On the social and the cultural in translation studies

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The numerous sociocultural approaches in Translation Studies are generally of the "toolbox" kind, where any number of models and factors may be drawn upon. This situation leaves many doubts with respect to what might constitute a sociocultural explanation, how pertinent factors can be located methodologically, what kind of causation is involved, and whether the social and the cultural might actually be the same thing. In attempting to formalize and solve those problems, we offer models where explanation requires methodological movement between the social and the cultural, where pertinent factors are located in and around the professional intercultures (or "translation cultures") that define the borders of large-scale social systems, where causation appears as relatively asymmetric correlation, and where the sociological is partly quantitative (abstract empirical data) and the cultural is usually qualitative (signifying practices). The general approach is deemed suited to the study of mediators as people, rather than just texts as objects in systems. As such, it draws on advances in Interpreting Studies and resists subordination to any more general study of whole societies.

The Prague conference in September 2003 was ostensibly on "Translation Targets". The papers in the present volume have nevertheless been brought together under the title of "Social and Cultural Approaches". A few months later there was a Tel Aviv workshop on "Institutions, Habituses and Individuals: Social, Historical and Political Aspects of Cultural Exchange", bringing together sociologists and translation theorists. In May 2005 there was an international conference in Graz, Austria, on "Translating and Interpreting as a Social Practice". And for quite some years, as Franz Pöchhacker observes in his article in this volume, much of the research on interpreting has been "going social" (having previously "gone psycho"). Something sociological is in the air. There is, one suspects, a general tendency at work, of some breadth and depth. Here we will try to see what that tendency might be, using the papers in this volume as a rigorously non-random sample.

A focus on mediators rather than translations

There is no shortage of social and cultural approaches to translation. One could go back to any of Eugene Nida's excellent books, which include titles like *The Sociolinguistics of Interlingual Communication* (1996). One could cite standard references such as Maurice Pergnier's *Les Fondements sociolinguistiques de la traduction* (1980). One should also note a remarkably Franco-Canadian interest in sociological approaches to literary translation, forging a small tradition that might link Annie Brisset's *Sociocritique de la traduction* (1990) with Jean-Marc Gouanvic's *Sociologie de la traduction* (1999). Or more generally, one could claim that the whole thrust of Descriptive Translation Studies, since the 1970s, has been to bring wider contextual considerations into the study of translation. In that sense, social and cultural approaches have been with us for thirty years or so, or considerably longer (Nida published important papers in the 1940s).

The vast majority of those books and theories, however, were fundamentally ways of studying *texts*. A sociology, sociolinguistics or cultural analysis was sometimes applied to the way the source text functioned in its context, and increasingly to the way the target text worked on its side, but either way, texts were the thing. That focus was understandable enough. Theorists of Bible translation are committed to the written Word; sociolinguists aim first to describe language use; and much of Descriptive Translation Studies came from literary studies, where the text remains the thing. True, the impact of critical discourse theory, particularly as in Foucault, has invited translation theorists to view both text and context in terms of discursive formations, effectively extending textuality into the social domain, where texts can become very big things. And that is a point one could equally reach from the Russian Formalists or from general semiotics. The upshot is that we have no real shortage of social and cultural approaches to translations as texts. Further, that general trend has kept in step with developments in well-established disciplines like Linguistics and Literary Studies. Witness the growth not just of Sociolinguistics but also of Text Analysis, Discourse Analysis, Pragmatics and Cultural Studies, all in search of wider contexts.

Something quite different, however, might be expected from approaches that focus on *translators* rather than translations. If we look for a sociology of translators, or more generally of mediators, what do we find? There is virtually no focus on individual human translators in Nida, nor in most of the prescriptive sociolinguistic approaches. After all, a theory that sets out to help an unidentified translator has little interest in analyzing variable social identities. Contextualized translators are similarly rare in most of the classical references of Descriptive Translation Studies as well (Popovič, Levý, Holmes, Even-Zohar), where the focus is mostly on translation as a series of changes ("shifts") manifested in texts, or as an effect

("interference") on a cultural system. In a sense, the initial negation of prescriptivism (which is why we talk about Descriptive approaches at all) simply repeated the exclusion of the mediator. We moved from a sociology of source texts to a sociology of target-side effects, but little was said about any sociology of translators.

Where then might we find a focus on translators as people? There have been important moments within and around more recent Descriptive Translation Studies. Gideon Toury (1995) went some way toward analyzing a social subjectivity when he adopted the eminently sociological concept of norms, understood as regularities of behavior (and hence of human actions, rather than just linguistic structures). Toury has moreover shown interest in the way people become translators (1995: 241ff), which necessarily entails questions of professional contexts. One could also cite studies of power relations between translators and patrons (cf. Lefevere 1992), questions about the social effect of certain translation norms on the asymmetric relations between cultures (cf. Venuti 1995), or interest in the role of social mediation as a feature of all communication and hence as a way of constructing the sociolinguistic identity of the translator (cf. Peeters 1999). Yet none of those initiatives has yet formed any orthodoxy that might be called a sociology of translators.

A somewhat complementary approach can be seen in the foundational texts of German-language "functionalist" approaches. Vermeer and Holz-Mänttäri, in different ways, have allowed the translator a very active role in the communication process, with a specific social identity (cf. Holz-Mänttäri's insistence on expertise, or Vermeer's awareness of the historical position of translators and their capacity to negotiate with clients). Those approaches were not strongly sociological in any empirical sense, yet they were certainly interested in analyzing social relations rather than just texts (note that Holz-Mänttäri found one of her points of departure in Malinowski). Not by chance did *Skopostheorie* provide the initial frame for Franz Pöchhacker's placing of the conference interpreter within the context of not just a source-text speaker and a target-text listener, but also of the whole conference as a macrotext (Pöchhacker 1994).

If there has been a growing focus on mediators and their social contexts, it is perhaps more evident in the field of interpreting than that of written translation. After all, the interpreter's situation is there, immediately visible for all to see. Its network of social relations could hardly be overlooked (and yet it was overlooked for many decades, in favor of psychological approaches). The more profound change is no doubt the developing interest in community interpreting or dialogue interpreting, dealt with here in articles by Pöllabauer, Grbić, Navarro Montesdeoca, Rudvin, Lannoy and Van Gucht, and of course by Pöchhacker himself. In the courts, in hospitals, in official interviews, the social situation is not only visible but also of overwhelming priority. There, we must be interested in who the mediator is

and how they relate to communication partners, and perhaps only then in the sociolinguistic qualities of isolated texts.

We might thus posit that, for some scholars and more particularly in some fields of research, the focus has shifted from texts to mediators. Many of us are no longer stopping at the sociocultural dimensions of source and target texts. We would like to know more about who is doing the mediating, for whom, within what networks, and with what social effects.

