1
Translation, epistemology
and cognition

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1.1 Introduction

The human brain, writes Nobel Prize winner Gerald Edelman (1992, p. 23), is “the most
complicated material object in the known universe”. Note the careful “known”. After all, it
seems that currently we can only perceive about 5% of the universe, the rest being mysterious
dark matter and equally mysterious dark energy (so we lay folk are told, at least). Edelman’s
qualification “known” is thus eminently justified, and a useful reminder of the virtue of modesty
in making claims about the world. But in what sense, and to what extent, do we even really
“know” that 5%?

How much do we know about the human brain? Less than 5%? Do we even know how
much we don’t know?

And further: what do we think we know about “knowing”? From Plato on, philosophers
have usually started from the assumption that “knowledge” is justified true belief; but such a def-
inition carries a number of problems and has given rise to a good many debates. (For a relatively
reader-friendly survey, see Steup, 2018.) The debated issues include the questions of how we can
know—for certain—anything at all, what counts as justification (and for whom), and of course,
what is truth. These are problems of epistemology, the philosophical study of knowledge. In the
present context, my rather sceptical focus will be on the limitations of knowledge and of our
methods of acquiring it. My position is thus a fallibilistic one: I assume that when we think we
know something, we may be mistaken.

So, when Krings (1986) asked “What goes on in translators’ heads?”—in one of the most
influential early publications in what was to become the “cognitive turn” in Translation Studies
(TS)—we can at least answer: well, it is unlikely that we will ever know completely. But we
might get to know something, and this something might eventually “evolve” into greater know-
ledge, better descriptions, better explanations and better understanding.

Suppose I introspect for a minute: what happened in my head when I just wrote what goes on in translators’ heads? For this is not what Krings actually wrote, which was: Wäs in den Köpfen von Übersetzern vorgeht. Actually, I first translated this as what happens …. But then I paused, and changed what happens to what goes on. Why? I’m not sure: perhaps it sounds more informal? Perhaps I was unconsciously influenced by the morphology of the German vor-geht? Would this
interference hypothesis be strengthened by other examples of me being influenced by German morphology when translating into English? What about my translations from (or indeed into) other languages? Tests could be done … Or was the change just due to intuition? The same kind of intuition that prompted me to write in translators’ heads rather than in the heads of translators, which would have followed the German form more literally? I just don’t know, however hard I try to introspect and to retrospect on what I just did. I do know that in the first line of this paragraph I wrote happened in my head rather than went on precisely because in the previous paragraph I had just shifted happens to goes on and did not want to repeat the same lexical item so close to the previous mention. But if I had not made this shift, would I have written goes on in my head? Again, I don’t know.

As I continue my retrospective introspection, the thought occurs that there is actually a semantic difference between happens and goes on (when neither is used in a progressive form), such that happen co-occurs more with events and go on with states or processes. (What happens when I press this switch? What goes on when the light is out?) I find this thought supported by a quick Internet search. But I was certainly not consciously aware of it when I made my change. Was this just my native language intuition, working below the level of consciousness? Perhaps. But why, then, did I write happens in the first place? Because I have lived for several decades outside an English-speaking culture and have lost touch with some of the niceties of my native English? And further: how do you know that I am introspecting and retrospecting, and reporting, truthfully? On what grounds do you trust me (or not)?

This trivial example illustrates just some of the problems faced by cognitive research into the translation process: problems of causality and explanation; the reliability of different data sources; the plausibility of conclusions about decision making and choices, and about the possible influence of earlier choices on later decisions; accessing the possible roles of the unconscious and of intuition; and even perhaps the influence of personal life history.

1.2 Historical perspective

A sense of the historical development of research on translation and cognition can be framed as a series of epistemological assumptions. Initial assumptions are always necessary, of course; but they may turn out to be misleading or mistaken, or only partially justified. We can start with the idea of the translator’s mind as a black box, with an input arrow on one side and an output arrow on the other, and a question mark in the box itself. This box model itself reveals an initial assumption: that we can learn something about the mind by considering it in isolation from its environment and from its own history, both phylogenetic and ontogenetic. Such an assumption can of course be defended as a necessary heuristic preliminary at the early stages of an investigation, a useful simplification. True, if it is borne in mind that it is just such an assumption; otherwise, there is the risk that the simplistic map is actually taken to be the complex territory it represents.

