THE COMMUNALITY OF INTEREPISTEMIC TRANSLATION: CHARLES SANDERS PEIRCE AND THOMAS KUHN ON THE INTEREPISTEMICITY OF SCIENTIFIC COMMUNITIES

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ABSTRACT: This paper explores interepistemic translation as only secondarily taking place between epistemic systems—primarily between what the historian, philosopher, and sociologist of science Thomas S. Kuhn calls 'epistemological communities.' In the 1969 Postscript to his 1970 second edition of *The Structure of Scientific Revolutions* Kuhn explores the specifically translational quality of attempts among those splinter epistemological communities in what he calls a 'paradigm shift' to overcome the 'communication breakdown' caused by the erosion and collapse of the previous paradigm; the long middle section of the paper is devoted to an unpacking of Kuhn's account of 'interepistemological' or 'intercommunal' translation. Bookending that long middle section, however, are accounts of other sociological theories of such 'epistemological' communities, from Charles Sanders Peirce's 1877 article 'The fixation of belief' on the communal movement from doubt to belief, through John Dewey's 1897 position paper 'My pedagogic creed', back to Peirce again on the expanded triadic semiosis of instinct-experience-belief and abduction-induction-deduction.
KEYWORDS: Epistemological Communities; Communication Breakdown; Persuasion; Conversion; Interepistemic; Translation

1. Introduction

We have not been talking about 'interepistemic translation' long. The term was mentioned very much in passing in Robinson (2017, p. 200), and by 2022 Karen Bennett had launched a research project in its name; in December 2023 the project organized a massively transformative conference in Lisbon[1], and in 2024 a special issue on interepistemic translation was published in *Translation Matters*.

And through all these strong initial phases it has been particularly remarkable how very ubiquitous interepistemic translation is. Once one has the concept of translation between epistemic systems in sight, one sees it everywhere.

What I want to explore here is a slight but significant shift in the theorization of interepistemic translation from the mediation between epistemic *systems* to the mediation between epistemic (or epistemological) *communities*. That shift is outlined most suggestively by Thomas Kuhn in his 1969 Postscript to *The Structure of Scientific Revolutions*; but I propose to expand a focus on Kuhn to beginnings in Charles Sanders Peirce (1868/1984 and 1877) and his pragmatist follower John Dewey (1897) on 'communities of inquiry,' concluding, after a close examination of Kuhn's 1969 Postscript, with a return to Peirce, specifically the light his abduction-induction-deduction triad can shed on how and why we launch interepistemic translation projects.

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[1] See https://www.epistran.org/international-conference (Accessed: 28 December 2024)

2. Before Kuhn

2.1 Peirce on communities of inquiry

Peirce defined what later came to be called the 'community of inquiry'[2] in an 1868(/1984) paper titled "Some consequences of four incapacities."

There he begins by dividing 'cognitions' into "two kinds, the true and the untrue, or cognitions whose objects are *real* and those whose objects are *unreal*" (p. 239), and defining the former as a product of our awareness of the latter.

The real, he says, "is a conception which we must first have had when we discovered that there was an unreal, an illusion; that is, when we first corrected ourselves" (p. 239).

That process of *discovering one's own error* is for him at the core of all inquiry, all science; and while it begins in the individual in the present, as scientific inquiry it is ideally directed at a distant future in which 'thought' will have established a conception of the real that will 'stand in the long run.'

"The real, then," he says, "is that which, sooner or later, information and reasoning would finally result in, and which is therefore independent of the vagaries of me and you."

And that leads to the formulation that has come to be known as the 'community of inquiry':

> Thus, the very origin of the conception of reality shows that this conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of an indefinite increase of knowledge. And so these two series of cognitions—the real and the unreal—consist of those which, at a time sufficiently future, the community will always continue to reaffirm; and of those which, under the same conditions, will ever after be denied. (p. 239)

In other words, the community he means is not a specific finite group of scientists or other inquirers ('the vagaries of me and you') but an *infinite* community of inquirers—a community 'without definite limits' either in communal extension or in future time—to which every historical scientific community and every member of such a community belongs.

That means that the thought of any given historical scientific community or community of inquiry is necessarily imperfect, incomplete, because it is always in progress, always en route to the future thought of that infinite community:

> Finally, as what anything really is, is what it may finally come to be known to be to the ideal state of complete information, so that reality depends on the ultimate decision of the community; so thought is what it is, only by virtue of its addressing a future thought which is in its value as thought identical with it, though more developed. In this way, the existence of

[2] See Gregory (2022) for a history of the coinage of "community of inquiry," by Matthew Lipman (1998, p. 278; 2008, p. 109) and Ann Margaret Sharp (1991, p. 37n13, 1995, p. 141): "From the early 1970s until their deaths just five months apart in 2010," Gregory writes, "Lipman and Sharp collaborated on developing both a theory and a protocol for the 'community of inquiry' as the standard method of practicing 'philosophy for children,' as they conceived and pioneered that educational endeavor" (p. 2).

