kind of legitimation for Translation Studies has in fact been found. Koller thought the answer would be to define Translation Studies as a science and thus regulate its relations with neighboring disciplines. But on the ground, the historical solution has been to *deregulate* relations, allowing translation to be studied in any number of disciplinary locations, in terms of any number of merely inclusive definitions. Instead of an independent Translation Studies, we now have an amorphous and fairly unruly grouping of approaches and interests that cover far more than any 1970s notion of translation. And we have a level of civil organization - the EST is only one of several associations - that should prove greater than any formal theory.

The currently dominant theoretical discourses mostly fail to recognize this social level of organization. Convinced that they came to power simply by being right, they

still argue for their own scientific rigor and institutional independence, apparently unaware that their very lack of an exclusive definition effectively debases rigor and independence. The result is a rather perverse series of power games between superficially opposed sides. A personal story might illustrate the point.

In the middle of an international (European?) conference in Prague, Professor Snell-Hornby quite legitimately challenged me to state my definition of translation, perhaps to situate myself in the theoretical panorama, to declare myself enlightened or retrograde. My answer, weak at the time but, in retrospect, not all that stupid, was to gesture to the audience and observe that there was a common kind of definition to which I subscribed. A conference on “translation” brings together people bearing numerous kinds of practical understandings and technical definitions (of which I carry one<sup>2</sup>), people with many different theories, including people from fields where translation is only of quite secondary interest. Within Translation Studies, there is thus an intuitive social definition of translation as a term for an area of discussion. And this social definition, together with its civil organizations, has solved most of the real problems even before the theorists start debating.

The continued calls for scientific rigor and academic independence contradict the social reality. Blind to this contradiction, theorists like Snell-Hornby turn their critiques on the equivalence paradigm, reproducing the values of the previous concept (rigor, independence) even in the absence of the crisis to which that concept responded. In a sense, the critiques of equivalence also reproduce the concept’s *institutional* functions, creating self-interested illusions of even more progressive rigor and independence (whatever we’re doing, it must be better than before), creating a mythology of the academic and professional “expert”, which I shall have to tackle elsewhere.

Thus, while Snell-Hornby sought to enact a centered integration in the 1980s, an upsetting sociology could now show a that successfully decentered disintegration was going on at the same time. This second reality, the historical solution to a legitimation crisis, is worth dis-covering.

## **Envoi**

Bourdieu’s sociology upsets people by analyzing those who finance sociology, by turning in on intellectuals, and by giving a certain voice to the social groups that are its object. European Translation Studies generally fails to upset anyone because, with a few exceptions, it reproduces beliefs reflecting its own centered certitude and thus

---

<sup>2</sup> For the purposes of my own empirical work, a translation is a text received in such a way that 1) has ostensibly been produced by transformation of an anterior text, 2) its quantity is considered to be determined by the anterior text, and 3) the first person of its macrostructural discourse does not correspond to the producer of that discourse. Just in case you were wondering.

overlooks the particularities of the social group it periodically claims to be representing, namely translators. The various calls for an independent discipline have come from academic theorists, not practitioners. At the same time, and in contradistinction to developments on the other side of the Atlantic, European translation scholars have been remarkably reluctant or unable to engage in any wider social or academic debates. Although translation inevitably concerns extremely problematic phenomena like cultural specificity, political identity, and their nationalist combinations, translation scholars have so far refrained from wading into such turbulent waters (will thought yet be provoked by Zhirinovskiy's past as a translator?). There has been virtually no deconstruction, little feminism, no careful understanding of eastern Europe, and minimal critique of a thing called "culture" that remains so vague it couldn't upset anyone's idea of a culture.