How far have we gone with that second set of questions? Here we seek answers from the texts brought together in this volume, first on the more traditional ground of literary translation, then with respect to the relatively recent advances in Interpreting Studies. We would generally expect many of the papers to adopt a "toolbox" approach to explaining translation, incorporating insights from the range of references we have mentioned above. Others, however, go in search of more specific concepts, drawing on Bourdieu, on Prunč's analysis of "translation cultures", or on a constructivist version of social systems. In surveying these developments, we shall find time to propose a few methodological categories of our own, hopefully of some use to future researchers.

Observation and explanation

Most studies in this volume start from a set of observations and then look for factors that might in some way explain those observations. The things observed mostly concern translation; the things that explain are in some way socio-cultural. The studies work on hypotheses, often implicit, that link the two. That is, the hypotheses seek to explain translation in socio-cultural terms.

Here we hesitate to talk about "explanation" in any grand definitive sense. We would agree with Chesterman (2004) that Translation Studies has so far been much better at observing than explaining, and that the real intellectual task awaiting us is precisely to form some consensus about what a satisfying explanation might be. In the meantime, the explanations we mostly find are discursive assumptions of *causation* of one kind or another. The hypothesis posits that a set of explanatory factors are in some way necessary for the occurrence of the thing explained, such that changes in those factors might bring about changes in the thing observed. That is a very problematic notion. It was effectively sidestepped by decades of studies that claimed to be merely "descriptive" or "systemic" (in a simple system, all factors would be weak causes of all other factors; in a polysystem, a bigger system would automatically have a stronger causal role than the smaller systems it interacts with). Explanation was nevertheless brought to the fore in Toury's project to formulate "laws of tendency" for translation (1995), where causal roles were attributed to

apparently non-translational things like the relative prestige of cultures (translators work differently when great prestige is attributed to the source culture). Toury, however, has consistently avoided searches for just one causal variable that might offer a grand explanation: "There seems to be no single factor which cannot be enhanced, mitigated, maybe even offset by the presence of another" (2004: 15). This would mean that deterministic reasoning cannot explain translation (nor translators, presumably). Explanations would drift off into lists of heterogeneous conditioning factors, each identified in a probabilistic way, with no one dominant causation at work. Translation would be what happens when many very different things occur in the same historical place, and little more.

Such pluralistic explanations are indeed what we mostly find in Translation Studies, and Toury's observation could probably be applied to most of social life as well. There is, however, something profoundly unsatisfying about approaches that offer no more than complexity. There is no reason to suppose that, since every factor may play a role, all conceivable factors are potentially good explanations. In theory, all researchers would have to consider the role of all possible factors. Toury is correct to point to *probability* as a way out of this dilemma. This means that the kinds of hypotheses we seek concern tendencies rather than mechanical cause-and-effect. They would be of the form "The more X, the more Y" (e.g. "The more prestigious the source culture, the more foreignizing the translation"). Probability then means that our studies hope to predict, to some extent, what is likely to happen when something changes in the explanatory factor. We would say that, on the basis of our previous studies, the prestige of a culture is likely to have a causal relationship with the selection of a translation strategy, and perhaps that this factor is more likely to be a cause than other potential factors (for example, the translator's experience, sex, or pay). This probabilistic way of thinking is the bread-and-butter of the social sciences; it should really move us into the statistical modeling of relative likelihoods. Unfortunately, to assess the probabilities we have to build up a database of known causation, and to do that we have to look at all possible factors. This means that we are brought back to square one, at least with respect to how to set up a research project, or how to advise those embarking on research. If researchers have to look at everything, they will finish up studying nothing; their reports will tend to become farragoes of facts, and particular case studies will not easily yield general principles. Even worse, if there are always further factors to consider, how will researchers ever know they have found an explanation?

Here we would like to ask some simple questions about how well different factors can provide explanations. First: Is there is any a priori difference in nature between the factors that are observed and those that explain? Second: What kinds of factors would offer the most powerful explanations? And third: Is there any operational difference between the social and the cultural in this regard? There is

also a fourth question, which underlies the rest and has been developed elsewhere (Pym 2003): Do explanations just "account for" observed factors in a causal or probabilistic way, or do they in some way solve research problems?

We will try to answer those questions by looking at the studies in this volume, asking how they relate the observational and explanatory moments. Let us see.

Stella Linn observes the translation flows between Dutch and Spanish, which are more or less hard numbers: how many texts of what kind were translated when. Her proposed explanations then concern the roles of individual translators, publishers, government policies, gaps in the target market, intercultural events like book fairs, and indeed the ideological modernization of Catholicism (to explain the translations of theological texts from Dutch). Those explanatory factors would ideally be the multiple causes of the actual translation flows. Note that, in Linn's paper and virtually throughout, there is little question of reducing those many possible causes to just one dominant factor, or suggesting that they all fit into just one large system. Research in this vein is now able to draw on many different theories, using them to explain the partial phenomenon at hand. This sociocultural approach is profoundly multifactorial. As expected, it opens up a methodological toolbox rather than apply a panacea. We can all become *bricoleurs*, as was said in the days of Lévi-Strauss. Eclecticism is nevertheless not always intellectually satisfying. As we have noted, since you cannot describe everything (at least not in one article), and since the explanations are perhaps all caused by other explanations, where do you stop?

Pekka Kujamäki adopts a somewhat narrower mode of explanation. Observing the literary reception of Finnish literature in Germany in the 1920s and 1930s Kujamäki seeks explanatory variables in the role of just two intermediaries, Johannes and Rita Öhquist. Drawing on exchanges of letters, the analysis is able to demonstrate the particular influence that individuals can have on the relative openness and closedness of one literature to another. That in itself should be seen as a sociological contribution (exchanges of letters only have an effect within the networks of people writing to each other, across cultural borders). Of course, this approach does not exclude the many other factors that undoubtedly contributed to the translation flows. Research can quite legitimately focus on just one explanatory variable, since each explanation contributes at least some knowledge. Then again, this fascinating case study also shows how the ideological context of the times influenced the two mediators, moving Johannes Öhquist to produce "typically Finnish" pseudotranslations in German and Rita Öhquist to self-censor apparently decadent elements in her translations from Finnish, as both mediators increasingly adopted the ideological norms of the Führer's Reich. So, to apply the first of our promised questions, did the social context cause the individuals' con-

tributions, or did the individuals help cause the social context of the exchanges? Any answer should involve a bit of both, of course. We expect that the allocation of the observational and the explanatory is thus to some extent arbitrary.