Looking at this box, first, we just wonder and speculate—surely where all science starts. We can observe what is going in and coming out, and attempt to draw inferences from what we see. The “interpretive school” of interpreter training (based at L’École supérieure d’interprètes et de traducteurs (ESIT) in Paris) arose from the initial inference that good interpreting (and translating) requires the deverbalization of the incoming message, so that its “sense” can be separated from its form and then reformulated in the target language (Seleskovitch & Lederer, 1984). This view of the cognitive process involved was based mainly on experience and intuition, and led to successful training methods that produced excellent professional interpreters. But it was not supported by empirical research on cognition, and the status of the deverbalization phase
was challenged, e.g. by research supporting the “literal translation hypothesis”. This claims that interpreters and translators are highly influenced by the form of the source text and tend first to select target forms that match those of the source when possible. If this is the case, it would imply that deverbalization does not take place—or at least not always. Interesting evidence supporting the literal translation hypothesis comes from studies of interim drafts and the revision process (see e.g. Englund Dimitrova, 2005). The difference between these two positions may be partly explained by the ESIT focus on what should happen in ideal interpreting, and therefore on what would be pedagogically relevant. But inferences may also be mistaken.

Continuing to ponder our black box, we might assume that our translator’s mind contains a number of smaller, interconnected boxes (modules, components) with different functions: one deals with comprehension, for instance, another with target-language formulation, yet another is perhaps engaged in some kind of quality checking, etc. So we assume that the mind is modular, or at least that it is useful to regard it as such at the initial stages of an investigation. The image is of the mind as some kind of machine, with various components all doing their own jobs, converting input into output.

Then we try manipulating the input and see how the output changes: i.e. we adopt an experimental method. We also begin scratching the surface of the box, with the idea of getting below the surface, at least to some small degree. We thus assume that by entering the outer layer of the brain we can begin to glimpse the workings of the mind: we can, for instance, use EEG (electroencephalography) or PET (positron emission topography) and examine which bits of the brain appear to be activated under different conditions (e.g. Kurz, 1994; Tommola, 1999). Some later research, however (e.g. García et al., 2016), has cast doubt on the idea that the brain has areas that are dedicated specifically to translation or interpreting.

But do these methods give us any knowledge about the mind? Is it reasonable to assume that the brain is a window on the mind? Not a transparent one, at least, it would seem. The huge mind–brain issue (or more generally, the mind–body issue) has not been resolved to general satisfaction, although a strict dualist position seems now to be out of fashion. The jury is still out and may well remain out for some time to come. Indeed, perhaps this issue is actually not solvable at all. (But see later.) There is, however, the risk that we too easily assume that new information about the brain tells us something about the mind. It may, but it may not: reservations are needed.

From the mid-1980s on, Krings and others experimented with a new way of collecting data on what were assumed to be the cognitive processes involved in translation. This was introspection in the form of spoken think-aloud protocols (TAPs). The method had previously been used in psychology (see Ericsson & Simon, 1993). Part of my brief introduction earlier was a kind of thinking aloud, not spoken in this case, but slightly delayed and to some extent monitored before being expressed in writing. In my case, I was retrospecting; but in the protocols used by the early TAP scholars the idea was to have the subjects talk aloud at the same time as they translated (usually working on a computer, with a separate recording machine).

It was widely recognized that the TAP method had limitations (it was obviously not appropriate for research on interpreting) and flaws. Assumptions were made that now look hard to justify. Critics pointed out that the very act of talking aloud would surely interfere with the normal cognitive process that was allegedly being studied. Indeed, the very act of being observed may have interfered (the observer effect). How reliable would the protocols be? Wouldn’t they be rather selective, focusing only on what can be verbalized, and within that, on translation problems at the expense of routine flows? How much of what goes through the subject’s mind would actually be verbalized? And wouldn’t the TAPs actually concern the results of cognitive processes rather than the processes themselves? Such criticisms were often discussed, and often acknowledged, but TAP research nevertheless went ahead. Attempts were made to rectify some
of the problems, for instance by setting up TAPs with pairs or small groups of subjects, on the assumption that this would allow a more natural dialogue rather than a possibly unnatural monologue. TAPs were also combined with retrospective interviews. Yet despite these developments, the numbers of subjects studied in each project remained very small—for the obvious reason that this kind of research is extremely time-consuming. (See Jääskeläinen, 2002, for a survey.)