In 1992 Susan Bassnett and André Lefevere declared that "The growth of Translation Studies as a separate discipline is a success story of the 1980s" (1992: xi). Translation Studies has indeed been reasonably successful in institutional terms, but the reasons are perhaps not always obvious (youth unemployment means students want to learn something that sounds practical; European integration means politicians are willing to fund practical-sounding translation). Few would claim that Translation Studies has really succeeded as an independent academic discipline with something of its own to say to other disciplines. Compared with neighboring intellectual pursuits, it remained rather mediocre throughout the 1980s. Many of its theoretical efforts were directed towards gaining academic power rather than identifying and solving significant problems. Now in a relatively comfortable position, European Translation Studies upsets virtually no one. Such has been the price of its happily disintegrative institutionalization.

## References

- Bassnett, Susan, and André Lefevere. 1992. "General editors' preface." André Lefevere, ed. *Translation, History, Culture. A Sourcebook*. London and New York: Routledge.
- Bourdieu, Pierre. 1980. "Une science qui dérange." Interview with Pierre Thuillier, *La Recherche* 112 (June 1980). Reprinted in *Questions de sociologie*. Paris: Minuit. 19-36.
- Even-Zohar, Itamar. 1990. "Translation and Transfer." *Poetics Today* 11/1, Special issue on Polysystem Studies. 73-78. First version published under the title "Translation Theory Today: A Call for Transfer Theory". *Poetics Today* 2/4 (1981). 1-7.
- Gouadec, Daniel. 1989. *Le traducteur, la traduction et l'entreprise*. Paris: Afnor.
- Heidelberg University. 1992. *Studienführer Dolmetschen / Übersetzen*. Heidelberg: Institut für Übersetzen und Dolmetschen.
- Koller, Werner. 1979. *Einführung in die Übersetzungswissenschaft*. Heidelberg: Quelle

- & Meyer.
- Lefevere, André. 1985. "Why Waste our Time on Rewrites? The Trouble with Interpretation and the Role of Rewriting in an Alternative Paradigm." Theo Hermans, ed. *The Manipulation of Literature. Studies in Literary Translation*. London & Sydney: Croom Helm.
- . 1992. *Translation, Rewriting and the Manipulation of Literary Fame*. London and New York: Routledge.
- Mossop, Brian. 1983. "The Translator as Rapporteur: A Concept for Training and Self-Improvement." *Meta* 28/3.
- Pym, Anthony. 1992. *Translation and Text Transfer*. Frankfurt/Main: Lang.
- . 1993. *Epistemological Problems in Translation and its Teaching*. Calaceite: Caminade.
- Reiss, Katharina. 1971. *Möglichkeiten und Grenzen der Übersetzungskritik*. Munich: Max Heuber Verlag, Heuber Hochschulreihe 12.
- Snell-Hornby, Mary. 1986. "Übersetzen, Sprache, Kultur." Mary Snell-Hornby ed. *Übersetzungswissenschaft. Eine Neuorientierung*. Tübingen: Francke.
- . 1988. *Translation Studies. An Integrated Approach*. Amsterdam - Philadelphia: John Benjamins.
- Tirkkonen-Condit, Sonja. 1992. "A Theoretical Account of Translation - Without Translation Theory?" *Target* 4:2. 237-245.
- Toury, Gideon. 1980. "Equivalence and Non-Equivalence as a Function of Norms." *In Search of a Theory of Translation*. Tel Aviv: Porter Institute. 63-70.

## Free extras

2. The relative lack of conflict might be attributed to the common goal of scientificity, which has caused subjective doubt to be hidden. Historico-descriptivism has not seriously undone anyone else's subjective interests because it radically denies its own subjective interests. Toury overtly distances himself from any attempt to set standards or to intervene in actual practice: "It is of no concern to translation *theory* to cause any changes in the world" (1991:189), relegating possible subjectivity to the level of "applied extensions." Description is assumed to be massively neutral and frighteningly scientific. Target-functionalism, on the other hand, which is more closely related to teaching practices and thus cannot easily overlook the question of standards, displaces critical doubt towards a new kind of student. If objective relationships between ST and TT can no longer be established on the basis of ST alone, then the lacking information must be provided in each case. Students must be trained to ask clients what they want. And if that doesn't work, clients must be trained to specify what they want. In this way, theorists will have enough determinants to replace doubt with certitude, at the price of turning clients into students. If clients can be trained correctly, the apparent objectivity of equivalence can return through the back door, under a different name.