Gaby Thomson-Wohlgemuth similarly draws on archival material, looking at the censorship files by which East German officials sought to control the translation of children's literature from English. In this fascinating peek behind the scenes of a wholly planned publishing industry, we find not only the actual paperwork by which texts were assessed for censorship purposes, but also the pertinent economic details of the literary exchanges involved. Here the observed data would presumably be the texts as printed or sold in East Germany; the explanation would be the intricate system of official policy, the practice of censorship, and relations with publishers. Once again, there are many other factors that one could have looked for or made more of – the role of translators and individual bureaucrats for example (no system can function without individuals), or the networks shared with other Soviet bloc countries (mentioned, but not seen as determinate). Where Kujamäki stresses individual mediators, Thomson privileges the official system. Some of the difference perhaps lies in the two very different cultural situations analyzed. However, differences might also ensue from our methodological assumptions of what we will find in those systems. The researchers have surely looked for and found the explanations that they initially considered the most probable or even comfortable. The logical large-scale application of this precept would mean developing different sociologies for different social situations. And how could we then form any common pool of probabilities?

Maria Goreti Monteiro observes that an eighteenth-century Portuguese translation of *Robinson Crusoe* omitted considerable material, notably the parts where Robinson explains to Friday what is wrong with the Catholic religion. Explanation is this time sought in the biography of the individual translator Henrique Leitão, who had problematic relations with the Inquisition and thus engaged in self-censorship. By omitting the most contentious passages, the translator quite probably saved his skin. Methodology in this case finds three clearly defined levels, like concentric circles: the observed omissions in the translation, the biography of a self-censoring translator, and the European struggle between Protestantism and Catholicism. The bigger the circle, the more explanatory the factor. Who could contest such fearful symmetry? The geometry of concentric factors has long provided a model of social causation, particularly in theories of social systems (the bigger the system, the more it can explain). The relative size of a factor (or "circle") can also be given a rather elegant definition: it is the number of other factors with which the factor has a causal relation. The translator's omissions must cause something, but their influence in the world is much less than the Inquisition. That is not

a problem. We note, however, that in this case the circles do not really capture any society as such, on neither the source nor target sides. We are concerned with the translator's past adventures in France, with his contact with a previous French translation (ostensibly published in Amsterdam), and with an ideological conflict that cut across the whole of Europe, both within and between nation states. The concentric circles of classical sociology (of the kind that would explain a national society) do not seem to hold in the case of translation.

Rodica Dimitriu shows us a series of further Robinson Crusoes, this time in Romania, where the text has spawned a multitude of translations and rewritings. Here the analyst observes the versions and seeks explanations in the apparent social reasons motivating their appearance: the translations were educational at first, then entertaining, subsequently a means of introducing the genre of the adventure novel, a complex novel in itself, and an economic parable in support of Communism. We are finally introduced to an imitative Robinsonade in which the hero is stranded not on a desert island but in a secluded village in Wallachia (part of Romania), confirming the rural traditions of the target culture. Each new version seems to bring with it its own special cause, building up a complex image of the target culture over history. But why only that particular culture, when most European cultures were producing similar and not unrelated Robinsonades? Because, one presumes, the researcher only looked at that culture, bringing it all home to a methodological Wallachia. Such are the beauties of traditional explanatory symmetries, in this case assuming then confirming target-side causation.

Gabriel Louis Moyal considers some of the factors motivating translations from English in early nineteenth-century France. Here the observation would be of attitudes to translations, particularly their appearance in the journals of the day. Explanation is sought in the expressed opinions of translators, writers, critics and journalists, all engaging in intercultural debate (we find Thackeray entering the fray as a Paris-based journalist). And floating somewhere above the complexity of the exchanges, there is grand politics: the translations were seen as being either for or against the regime in power. Are the circles in this case at all concentric, as in East German censorship, Robinson Crusoe tailored down for the Portuguese Inquisition, or another Crusoe transported to a Romanian village? The question is not as simple in this case. No doubt the political regime, as the ultimate level of explanation, defines the only place that could be encircled (France, as a monarchy or republic). For sure, there were many other factors intersecting across the Channel (or should we believe the British were entirely passive?). Yet the question is complicated most directly by the analyst's ethical desire not just to explain, but also to critique. For Moyal, translations require minimal standards of representationality. Not to seek such standards means appropriating the other, in this case misrep-

resenting a foreign culture in the interests of commercial gain. In the end, for Moyal, the ultimate explanation of reprehensible translations is commerce, whereas the ultimate explanation of debates about translation remains politics. This is not a simple world.

Agnès Whitfield observes Hugh MacLennan's literary work *Two Solitudes* not as an expression of the two sides of Canadian bilingual identity, but as the object of very different receptions by Québécois letters. The receptions were positive following the English publication in 1945, then far more critical following the French translation in 1963. The differences between the two moments would appear to be due to changes in Québécois society itself, which appreciated openness in the late 1940s but sought independence in the 1960s. And that explanation would probably be enough for all the sociologies of concentric circles. Whitfield, however, insists that there are not just two sides involved. The original novel was already a translative text, an English representation of francophone Quebec society, published in New York and aimed as much at the American as the Canadian public. Its French translation should thus take us back to a Québécois "original", except that it was published in France, according to French linguistic norms, at a time when European French as well as English were seen in Quebec as instruments of cultural domination. Whitfield's argument thus follows causal paths that do not allow us to stop at the simplicity of "new society, new reception". Indeed, part of the same complexity is found in the nature of Canadian academic disciplines from which the analyst speaks. Comparative Literature largely stays with the binary frame (one literature compared with another), whereas Translation Studies is seen as being methodologically able to reveal intercultural dimensions such as those unraveled in this case. Causation leads to the social, and we too are in the mix.

One final study in this batch, which fits in well enough with the intercultural import of Whitfield's approach:

Daniel Gagnon writes here in the first person as an author and self-translator working in the intercultural context of Canada/Quebec. Identifying his position within the social context of writers who are expatriates, exiles, or authors of multilingual texts, he compares his own work with that of Nancy Huston, another Canadian self-translator working between English and French. Gagnon observes that self-translation allows more liberties than does the translation of another's work. In fact, in both cases the translations into French won prizes as *original* works. Self-translation would thus raise many problems with standard notions about translation as passive representation; it might even question Moyal's critical insistence on "minimal norms". The awareness of social context is nevertheless found here in the complex subversive stances that the writer/self-translator can potentially adopt with respect to the standardized languages of colonialism.

Whereas Huston translates into the "imperial" French of France, as an incomplete displacement of her critique of North-American English-language culture, Gagnon seeks to free himself from French hegemony by writing an original English text as a playful foreigner. In both cases we find writers working in postcolonial spaces, from within overlaps mostly unseen in sociologies that would separate one culture from another.

At this point, that of the individual author/translator explaining individual practices, we have perhaps reached one of the limits of social explanation. Yet the writer's consciousness is in this case clearly expressed in social terms, as an interaction with a very specific context. This is by no means naïve "self-report data". Perhaps it is at this point, near the limit of sociology, that our studies should learn from what the practitioners have to say: there is more than just one society (language, culture) at stake, and the position of the mediator is not simple.