Further refinements added new kinds of data gathered from new kinds of technical tools, the most noteworthy being Jakobsen’s Translog keystroke logging program, and later the application of eye-tracking technology. (See e.g. Jakobsen, 2017.) Here too there are interesting assumptions. Now, for instance, it is the eye that is assumed to serve as a window on the mind. There is also the assumption that when the subject pauses, i.e. stops typing for a few seconds or minutes, this absence of observable activity is a significant indication of non-observed cognitive activity, such as grappling with an awkward translation problem (rather than, say, day-dreaming). One big advantage of these new methods was their use in triangulating results, e.g. cross-checking the results of a TAP with those of keystroke logging and/or eye-tracking data and/or a retrospective interview. These innovations allowed more specific hypothesis formulation and more robust hypothesis testing (for instance of the above-mentioned literal translation hypothesis). TAPs have suggested significant differences in the way professionals or experts translate in comparison with students or untrained translators. They have also generated promising new lines of research, such as on the relevance of attitudinal and emotional factors, which have further complicated our original image of the black box. Much of this research has been methodology driven (we do it because we now can) rather than theory driven (e.g. to test theoretically significant hypotheses) or problem driven (e.g. to solve translation quality problems). It has often been exploratory: let’s do this, and see what we can find. This is not to say that such research does not bring new knowledge, but such knowledge may appear fragmentary if its theoretical framework is unclear. Observation is used to test theory, yes, such as when hypotheses are tested; but theory is also needed to make sense of what is observed.

Research into the cognitive processes involved in translation has been increasingly influenced by aspects of the related fields of psycholinguistics, cognitive linguistics and cognitive science. Translation is a complex activity, true, but on what grounds might we assume that the nature of translators’ cognitive processes would be significantly different from those of electricians, or dentists? Apart from the factor of the two languages, of course. More recently, “Cognitive Translation Studies” has been launched as a cover term for a broader framework of study that would open up to other relevant fields such as bilingual research and language acquisition, and more broadly still, neuroscience. (See e.g. Muñoz Martin, 2014, 2016a.)

We started with the assumption that the translator’s mind could be initially conceptualized as a black box, and it has seemed that the study of translation cognition also started with a similar image of itself, i.e. as a self-contained field. In both cases, such assumptions have now been largely abandoned or become superseded by others. This in itself is of epistemological significance. As noted earlier, it is well known that we need concepts in order to interpret what we observe. It is also well known that our concepts and models, including our heuristic metaphors, influence the way we make sense of what we observe. If you see the brain as a machine, you will be disposed to think of it in terms of components and functions, etc. If you see it as an information processing computer, you will visualize algorithms and programs. Indeed, several of the first models of the translator’s mind were pictured as flow diagrams that were algorithmic in form, from Krings onward. More recent work influenced by computational linguistics also attempts to model human translation processes in computational terms. (See e.g. Carl & Schaeffer, 2017.)

But suppose, like Edelman (1992, p. 29), you see the mind as a jungle, a rainforest? What would then follow? At least this: that everything has evolved in connection with everything else,
in a complex environment including emotions, history, evolution, material factors, social and cultural factors, and much else. Such an image would be consistent with the position known as embodied cognition. (See further later in this chapter.)

This is an empirical position: research on translation and cognition has indeed been dominated by a general empirical paradigm, as opposed to the postmodern, relativist paradigm, which has become prominent in some other areas of Translation Studies (see Delabastita, 2003). The former view takes TS to be an empirical human science rather than a branch of philosophy or literary theory. On this view, it is at least possible to strive for neutral and objective knowledge, although we know (or assume) that this ideal is ultimately unattainable.

1.3 Core issues

1.3.1 Different concepts of theory

Knowledge evolves. This implies that even our concepts of what knowledge is, or what a theory is, presumably also evolve. If you have a broad view of what a theory can be, you can even see myths as a kind of pre-scientific theory, offering at least some kind of explanation for phenomena such as the cycle of the seasons or the causes of volcanoes (cf. the myth of Vulcan’s forge). Metaphors too can be seen as a kind of primitive theory: recall the many metaphors that have been proposed for the act of translation, from the dominant but problematic transfer metaphor to hundreds of others, as a “way of seeing” translation (cf. the Greek etymology of the word theory). In the general empirical paradigm, one can take hypotheses, too, as mini-theories, hopefully related to a larger framework of descriptive and explanatory propositions that together make sense and perhaps allow prediction.