The names for objectivity vary. In descriptive work, a very popular and scientific-sounding one is "system", which becomes even more scientific as "polysystem". Thus when Theo Hermans states that "as a theoretical model the polysystem theory appears to provide an adequate framework for the systemic study of translated literature" (1985: 12), the technical prose does indeed appear to be meaningful. But could polysystem theory be anything but a "theoretical model"? Could a "system" model produce anything but "systematic" study? Why this cautious "appears to provide" when, after all, only Persil washes like Persil.

On the other side of the fence, prosaic caution was thrown to the wind as *Skopostheorie* drew from all quarters, from Greek, from English, from massive German compounds and massive German definitions. In order to be scientific, it seems, one must speak with precision and thus invent precise terms. Indeed. But the proliferation of names-for-things then somehow passes for theory (are the names actually saying anything about the things?). And the guarantor of this theory, now that linguistics has gone its way, is the magic word "expert". This term and its cognates are found all over the place, in descriptions of the translator as an expert in intercultural communication, or in claims that translation should be left to the expert. It might translate the German "Fachmann" (the "professional" of a trade rather than a science). But no, Vermeer explicitly says "expert", in German. And only an expert can legitimate an expert. Only Persil washes like Persil.

Descriptive approaches at least go out into the world. Functionalist theorists, on the

other hand, cite each other with admiration, build up bibliographies, then declare the quantitative result to be progress. However, in order to protect their own scientificity, to protect the objectivity of their systemic results or self-declared expertise, neither of these approaches have really tried to understand the concept of equivalence. Translation Studies in the 1980s, like the various hierarchical organization charts inspired by Holmes, looked more like parallel play than serious debate. Where there had been one kind of institutionalization, protected by linguistics, now there were three, at least. And everyone was reasonably happy. Then someone suggested that Translation Studies should be something further, a happy family.

3. A certain amount of empirical inwardness might be considered an advantage when compared with American tendencies to project truth onto outdated European philosophies or run-down feminist puns. Some Europeans have at least started to look at translations rather than fete opinions about translations. And yet, whenever they have to state their reasons for looking at translations, for seeking research grants or for training translators, the ideology that flows out is mostly brute publicity shielding a dangerous lack of thought.

Read the tracts, the introductions, the brochures. To translate is inevitably a good thing; the rise in the demand is a sign of international progress and understanding; a nation that translates is a nation among nations; translators find employment and should therefore be produced. All fine stuff. But the rise in translator-training - the real growth sector - corresponds not just to the social demand for translations but also to high levels of youth unemployment, creating a market for anything distantly associated with the job-market limbo. At the same time, the subsidization demand for translations themselves is becoming a crippling cost on the European Union, financed by nationalist ideologies interested in anything but substantial union. Translation scholars mollicoddle these ideologies in order to protect their own interests. After all, the more Europe translates, the more it needs self-declared experts to train translators. Little attention is given to the problem of what should *not* be translated, nor to what weighting should be given to language-learning policies (José Lambert's empiricism raises these questions but gives no answers). Thus bulwarking the prestige of Europe's major non-English languages, translation has become a classical sacred cow; translation scholars have indirectly become the silent priests of its subsidized rites; the last thing anyone wants is a real translator to come out of the woodwork and talk about the rubbish that is translated unnecessarily, the traditional training courses that are largely irrelevant, the underemployment of many graduates, the unread machine-translations, and the ways in which translation costs can and should be reduced by politically incorrect means (including the unspeakable: using English as in the international language it is). The current debates occasionally suggest the mutual aggrandizement of

intellectual tadpoles, in a rather remote academic backwater.