Symmetrical and asymmetrical correlations

We break off our survey in order to reconsider the question of explanation. We have asked if the allocation of observational and explanatory roles might ultimately be arbitrary. If our researchers tend to start from texts (as the things observed), is it simply because of tradition? Along the way, we have asked if the explanatory factor is really the cause of the observed, and not vice versa. We have also come across the idea of concentric circles, where the bigger circles would somehow be the explanation of the smaller ones. And we have encountered a few problems with that kind of explanation: Is it really a mode of causation? How sure can we be of its directionality? Does the concentric idea apply in studies involving more than individual societies? Let us now try to formalize those problems.

As we have noted, our basic observation-explanation relation might be of the kind "the more X, the more Y". To repeat our example, when a culture is accorded prestige, there tend to be many foreignizing translations from it (cf. Toury 1995). This is potentially a commutative relation, since we might equally propose that "when a culture has many literalist translations from it, it is accorded prestige" (cf. Kothari, in this volume). The prestige could be due to the translations, and the translations could be due to the prestige. The proposition is thus relatively symmetrical, to the point where it makes little difference which factor is observed and which is held to be explanatory (hence the commutative nature, as in addition: $3 + 2 = 2 + 3$). This is the kind of symmetrical relation that we might find in Kujamäki (the influential intermediary initiates the literary exchanges, and the literary exchanges develop the influence of the intermediary). That kind of model seems not to produce explanatory knowledge in any strong sense, not because its

dialectic is in any way false, but because any number of other factors could be involved as well.

What about Monteiro's basic proposition that "the translator's omission of religious details is due to the influence of the Inquisition" (our paraphrase)? Would we say that the self-censorship caused the Inquisition, or that the Inquisition caused self-censorship? Both ideas might make interesting pursuits (some of us spend our lives trying to reverse apparently obvious relations). There is some truth in the proposition that self-censorship (in translations and elsewhere) enabled the Inquisition to become a long-lived institution in Iberian societies, so that certain translation practices could indeed be seen as causes of the social institution. Indeed, it would be counter-productive for Translation Studies not to consider such causation. If translations were always effects rather than causes, they could not aspire to have any influence on the way of the world. They would scarcely be worth studying. At the same time, the Inquisition certainly had more influence on the individual translator Henrique Leitão than his omission had on the Inquisition. One factor is somehow bigger or of more weight than the other; it has causal connections with more other factors; it is more systemic. This is the kind of relationship we have been describing as concentric circles. We are now able to offer a slightly better description. The observation-explanation tandem in this case might still be possible in both senses, but one direction has more weight than the other. Let us call this "asymmetric correlation". Is there any causation at stake? No doubt there is. But the factors are interrelated with such complexity that we could not turn the hypothesis into a simple prescription for action. Or would we have perhaps advised Henrique Leitão to translate the details in order to get rid of the Inquisition? Smart subversion has subtler techniques.

One final example. Consider the idea that "feminist texts tend to be translated by women" (a passing proposition in Wolf, in this volume). The proposition would appear to be very asymmetrical, in something more than the sense just described. The nature of the texts would cause them to be translated by women, but the translators are certainly not women because of the nature of the texts. One factor seems sociological and fixed (the sex of the translators); the other appears more cultural and contextual (the nature of the texts). The sociological then causes the cultural. Or does it?

That entirely one-way explanation is of course an illusion. Do women translate feminist texts because they were born biologically female? We might more fairly claim that, in patriarchy at least, birth as a female entails a specific set of life experiences, and those experiences are likely to be addressed by feminist texts (more so than by non-feminist texts), at the same time as those texts help raise awareness of those same life experiences. Causation comes from both the social determinant *and* the cultural practice. Further, the two factors, falsely isolated in

our proposition, are connected by multiple chains of other factors, all interrelated in ways that none of us has time to describe in detail.

One-way causation is decidedly unfashionable. Marxists would claim that, in the last instance, the determining factors are economic (well, the relations of production in a society). Once we get down to who gets paid for what, we have reached some kind of bedrock; no need to dig further. It is surprising, in this respect, to see how rarely economic factors are cited in our studies (in the ones surveyed so far, only Thomson's analysis of East German censorship gives any weight to them, and Moyal only names them to dismiss them; they will however return in Wolf and Kothari below). There is an almost entire absence of factors like class and class-consciousness, which would be the social corollaries of relations of production. This might be because we know relatively little about that side of business (cultural discourses tend to hide their pecuniary implications). However, we are more likely to be distrustful of one-way determinism as such, and undoubtedly reluctant to analyze our own interests in those same terms. Our preferences are for models and sociologies that break class relations down into smaller and more complex terms. The weaker the causation, the greater the social complexity, and the more we can dissolve our responsibilities into toolbox approaches.

At this point some researchers choose to talk consistently of "conditioning" rather than causation. When there are multiple factors all in causal relations with each other (for some, this would constitute a system), we cannot say that the elimination of any one factor will lead to the elimination of all the rest. The presence of one factor, in a certain quantity, will certainly alter the nature of some other factor, but the change may be slight, even negligible. If there were no book fairs in Frankfurt, would the translations between Dutch and Spanish be the same? Well, not exactly. Then again, the change would probably not be enormous. Such weak forms of causation thus *condition* the observed; they are at best *partial* forms of explanation. Of course, if there were no correlation at all, if a change in one thing brought about no observable change in the other, then there is no causation to speak of. We might then talk about "relations" in a very general sense, of the kind that would place things together because the observer chooses to see them together (cf. theories of "relevance" as a fact of interpretation). Here, however, we are concerned with forms of empirical intersubjectivity that require something a little stronger.

How might we reconcile the traditional search for strong asymmetric causation (social determinism) with awareness of relatively weak multilateral causation (conditioning)? One kind of answer might come from India:

Rita Kothari observes that attitudes toward translation into English have become increasingly favorable in the state of Gujarat. Although translations were previ-

ously seen as an index of cultural inequality, different social agents now view them in positive terms. Official policy sees translation into English as a way of projecting Gujarati culture beyond its borders; a wide range of expatriates use those translations as a means of keeping in touch with "home"; educational institutions use translation as a way of opening new subject areas in literary studies; publishers use it as a means of tapping new markets.