But how far can we go without such a larger framework? The so-called “new experimentalist” position argues that progress can be made without claims being necessarily embedded into a large-scale theory. Experiments that work, i.e. that have the predicted effect and are corroborated when they are replicated, remain valid evidence even though no-one can explain why they work. (See Chalmers, 1976/1999, Ch. 13, for a philosophical discussion of this point.) This view does not claim that we do not need theory, but that good progress can sometimes be made before theory can catch up, as it were. This seems to be what is happening in some contemporary work on translation cognition (e.g. Carl et al., 2015). On this view, then, tentative progress is first made inductively, only later being integrated into a theoretical structure from which further hypotheses can be formulated deductively.

1.3.2 Interpretive hypotheses

Another question concerns the role of purely conceptual analysis in empirical research. Definitions (e.g. of “cognition”), categorizations, and interpretations of X as Y can all be taken as hypotheses of a kind, sometimes known as interpretive hypotheses (see further Chesterman, 2008a). I have just claimed that definitions etc. can be taken as hypotheses: this itself is an interpretive hypothesis, in my view at least. Not all scholars agree with this characterization, which indicates that interpretations can always be argued over. As interpretations, however, they cannot be falsified (cf. Popper’s criterion for scientific knowledge, e.g. Popper, 1959). They are not empirical claims but conceptual ones. However, they can certainly be tested: not on the true-or-false criterion but on the pragmatic one of usefulness. A useful interpretive hypothesis has added value in that it brings clarity, generates significant empirical research, stimulates further questions and so on. An interpretive hypothesis that turns out not to be useful simply fades away.
This testing process is epistemologically just as important for conceptual claims as for empirical ones; however, this testing is often neglected. Conceptual proposals are often made, and supportive illustrations given, without adequate account being taken of counter-examples to which the proposal does not seem to apply, and without specification of the added value of the proposal in question—its advantage over competing proposals, for instance. One advantage of the “interpretive hypothesis” proposal is that the term “hypothesis” relates these conceptual claims to empirical hypotheses and hence to the necessity of testing them, albeit on different criteria. That is, the term underlines their status as precisely hypotheses, not facts or truths. There is always the risk of presenting a conceptual distinction (such as “there are three kinds of translation”) in a way that makes it look like an empirical fact when it is really a conceptual suggestion that is hopefully a useful way of thinking about something.

A particularly important kind of interpretive hypothesis concerns the notion of a category and the process of categorization, without which we cannot formulate or use concepts, for a concept is itself the result of a categorization. Ellis (1993) shows how categorization is absolutely central to knowledge, because without categories we cannot generalize. Indeed, the whole point of a category is to create the possibility of generalization. A category groups together individual instances that can be counted as relevantly similar for a given purpose—to allow a generalization. Instances within a category are thus counted as equivalent, for a given purpose, although, as individual instances, they are in fact all different in some respects. Ellis (1993, p. 117) cites Peirce on the idea that to know something is precisely to categorize it, i.e. to place it in relation to other things. Categories may or may not be “natural categories” (translation, for instance, does not seem to be a natural category); categories may be classical (Aristotelian), fuzzy, prototypes, clusters, continua, etc., but the point to bear in mind is that they are all conceptual tools, like all interpretive hypotheses, to be used or revised or discarded, as seems appropriate.

As noted earlier, research into translators’ cognition is empirical research. But the object of this research is not like the objects of research done in the natural sciences, because we are dealing with the mental behaviour of human beings, which is (we assume) much more unpredictable, messy and varied than, say, the behaviour of atoms and molecules. This assumption too may turn out to be false as more is revealed about the role of chance and unpredictability at the quantum level of physical matter: the unknown unknowns are multiple. However, given what seems still to be a significant difference between the natural sciences and the human sciences, we are justified in enquiring about the status of our explanations in cognitive research. The standard view has long been that the natural sciences look for explanation, whereas the humanities aim for understanding (see von Wright, 1971). Cognition research perhaps lies somewhere between these two alternatives, given its significant experimental input. And perhaps the relation between explanation and understanding is not one of opposition but, rather, complementarity. When we think we have a good explanation, we say that we understand. And it is only to the extent that (we think) we understand that we can (we think) validly explain.