> thought now depends on what is to be hereafter; so that it has only a potential existence, dependent on the future thought of the community. (p. 241)

As a consequence, any disagreement between and among historical communities of inquiry, such as the state of revolutionary science that Thomas S. Kuhn will describe a century later—the struggle to establish the new paradigm—is ultimately to be settled by this entelechial movement toward a future state of certainty, in which "reality depends on the ultimate decision of the community."[3]

This model offers one possible motivation for revolutionary scientists to cross communal lines to learn the thought of a competing community, and possibly to be converted to it.

In a sense Peirce's conception is far more idealistic than Kuhn's, who portrays the history of science as repeatedly starting over, always once more replacing one paradigm with another, potentially without end. Peirce seems to be saying that science moves steadily toward a single goal.

But for Peirce that goal is only the *ideal* end of scientific inquiry. That doesn't mean it will ultimately be reached. It only means it that is the end toward which scientific inquiry must always strive.

And in that sense Kuhn's more skeptical model is simply an account of how scientific inquiry moves toward that end, or let's say *sees itself* as moving toward that end—an account, in other words, at a higher level of granularity than Peirce's 1868 formulation.

Peirce comes closer to anticipating the higher granularity of Kuhn's model, in fact, in his 1877 article 'The Fixation of Belief':

> But this method of fixing belief, which may be called the method of tenacity, will be unable to hold its ground in practice. The social impulse is against it. The man who adopts it will find that other men think differently from him, and it will be apt to occur to him, in some saner moment, that their opinions are quite as good as his own, and this will shake his confidence in his belief. This conception, that another man's thought or sentiment may be equivalent to one's own, is a distinctly new step, and a highly important one. It arises from an impulse too strong in man to be suppressed, without danger of destroying the human species. Unless we make ourselves hermits, we shall necessarily influence each other's opinions; so that the

[3] For a useful discussion of Peirce on the community (of inquiry), specifically in terms of Peirce's "maxim of pragmatism," see Kiryushchenko (2023, pp. 15-20), who also read this essay and responded in private correspondence (9 August 2024):

> This infinite community exists not only in the future, but, in some important sense, also in every 'now' of the present, as it is fully embodied in the method (the maxim of pragmatism) that we as inquirers routinely apply. According to the maxim, our every idea or conception of something is such as it is because in constructing it, we take into account the ways the world will react in case we accept it/act on it as if it was true. From this, it follows that, in Peirce, the future directly ('virtually,' in Peirce's own terms) affects the present. One might claim that, within this theoretical framework, time flows in the opposite direction, from the future to the present.

I would add that Kiryushchenko is describing a version of what Kuhn calls historian's time, and in my reading is characterized by recursive iterability.

> problem becomes how to fix belief, not in the individual merely, but in the community. (1931-1958, vol.5, section 378)

The social impulse is against it—against the 'method of tenacity.' The social impulse is that we 'influence each other's opinions,' and it does repeatedly prevail against the method of tenacity.

But note: it does *repeatedly* prevail. Not finally, once and for all. Over and over again.

See how Peirce phrases it: the method of tenacity "will be unable to hold its ground in practice." In other words, scientists will repeatedly split up into tenacious factions that will repeatedly prove unable to hold their ground as the social impulse repeatedly prevails.

This is very close to Kuhn's model almost a full century later.

2.2 John Dewey on communal inquiry-based pedagogy

Peirce's pragmatist follower John Dewey developed his notion of the community of inquiry[4] for education, specifically for a communal-inquiry-based pedagogy, in an 1897 piece titled 'My pedagogic creed':

> I believe that knowledge of social conditions, of the present state of civilization, is necessary in order properly to interpret the child's powers. The child has his own instincts and tendencies, but we do not know what these mean until we can translate them into their social equivalents. We must be able to carry them back into a social past and see them as the inheritance of previous race activities. We must also be able to project them into the future to see what their outcome and end will be.

And:

> I believe that the school is primarily a social institution. Education being a social process, the school is simply that form of community life in which all those agencies are concentrated that will be most effective in bringing the child to share in the inherited resources of the race, and to use his own powers for social ends. I believe that education, therefore, is a process of living and not a preparation for future living.

Neither Peirce nor Dewey uses the phrase 'community of inquiry.' That is a term imposed on both American pragmatist philosophers in later decades, for an approach to science and education based on the sociality of knowledge-creation.

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[4] "Community of inquiry," as I've noted, was a phrase that neither Peirce nor Dewey ever used; it was coined in the early 1970s by Matthew Lipman and Ann Margaret Sharp (see footnote n°2). As Gregory (2022, p. 1) writes:

> Many contemporary scholars who use the phrase "community of inquiry" do not mention Peirce. Some (particularly those writing on education) attribute the idea to John Dewey; few acknowledge Lipman and Sharp. Of those who do attribute the notion of a community of inquiry to Peirce, most—including Lipman and Sharp—do not offer any detailed account of which aspects of his thinking are relevant to their projects.