The explanatory model here is one of different stakeholders reaching consensus (we would prefer to say "cooperation"), since translation into English apparently brings benefits to all. Here the world of economic interests is not only very present, but it is viewed in a less prejudicial light: publishers seek new markets, the government seeks investments from wealthy expatriates, academics seek new job positions. Yet there is no one-way determinism, as if one group controlled everything. Indeed, Kothari's explanatory model becomes stronger the more social groups are brought into consideration, since social causation here is founded on consensus between otherwise competing groups. Kothari is keen to point out that individuals may have many other reasons for turning to translation. Further, not all the stakeholders are located within the one society. What happens in Gujarat with respect to translation is to some extent dependent on what happens not just in the rest of India (positive attitudes to English seem to be found across the board) but also among Gujarati expatriate communities and, we might add, in changing international ideas about the nature of English literature itself, now a postcolonial field. In this kind of explanation, based on concepts of conditioning and consensus, there is no apparent need for concentric circles of any kind. Rather, the circles develop and intersect as we go, looking at one social group, then another, then another, on a basis that could only be ad hoc.

This approach presents problems of a very practical order. In the face of so many factors, where do we start? Where do we stop? One cannot do everything, of course. All our researchers have selected points of departure that are fairly traditional fare for Translation Studies; all of them have had to decide where to stop, if only for the sake of finishing an article. This means, first, that the selection of observational and explanatory factors is in each case a fact of the research design and not of any eschatology, as if the world had to begin in one place and end in another. It also means, second, that some priority has been given, in most cases, to the more *asymmetric* correlations, where causation is apparently more directional and salient, such that some degree of explanation may result – the wider concentric circles are still privileged. And it means, third, that all those explanations, no matter how varied or limited in scope, add something to our knowledge of the way translators act in the world. One cannot do everything, agreed, but the important point is to discover something.

Cultural or social?

We pause to consider a dilemma that has been quietly put on hold. We talk, too readily, about "sociocultural" or "social and cultural" approaches, contexts, factors, whatever. This is no doubt a cheap inheritance from the days when textbook Marxists (some as would-be subversives, others with little choice) cited mantras of "historical economic, social, cultural and political conditions" as explaining all phenomena, as a kind of generalized relativism. Are there any important particularities behind these adjectives? No doubt the "social" is also the "cultural", in the sense that both are opposed to the "eternal" or the "ontological". But why then do we need the two terms?

Academic tradition would suggest that "social" factors are the preserve of Sociology, whereas "cultural" factors fall into other disciplines (Anthropology, Ethnology, Semiotics, Communication Studies, plus the range of approaches these days roped into Cultural Studies). Fair enough. Yet are there any social factors or data that are not cultural as well? Or vice versa?

Two kinds of answer are possible here. First, let us consider the various factors used in the papers we have just summarized. Some of them would seem to pass as eminently social, at least in the sense that they can be handled by the methodologies of Sociology. These would include exchanges of letters within a social network, statistics on translation flows, censorship files, or economic costs of publication. Others would appear to be more properly cultural: translators' strategies, functions of literature, images of other cultures, the role of academic disciplines, or postcolonial discourse. Our own shortlist then has a few leftovers: the influence of the Inquisition, and support for one political regime or another. Both those factors are so clearly political that one hesitates to call them by another name.

On the basis of this small sample, we might surmise that social factors tend to have a quantitative aspect and can be associated with relations between people. Cultural factors, on the other hand, are more predominantly qualitative and can be related to signifying practices (texts, discourses). Indeed, some definitions of "culture" would have the term cover nothing but a set of signifying practices (cf. Hall 1997). They would moreover see those practices as constituting the identities ("subjectivities") engaged in the practices (variously after Althusser). If we are analyzing the way people converse or eat, we are handling the cultural side of life. However, if we analyze people in terms of ages, places of birth, or levels of education or income, we are dealing with social factors, not with cultural subjectivities. And if we are describing power relations, politics might be the measure of what we do. Note, however, that the one piece of information can be contextualized in more than one way. For example, the political influence of the Inquisition might be seen

as a practice imposing counter-reformist discourse (cultural fact), or as a shoring-up of the economic interests of a hierarchical social structure (social fact), or both at the same time. The longer we play in these waters, the muddier they become. And when we get to something like Luhmann's view of society as nothing but "communication", the very basis on which we would distinguish the social from the cultural has dissolved. Let us nevertheless risk a few transitory distinctions of a purely methodological or operational nature.

We find that *cultural* factors (e.g. language use or translators' strategies) tend to be the ones that are *observed* in our studies, whereas *social* factors (e.g. the social groups translators belong to) tend to be the ones used to *explain* the cultural factors. This schema seems to fit most of the papers just summarized, albeit not all (Linn at least starts from hard data on translation flows). We also find, even in our small sample, that a double movement is possible. Whitfield's paper moves from the cultural (receptions of a novel) to the social (changes in Quebecois society), as we would expect, but then returns to the cultural (a new academic discipline offers a way of reading not just the novel, but also the societies). Could one actually start from observation of the social? There is no reason why not. In search of illustration, let us summarize a further example:

Michaela Wolf sets out to look at women in German-speaking countries working for women publishers or women's book series. She surveys the opinions of translators and publishers, revealing the relative freedom that various editorial policies allow the translators with respect to visibility and such things as the use of inclusive language. Wolf finds a relatively close social network where women translators not only attain some visibility (their names are mentioned in the texts) and feel able to be creative in their strategies, but they also tend to accept low payment because of engagement with feminist causes.

Wolf's study merits special attention here because it aims to be almost purely sociological, drawing its concepts from the work of the French sociologist Pierre Bourdieu. Social factors define the object of knowledge (women, publishing institutions), just as empirical sociology provides the data-gathering procedures (questionnaires, interviews). Wolf, however, seeks to "explore the factors conditioning the production of female and feminist translation". In those terms, the explanatory ("conditioning") factors would be social, and the thing explained would certainly appear to be a cultural practice, as in our baseline model. Bourdieu's concepts nevertheless enable us to extend the sociological deep within the cultural. The production of these translations is seen not just as a signifying practice or a use of language, but as a potential "field", where the various agents form power relationships and deploy their individual "capitals". People moreover act within this space in terms of their "habituses", dispositions that they have acquired and internalized

over time. Cultural practices are thus cut up into social relations (field) and behavioral dispositions (habitus); the cultural becomes at least sociological, if not wholly social. Further, applying Bourdieu, the capitals people deploy can be economic (money), social (networks of contacts), symbolic (prestige, fame) or, of course, cultural (education, competence in signifying practices). These are the terms that actually allow Wolf to explain why the women translators accept low pay. For them, cultural capital has a higher priority than economic capital; the translators work for the cause more than for the money. So the specific weight of cultural practices actually returns here, like the suppressed, to explain the social configuration of the field (and indeed to enable Wolf to question the existence of a translation field at all, a problem to which we will return). If we had to summarize Wolf's methodology, we might say that she starts from the social (to define the field), identifies the cultural (describing a cultural practice), does a sociological analysis (the world according to Bourdieu), and finds her closing explanation in the trade-offs of cultural capital. As in Whitfield, the movements are doubled, but certainly do not cancel each other out.