1.3.3 Causes and other explanations

Yet there does remain an important epistemological difference vis-à-vis the natural sciences, and it has to do with causality. In the natural sciences explanations are typically causal. More specifically, in Aristotelian terms they are typically material causes, based on law-like (nomic) generalizations. (True, one may then wonder why nature should seem to obey laws at all—see e.g. Chalmers, 1976/1999, Ch. 14.) On the other hand, although explanations of human behaviour, including cognitive behaviour, may partly be based on material causes, they are also based on final causes (purpose, intention), formal causes (e.g. socio-cultural norms), and efficient causes
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(e.g. pertaining to features of the agent(s) in question). Consider history as a humanities discipline: the causes of historical events such as a war may be a mixture of all four cause types. As historical explanations, these causes are not law-like, and they do not allow precise predictions. Some scholars would hesitate to use the term “cause” at all in such contexts, and speak instead of influencing conditions, quasi-causes or the like.

Now consider the translation process. All four Aristotelian causes are relevant here too, but in a non-nomic way. In any given translation event, the process has a goal: say, a decent translation of a particular kind for a particular purpose (final cause). The process is influenced by expectations and norms (what will count as a decent translation, etc. in the given situation: formal cause). It is influenced by the linguistic form of the source text and by the material constraints of the target language (material causes). And it is influenced by the mood, experience, competence, attitudes, habitus, etc. of the translator (efficient cause); and perhaps by other things too, such as the situational constraints of time, resources and so on. Within TS, all these cause types have been explored both conceptually and empirically. Historically, the focus was first on the material causes, i.e. the contrastive relation between the two languages in question, but within the past few decades there has been a huge expansion into research on cultural, social and psychological factors as well. (See e.g. Chesterman, 1998.)

Establishing firm evidence for any of these causes is far from unproblematic. Final causes imply intentions, which are notoriously difficult to pin down. Formal causes such as norms imply both textual evidence (regularities) and extratextual evidence (such as an authoritative statement that X is indeed a norm), since textual regularities may also have other causes themselves (such as cognitive constraints). The range of potential efficient causes appears to be enormous. And the material causes may intertwine with any or all of the others …. The assumption of a single cause, in any given case, would rarely be justified.

There are also other possible types of explanation, not just causes or influencing factors. Here, too, scholarly opinions differ. On one view, generalizations also rank as a kind of explanation. This is because generalization allows us to relate a single puzzling event, for which an explanation is needed, to other similar puzzling events: we generalize from an event to a class of events. And then we can perhaps generalize across classes in terms of a higher class. The lower-level events can then be said to be “explained” by appeal to a higher generalization—even though the causal mechanism is not known. The distinction between description and explanation thus becomes blurred. (See Croft, 1990/2003, pp. 284–285; Halverson, 2003.) One problem in the human sciences, however, is that proposed generalizations may be based on rather few instances (such as in TAP studies) or be too vague to allow testable predictions. Generalizations are often formulated as tendencies, but information on exactly how a given tendency is defined in terms of probability is often lacking, which means that the claim in question cannot be adequately tested, let alone falsified. (Does the phenomenon occur in at least x% of potential cases? More often than some opposing tendency? Always?) And the scope of a generalization also needs to be stated if the claim is to be tested. (Under what conditions, exactly, is the phenomenon claimed to occur?)

Yet another kind of explanation is what Salmon has called “unification” (Salmon, 1998, pp. 69–70). This works by relating the explanandum to a wider context so as to bring together a variety of facts and synthesize them under a single structuring concept. The classic example is Darwin’s concept of natural selection (ibid., p. 360), which made excellent sense of a great deal of data, even though he was not aware of the underlying genetic mechanisms.

Any explanation can be framed as an answer to an explanation-asking question. And any explanation may be wrong, even though it may satisfy the questioner, at least for a time. (What’s that smoke, Mummy?—That’s just Vulcan, heating up his forge.—Oh, I see.) An interesting
question is then: what do people accept as an explanation, or as a good explanation, and why? Opinions will often differ. (For further discussion, see Cattrysse, 2014, Ch. 10; Chesterman, 2008b.)

1.3.4 Models

Another problematic issue concerns the kinds of models that are proposed when scholars wish to represent the translation process. In terms of a distinction made by Toury (1995, p. 249), some of these models seem to be of the translation act (cognitive, not directly observable) and others are of the translation event (observable, sociological). A translation act is thus embedded in a translation event, although in Toury’s view the two cannot be completely separated.

Chesterman (2013a) related three kinds of process model to Toury’s discussion of types of translation problem. A “virtual process” is defined as one that outlines “ideal” translation strategies, either in general or with reference to the translation of a given source-text item or item type. Such a process model usually has a pedagogical function and is often formulated as a flow diagram marked with decision points and available options. An early example was Höng (1995). A “reverse-engineered process” model starts with a translation solution and aims to reconstruct the decision chain that led to it. These models are proposed or implied e.g. in research on errors, when possible causes and sources of errors are investigated. They have also been used in attempts to plot the source of unusually successful, creative translation solutions (e.g. Kußmaul, 2007). These models also usually have a pedagogical function. A third type of model seeks to represent “actual processes” in real time, as is done in TAP research. Such models have many functions, including the aim of developing better CAT (computer-assisted translation) tools for translators.

Muñoz Martín (2016b) responded to Chesterman’s proposal with a detailed and clarifying critique. A key point in this critique is that the original distinction between cognitive acts and sociological events is a misleading oversimplification of the relation between the human brain and its environment—i.e. an assumption that needs reassessing. However, models too are conceptual tools. For some research questions, an albeit simplifying distinction between translation act and event may be useful, but not for others. The perceived usefulness of the distinction also depends on the initial theoretical position taken by the researcher. Proponents of the embodied cognition view will obviously not find it so relevant. But a view is just that: a view, a perspective; other perspectives are also possible, including ones no-one has thought of yet. In any case, any proposal about a model of the translation process needs to be as explicit as possible about precisely what is being modelled, and for what purpose, so that it can be appropriately tested. What does the model predict, if anything? What evidence or argument would suggest that the model needs revision? In what respects is it better than alternative models?

1.4 Some recent debates

The great linguist Sapir famously said that “all grammars leak” (1921, p. 39). It now appears that the mind leaks too: it is “a leaky organ” (Clark, 1997, p. 53, cited in Risku, 2014, p. 335). This view implies that we cannot understand the mind adequately if we consider it in isolation from the physical environment in which it is “embodied”—not only the brain but also the rest of the body, plus the wider context, both temporal and spatial. It is also assumed that these processes are highly contingent on local circumstances. Risku summarizes thus (2014, p. 335): “we will actually also have to study translators in their authentic, personal, historically embedded environments and translation situations if we want to be able to describe the cognitive process”. Edelman’s rainforest metaphor seems to take on renewed relevance.
If this is indeed the direction in which cognitive research is heading, the methodological challenges are daunting. If everything is connected to everything else, and the agent is seen as embedded in a complex environment, it will not be easy to isolate elements for close study; and if one has to study the whole network, where does the network end? To totally understand a single atom, does one have to study the universe? Multiple methods will need to be combined and triangulated, as Risku says. One recent suggestion is that more use should be made of “mixed methods” research (e.g. Meister, 2017). This pragmatic approach aims to integrate qualitative and quantitative methods, and it uses both induction and deduction, back and forth by turn, in an iterative cycle. However, it does not yet seem clear what kind of developed and coherent epistemology might underlie such a mixture, where subjective and objective are also merged.

And what about the relation between cognition and the unconscious? On the rainforest view, presumably these too are connected? Venuti (2002) attempted to illustrate the influence of the translator’s unconscious on translation decisions by offering some anecdotal evidence. But we are a very long way from any systematic knowledge of what this relation might be, or even how to study it.

Mention was made earlier of the well-known observer effect, evident not only in sociology but also in quantum physics, we are told. A not dissimilar problem arises when the results of empirical research on the translation process become filtered into translator training. If trainee translators become aware of what professional translators typically do, and if some of these tendencies are, in the trainees’ view or that of their teacher, not desirable ones, then one result might be that these trainees decide to act differently. For example, after conscious exposure to certain so-called “universals” such as the under-representation of target-language-specific items (Tirkkonen-Condit’s “unique items hypothesis”, 2004), students might decide to resist this tendency in their own work. In so doing, they will themselves be weakening the given tendency that scholars have described, and thus undermining the predictability of the original description, or at least reducing its generality. The very fact of describing, and then publishing and publicizing the description, could in principle even end up falsifying the original description. In this way, translation practice—and the cognitive processes underlying it—may evolve, and thus not remain constant. This in turn would imply that the whole object of cognitive translation process research could be a moving target. (See further Chesterman, 2013b.) The target is also moving because of the increasing use of CAT tools such as translation memory systems. Here, the technological progress that has been one applied goal of research on translators’ cognition is itself affecting the processes that are the object of the research. (For an example, see Dragsted, 2004.)