Let us propose that, in the studies such as we have them, a kind of explanation moves from the cultural to the social, and/or from the social to the cultural. Where there is no such movement (i.e. the social simply correlates with the social, or the cultural with the cultural), then our desire for explanation seems decidedly less satisfied; we enter into illusions of simple description. That might partly explain the diffuse discontent, at least in continental Europe, with Cultural Studies as a discipline, often felt to be lacking in discovery procedures. If translators are seen as nothing but discursive and observational figurations ("the translator in the text") and their social institutions are analyzed in the same terms (be it from Foucault or semiotics), then the sociological has lost its explanatory position. Rather than produce explanatory knowledge, such research struggles to do more than confirm a certain view of the world. This could be one reason for distinguishing general Cultural Studies from a more empirical brand of "culture research" (the term currently used at Tel Aviv University).

Then again, we should resist the obverse illusion that the real explanations only come from a wider and better-established discipline called Sociology. There is something of this not only in Wolf (where the main terms are to be understood only as Bourdieu apparently understood them), but also in standard references like Hermans (1999), where the future of systemic approaches to translation is sought in a combination of Bourdieu and Luhmann. The very existence of double movements suggests that the cultural cannot entirely be reduced to a set of classical sociological variables. We are no longer in a world where the hard countable facts (the economic relations of production, for example) explain away cultural practices. We know that the isolation and counting of the facts is itself a cultural

practice – the sociology of culture takes place within a culture of sociology. In Bourdieu's terms (cf. 1980: 19–36), we must still subjectivize the objective (we must ask who is selecting and analyzing the object, and why), as well as objectivize the subject (we must do the sociology of the people carrying out research). In Australian terms, the boomerangs we throw at others will come back at ourselves. There is something of this in the present volume. Agnès Whitfield's piece is perhaps the clearest example, as mentioned, although Gagnon is engaged *ex officio* and Kothari and Wolf do not hide their positioning. Yet there could be much more; our empiricists could look more closely at the rationales behind action research.

This perspective should enable us to adopt a critical view of the more traditional sociologies we draw on, especially since there are various sociologies to choose from. For example, we have found that Kothari's model of explanation is based on consensus between social groups, whereas Wolf, following Bourdieu, believes that a field should be formed by struggle between agents. As we have said, what Wolf actually finds is a trade-off between economic and cultural capital, between publishers and translators (the latter accept less pay in return for a higher profile and engagement in the cause). That would be a basic form of cooperation. It may not correspond to what Bourdieu wanted to find in a "field" worthy of the name. However, it could be what we mostly find in intercultural practices, given their shared marginality.[1] Further, such cooperation could be ethically laudable, over and above the constant struggles that the French sociologist saw in the societies around him.

Cultural and social?

There is a second way of handling our quiet dilemma about social and cultural factors. Social factors tend to concern societies; cultural factors have to do with cultures. This is of interest for the simple reason that societies and cultures tend not to be co-extensive. We can find many cultures within the one society (we talk freely about "multicultural societies"), just as we can find the one cultural practice in many different societies (monotheism, vegetarianism, *jus solis*, or soccer, for example). There is admittedly some sleight of hand here. Social factors might equally apply to such things as peer groups or scientific communities, which are much smaller than nation-state societies. The non-correspondence between the social and the cultural is nevertheless a useful reminder of where our concepts come from.

Nineteenth-century sociology developed for the description of European nation states, with the idealist assumption that each had its distinctive set of cul-

tural practices (cf. Wallerstein 1997). Hard-core sociology tends to maintain that allegiance. Governmental or intergovernmental agencies are the best places to get good statistics (on book production for example), and so the nation state is bound to be a unit in such analyses. The same agencies tend to be the ones that form policies, so they might be the only readers that could enable our research to influence large-scale cultural practices in any direct way. Twentieth-century sociologies certainly paid attention to progressively smaller social groups, yet the discipline as such has little political interest in dissociating itself from description and prediction on the level of the nation state.

When we do the sociology of translation, we are necessarily challenging the kind of sociology that would stay at the level of just one nation state, or perhaps two. The Marxist translation theorist Otto Kade instinctively adhered to the national frame when he attributed translation problems to the non-correspondence between "two historically developed societies" (cit. Koller 1979: 156). For Kade, if we study entire societies, we will be able to explain why it is hard to translate between them. For most of our current researchers, however, there is little need to survey anything like national societies, with their classically concentric circles of social determinism. The cultural practices that concern us are mostly of different (smaller or wider) dimensions, and their explanatory factors are rarely concentric, simply because the practices cross boundaries. To transform Kade's terms, the relations of cultural production, in the case of translation, are never entirely within just one historically developed society. The work of translators and interpreters necessarily cuts across the lines between historical places like "France" or "Spain", even when those places are themselves reinforced by ideological reductions of a cultural nature (the assumption of common languages and cultural practices). We thus find in some of our studies that the materials themselves, the very things we talk about, necessarily challenge the national frames of much sociology.

Another of our papers may be taken as a case in point:

Ieva Zauberga observes recent changes in the strategies used for the rendition of foreign proper nouns in Latvian. Traditionally, Jacques Chirac would be written as *Žaks Širaks*, whereas the new tendency is to allow him his French spelling. This is seen as a challenge to Latvian cultural orthodoxy, associated with linguistic purism and implicitly with stable national identity. The change is to be explained as a challenge to the national level, in terms of something that is happening to Latvian society as a whole. Causation is, naturally enough, attributed to economic globalization, bringing increased trade, travel and access to information. The new transcription strategy is needed so that Latvians can recognize foreign terms when they are abroad, or when they use search engines on the Internet, and so on. However, that level of explanation is itself cultural in essence: the new renditions are

needed because of new cultural practices that extend well beyond national bound-
aries. A more sociological causation is then attributed to the actions of translators
and publishers, whose agency is explicitly recognized. Change is thus brought
about *because of* cultural factors that no longer correspond to the nation, and *by*
the social groups directly involved in cross-cultural communication.

Studies like Zauberga's operate at levels at once wider than the nation state in
terms of cultural practices (e.g. globalization) and smaller than the nation state in
terms of sociological dimension (e.g. translators and publishers). This gives Trans-
lation Studies, and more generally Intercultural Studies, a critical potential that
might connect easily with similar views from postcolonial approaches and theo-
ries of globalization, where the other is seen within the self. On many counts, the
national frame no longer provides adequate explanations. Translation Studies
might usefully go with that trend, searching for the ways cross-cultural communi-
cation connects with emerging world systems. At the same time, we might direct
our attention to the actual contact situations, to the quite small social settings in
which translators and interpreters actually work. Our frames can be wider, or nar-
rower, or both (as in Zauberga).