As noted above, our metaphors and categories influence how we think, including what we think we know. So do our concepts, and so does the very language we use. Consider the term “process.” This nominalization seems to have some inbuilt sense of a linear direction, of going from a starting point to a goal. But could this be a misleading idea? In the rainforest of the mind, would it be more appropriate to imagine cognitive activities that were simply “going on” without much sense of direction? At least, such “stuff going on” would be a significant background against which something more goal oriented could be set.

Mind, brain, context, environment—these are all grammatically nouns (in e.g. English): they are conceptually reified. To what extent might this fact mislead us? The naturalist Richard Dawkins (2004, p. 232) comments on the human tendency to think in terms of discrete entities—nouns—rather than continua: he calls this “the tyranny of the discontinuous mind”. Perhaps this is one consequence of the traditional image of the mind itself as a discontinuous organ—precisely the view that embodied cognition rejects. What effect might this “tyranny” have on our thinking about translation and cognition, about the very concepts we use? Do we
just accept them “without thinking”, as it were? It was no less a thinker than Wittgenstein who wrote that “‘concept’ is a vague concept” (1956/1978, #70).

1.5 Concluding remarks

In the light of the reservations and limitations mentioned earlier, it seems that we would do well to be rather cautious about what we think we know about translators’ cognition. So many different kinds of data sources are now being made use of, with so many different methodologies, that hesitation is warranted when colligating all the various interpretations into some kind of a coherent theory, whatever that might look like. There is always a gap between data and interpretation, of course, but perhaps also between the interpretation and the linguistic form in which we choose to express it. And there is a gap between laboratory experiments and real life. Generalizations are hard to make in the face of a wealth of variation; and their validity is constrained by the number of cases or subjects concerned. Potential explanations are many and diverse.

More generally, there will often remain a gap between “is” and “seems”. It is not always clear what counts as a fact. It was once apparently believed to be known, as a fact, that the Earth is flat. This was believed to be quite adequately justified, e.g. by common sense and the fact [sic] that no-one had ever fallen off the edge. But as we know, knowledge evolves, and most of us now believe differently. Today’s knowledge may have a different status tomorrow (Is butter good for you, or bad for you? The “facts” appear to change quite often!). But this does not mean we just have to give up and descend into a nihilistic relativism. One can justifiably be more certain of some facts than others. I am 100% certain that, as I write, Helsinki is the capital of Finland. And in general, I am pretty certain of a great many large and small facts within a kind of medium range of size and time. I am much less certain about what little I know of the very small (sub-atomic quarks and the like) or the very large (gravitational waves, the edge of the solar system …) or the distant past. And I am not too certain about what I think I know about cognition.

As Delabastita puts it (2003), empirical researchers in the humanities (including TS) are epistemologically utopian. They acknowledge that totally objective knowledge is an unattainable ideal, but an ideal that is nevertheless worth striving towards. None of our sources of knowledge are infallible: perception can be deceptive, introspection may be selective, memory can be false, the testimony of others may be unreliable, and even rational induction may be misleading. All this can be admitted. Yet, realistically, we can always seek for better understanding by testing our claims and hypotheses: not in order to confirm or prove them—the utopian ideal—but to check whether they are supported or not, or need revising, or are perhaps false after all; and, of course, by testing and replicating the tests themselves. We can aim for more truthlikeness, even though ultimate truths seem [sic] to be out of reach.

With respect to knowledge about cognition, Edelman (1992, p. 162) proposes a “biologically based epistemology”, which recognizes that knowledge must remain “fragmentary and corrigeable”, precisely because we are human, we evolve, and so does knowledge. So there is always room for doubt. Pym (1993, p.17) starts his first epistemological seminar on translation from “the primacy of doubt”. Indeed, to paraphrase Descartes: “Dubito, ergo cogito, ergo sum” (I doubt, therefore I think, therefore I am).

Further reading

Chapter 10 in particular is a thought-provoking discussion of epistemological issues pertaining to adaptation and translation, issues that are eminently relevant also to research on translation and cognition.


A clear presentation of the basic epistemological divide in TS, between empirical and postmodern positions, and the implications of this for the increasing fragmentation of the field.


Written version of an entertaining and stimulating series of seminars “for thinking students”.


An accessible overview of the most recent development in cognition research, towards embodied cognition.

References


Chesterman