Our more literary studies might be expected to drift toward the supra-national
level, since such things have been in the repertoire of literary theory at least since
Goethe. More decisive, however, might be the frames used in research on inter-
preting, to which we now turn.

Learning from Interpreting Studies

As much as we personally include interpreting in our own usage of the term
"translation" (which for us covers both spoken and written forms), there are cer-
tainly social reasons for looking at Interpreting Studies as a separate category. The
research community is quite different in the case of interpreting, with very differ-
ent relations with disciplines like neurology or psycholinguistics. Almost instinc-
tively, we associate interpreting with questions of either individual performance or
work in relatively small social settings, ranging from interviews to conferences.
That, and many similar assumptions, are clearly challenged by the paper closing
this section.

Franz Pöchhacker gives a wide-ranging presentation of how the main ideas of
Interpreting Studies have developed in recent decades. Alongside several long-
standing paradigms, he finds growing awareness that interpreting involves more
than conference interpreting. This shift not only focuses attention on the diverse
social contexts in which interpreters work, but also challenges several *partis pris*

with respect to the defense of professional standards. Why has the new frame developed? Pöchhacker only intimates that it might have something to do with a new generation of researchers. He might also have added that the forces of migration in a globalizing world have increased the social demands for interpreting in the public services of receiving countries. Hence the key ideological role of what is known as "community-based interpreting" (or "community interpreting", or "social interpreting", among other names). The social commitment of researchers can only develop when there are pressing social problems to resolve.

The supra-national might thus be seen as shaping the new objects of interpreting research. The analytical frame does however tend to remain at the microcosmic level, staying close to the concerns of discourse analysis and pragmatics. As Pöchhacker notes, the linguistic paradigm has been strong since the 1970s. But new paradigms are now at work. The following are summaries of the papers on various specific aspects of community (or "social") interpreting:

Sonja Pöllabauer presents a study of interpreting at asylum hearings in Austria. By paying close attention to basic pragmatic features such as facework and footing, she finds that the interpreters tend to cooperate with the interviewing officers but not with asylum seekers. The interpreters thus operate as "auxiliary police officers". This discursive positioning is attributed to the asymmetrical power distribution of the hearing situation, and to the corresponding "translation culture" (we will comment on the concept below), which in this case favors one-sided loyalty and self-protection rather than absolute communicative transparency.

Nadja Grbić presents a comprehensive empirical study of sign-language interpreters in the region of Styria, in Austria. The study focuses on the professionalization of the sector in terms of academic training, qualifications and the development of an association. The provision of academic training is found to have had considerable impact on the social matrix within which a profession is exercised. Grbić nevertheless asks to what extent the resulting network constitutes a social system, here in a sense where coordinated interaction between members allows them a "common construction of reality" (hence the concept of "constructivist" systems). That question proves difficult to answer. What we do find, on the basis of empirical sociological research, is that the norms of this system are unstable and contingent, none the least because interpreters have credentials in a range of different social systems.

Guillermo R. Navarro Montesdeoca deals with a neighboring field, describing his own experience as an interpreter at an Immigrant Detention Center in Las Palmas de Gran Canaria, Spain. As in Pöllabauer, we discover a strongly hierarchical situation where communication is marked by power relationships. Here,

however, the interpreter places himself well down in the hierarchy, no doubt because he is not employed on a permanent basis and his professional qualifications are not recognized.

Mette Rudvin offers a plethora of ideas on how identities are built and negotiated in interpreter-mediated encounters. Once we adopt the position that "cultural/ethnic identity is made manifest in language", the floodgates are opened to any number of references concerning culture, language, discourse, text analysis, and repeated confirmation of the point of departure. Health-care services are thus seen not as a social institution in the hard empirical sense but as a cultural system, a set of signifying practices, of which interpreting becomes a part. Rudvin detects processes of dehumanization in these discursive formations, seeing certain types of frame-switching as means of resisting that trend. Is the interpreter condemned to reproduce the institutional dehumanization of patients? The answer is not as clear as it seems to be in the empirical studies. The theories of Cultural Studies allow ample space for resistance.

Katrien Lannoy and Jan Van Gucht present the findings of a commissioned survey of interpreting and translation services provided to social welfare institutions in Flanders. Service providers and users were observed and quantified through interviews and questionnaires, using classical sociological methods. We learn, among much else, that non-professional interpreting plays a considerable role, along with communication strategies other than interpreting. Most encounters involve "use of a common contact language, followed by the use of simplified Dutch and gestures, and then the use of friends and relatives or untrained colleagues as ad hoc interpreters". Although a surprising number of users are quite content with such non-professional services, one of the key factors is the financial cost of services. The authors recommend that telephone interpreting services be kept free of charge for social welfare institutions, that the administrative categories not confuse telephone interpreting services with general community interpreting, and that training programs assist in the professionalization of the field.

The differences between these papers are extreme and instructive. At one end, Lannoy and Van Gucht are doing straight empirical sociology, within an overtly national frame (as a report for a government). They bundle together what people say and do; they provide quantitative findings so that the corresponding administrative action can be taken. We thus buy into the power of sociology to speak on behalf of collective social agents. This is present nowhere else in our volume. At the other end of the spectrum, Rudvin adopts what we see as a Cultural Studies approach, which has no real need of quantitative data at all. And yet, in Lannoy and Van Gucht the conceptual leaps from the empirical data to the recommendations remain precarious. Their conclusions do indeed concern questions of profes-

sional identity formation, questions for which they do not present appropriate methodological tools. So much else is happening on the level of interpreting as a social practice that one could only see Rudvin's approach as not only complementary but even necessary. As we have said, the social needs the cultural, just as the cultural can still seek explanation in the hard core of empirical sociology.

In between these extremes, each of the above papers achieves its own distribution of the social and the cultural. Pöllabauer observes linguistic phenomena and attributes them to certain assumptions concerning social agents, but she does not have any particular interest in the sociological identity of the agents as such. We are given no information on variables such as the interpreter's mode of employment, qualifications, distribution of financial resources or ethnic provenance, all of which could be key factors in the case of Grbić or Navarro Montesdeoca. In Pöllabauer, observation and causation thus remain on virtually the same level. We find something happening in the discourse (interpreters side with the interviewing officers) but we do not know why this happens. The mode of analysis is nevertheless not without interest, basically because it draws on the concept of "translation culture" (on which, comments very soon).

In all these papers, implicitly or explicitly, there is a search for a conceptual frame located somewhere between the whole of society and the linguistic situation, between traditional sociology and close-range cultural analysis. This is where we try to extract a general conclusion from the above contributions.

New dimensions for cultural sociologies?

We have formulated the principle of asymmetric causation for a very simple reason. If translations and translators were wholly explained by sociocultural factors, then they themselves would logically be unable to cause any changes in the world. Our object of study would be without influence, without effect, without power. Indeed, we would soon be back to the days when translations were seen as powerless because they were written off as mere reproductions of sources. In allowing for asymmetric causation, we insist that translation is more productive than reproductive. Translational phenomena are partly the causes of other phenomena, even though their agency often seems lesser than the wider factors.

One of the problems with this principle is that translation, as the thing to be explained, is often approached not just as a passive object but also as a factor that is inherently smaller than the explanatory variables. Translational things have relatively reduced dimensions; social relations are larger; societies and cultures are larger still (i.e. they enter into more causal relationships). Many of our researchers instinctively focus on quite particular observations: a translation, a mediator, a

text and its translations, a period, a country, one culture receiving another, and so on. This need not be so, of course. We might also observe translational phenomena as spanning centuries and continents, as constituting a major driving force in cultural history. If translation can thus be made a much larger factor, assumptions of one-way causation would be much harder to maintain. A pro-active view of translation becomes all the more possible.

Research on those wider levels is difficult to set up with any degree of rigor. Yet it is not impossible. A step in that direction is the study of translators instead of individual translations, since the human agent necessarily brings together several social and cultural fields. A second step would be to see the object "translation" not just as a set of texts or actions, but of principles that underlie texts and actions over considerable stretches of history and geography. The focus on norms was a major advance in this direction. Our papers in this volume show that further attempts have been made to see translation as a wider, richer object of knowledge.

Pöllabauer, as we were saying, does not seek to reduce everything to sociology. Instead, she tries to describe a certain "translation culture" (rendering the German *Translationskultur*). That term, developed by Erich Prunč (1997), is defined as the "variable set of norms, conventions and expectations which frame the behavior of all interactants in the field of translation" (Pöllabauer, referring to Prunč 2000: 59; cf. also Pöchhacker 2001 and comments in this volume). The concept is of definite interest. It is more dynamic than the similar term "translation culture" (rendering *Übersetzungskultur*) used by the Göttingen group (see Frank 1989) to describe the cultural norms governing translations within a target system, on the model of *Esskultur*, which would describe the way a certain society eats. Prunč's notion of a translation culture is of something that is constantly evolving, and in which both translator training and Translation Studies should be actively engaged. There is some doubt as to the exact location of Prunč's translation culture, since another definitional frame locates it as a "historically developed subsystem of a culture" (Prunč 1997: 107), without saying precisely what that larger "culture" is supposed to be. However, read in terms of its internal elements, the concept of a translation culture need not be extended to a national frame; it could remain the preserve of only those agents involved in the translation process; it might thus be of rather reduced dimensions, potentially straddling national boundaries. That is indeed where we would like to position it.

With this minimal specification, the concept of a translation culture opens an interesting space. It could be made to occupy similar dimensions (in time and space) to Bourdieu's notion of habitus, if and when we can ask critical questions as to how and to what extent individuals might internalize a translation culture. The notion of a translation culture might also be made to speak to Wolf's search for a space that is not a full-fledged sociological field in Bourdieu's sense, if indeed we

could make it explain how the principles evolve interactively, or how they relate to what happens beyond the translation culture. The concept might even be brought close to Grbić's preference for a constructivist social system that is decidedly weaker than what is ideally required in sociology (a translation culture apparently does not have mechanisms for self-reproduction, boundary maintenance, or the prolonged stabilization of norms). At all these points, our researchers have felt uncomfortable with translation as a small thing. They have made it a larger thing by trying to extract its underlying principles, as indeed would a sociologist when studying a group or society. Yet, *nota bene*, our researchers seem not to have made translation a sociological object in itself. They have intuitively had recourse to a terminology of what would appear to be cultural factors (a translation *culture*, indeed).

For us, "translation culture" could be a rough synonym for a "translation regime", understood as a set of "implicit or explicit principles, norms, rules and decision-making procedures around which actors' expectations converge" (Pym 1998, modifying Krasner 1983: 2). We borrowed the idea from negotiation theory, from a cultural practice that necessarily takes place on the frontiers of the major social systems. Others appear to be going the same way, on the basis of quite different models and data. We would see all these initiatives as leading toward the conceptual terrain of what we have termed "professional intercultures" (Pym 2000, 2004), formed and deployed by the people engaged in cross-cultural communication. This is not the place to go into the theory of intercultures. Let us simply insist on the word "culture" that forms part of our terms ("inter*culture*", "translation *culture*"). For as much as the study of intercultures has been associated with quite sociological questions like membership, provenance and power relations, we have wanted it to remain closely attached to sets of qualitative factors as well (the stuff of "regimes"). We are not talking simply about the social groups or communities that straddle borders; we are not setting out to do straight quantitative sociology. Perhaps like Prunč, along with most of the authors in this volume, we have hesitated to reduce the cultural to the social.

What kind of sociology, if any, might best help us explain translational phenomena? In lieu of conclusion, the following is a wish-list concocted from what we have found in this volume, and what we would like to find in future research:

- Our sociology should be able to focus on mediators, not just on the social aspects of source texts and target texts.
- It should resist the simple binarisms that oppose one society (language, culture) to another, with the mediator on one side or the other. It should be able to perceive overlaps and complex positions.
- It should embrace both cultural factors (usually qualitative) and sociological factors (mostly partly quantitative).

- It should be able to explain as well as describe.
- It should seek explanation by moving between the cultural and the socio-logical, without according absolute explanatory status to either side.
- It should be able to relate factors in terms of asymmetrical or relatively symmetrical correlations, through hypotheses that model causation or multifactorial conditioning.
- It should not pay undue allegiance to heroes imported from Sociology (or from any other discipline for that matter).
- It should be able to work from a plurality of concepts (translation cultures, social systems, regimes, intercultures) appropriate to the social spaces in which intermediaries work.

Beyond that, the field is still very open to creative research. Much remains to be done before we can hope to offer any general explanations of cross-cultural communication. The challenge, however, remains constant. The most problematic relations of today's world are between cultures. To model those problems is our first step toward solving them.

Notes

1.   Both Wolf (herein) and Simeoni (1998) raise serious questions about whether translation can constitute a "field" in Bourdieu's sense. According to the evidence they present, the activity of translators would be too unstructured, too subservient to other fields, and too ill-equipped to ensure its own reproduction and boundary maintenance. Translation would then somehow be unworthy of proper sociological status. Much depends, however, on the kind of evidence one looks at. The growth and hierarchization of translator-training institutions, for example, might be seen as the development of a field. Further, within smaller societies the role of translation would seem proportionally greater, to the extent that many of the attributes of a field are in fact fulfilled (as argued by Czech students at our doctoral seminar in Prague in September 2004). The conclusion that "translation is not a field" might not only turn out to answer a subservient question (why should a sociologist provide the yardstick anyway?) but could also be a result of looking at major rather than minor cultures.