Dewey did not acknowledge Peirce's influence until his 1938 book *Logic: Theory of inquiry*.

Their notion is that inquiry relies for legitimacy on intersubjective agreement in the community of inquiry that pursues it, and that that legitimacy is both social and psychological, both public and private.

Peirce specifically insists that belief is only 'fixed' in the individual by collective forces outside the individual, and that those forces most effectively fix/legitimize belief when wielded not by the state or religious or other ideological institutions but by the community of one's like-minded peers.

What Thomas S. Kuhn will help us do next is to situate the nagging effects of doubt in two different communal contexts: in one specific community of inquiry—our own—*and* in the discursive differences between two scientific subcommunities of inquiry. 'Our' community has its specific doubt that 'stimulates us to inquiry until it is destroyed,' and that is all fine and good in normal science; but in revolutionary science there are competing subcommunities of inquiry studying the same phenomena, each with a different paradigm, which means not only different beliefs but different doubts and different pathways from doubt to belief. Our own pathway from doubt to belief helps us solve the problems we find within our paradigm, but as long as that paradigm is not accepted as *the* paradigm, the single normative one, and thus as *the* way to do normal science, clinging blindly to that pathway is like the ostrich burying its head in the sand. In the throes of a paradigm shift, the doubts that arise in the differences and conflicts between competing epistemological communities are the essential forces that drive all of the competing subcommunities toward belief in the emerging new paradigm. And once that new paradigm has been selected, belief in that paradigm is what organizes the normal-scientific pathway from doubt to belief.

3. Thomas S. Kuhn on interepistemic translation in revolutionary science

The thinker who has most trenchantly explored the notion of communities of inquiry in the sciences, then, is historian and philosopher of science Thomas Kuhn. That sociological orientation is often missed, however, in Kuhn's influential 1962 constructivist history of science *The Structure of Scientific Revolutions*: his account of 'paradigms' and 'paradigm shifts,' of 'normal science' and 'revolutionary science,' has been so wildly attractive to scholars in an impressive variety of fields that it has been easy to overlook his focus on scientific communities.

The basic argument of Kuhn's most famous book is that 'normal science' tries exclusively to prove a paradigm right, but in the process also generates new anomalies that can't be explained with the current paradigm, leading to a breakdown of confidence in that paradigm and sense of crisis within the scientific community. In response, different groups tend to come up with strategies for explaining each anomaly, and those strategies tend to congeal into overall models and theories, each championed by a different scientific subcommunity that advances their model as a candidate for the new normative paradigm. This period of 'revolutionary science' is stressful and distressing to scientists, and that stress pushes the rival subcommunities to struggle toward an initially radical and then

increasingly and eventually normative retheorization of the whole field—a paradigm shift.

Kuhn's is in fact specifically a *sociological* history and philosophy of science. For him a paradigm is not just a normative belief structure but the belief structure that organizes what he calls an *epistemological community*. The 'failure' or perceived breakdown of a given paradigm is experienced as a breakdown of communication in a specific epistemological community—or rather, as the communicational splitting of a single normatively constituted epistemological community into several competing ones, and the resulting breakdown of communication among the splinter groups. For Kuhn an epistemological community is organized not only by what its members know and how they know it, but by what kinds of problems they consider interesting and worth solving, and by the scope of solutions to those problems that they accept; and, further, by *how they talk* about those problems and solutions.

Another term Kuhn uses for scientific communities is 'textual community,' in the sense that they are organized by what texts they read, as part of their education and ongoing professional development, what journals they subscribe to and read regularly, how they accredit their members, what conferences they attend, and so on. This points us ahead in time to what Stanley Fish called 'interpretive communities' (1980).

In other words, a paradigm shift for Kuhn is not primarily a cognitive event taking place internally and invisibly in the heads of specific scientists, but a communicational event that can be tracked externally in the behaviors of scientific communities.

As it turns out, too, in his 1969 Postscript to the book, published in the 1970 second edition, Kuhn lays out an explicitly translational model of the paradigm shift, arguing that "the problem of choice between two incompatible theories" might be explored by the expedient of thinking about "men who hold incommensurable viewpoints … as members of different language communities" and analyzing "their communication problems … as problems of translation" (p. 175). He returned to that model later in life, and in the texts collected in Kuhn (2022) worked out additional complexities; space limitations will not allow us to explore those complexities here, but the foundation for that later rethinking was still the model he developed in the 1969 Postscript to *The Structure of Scientific Revolutions*.

What Kuhn developed in the late 1960s was a kind of sociology of interepistemic translation—or perhaps, more cumbersomely, of interepistemological/intersubcommunity translation. He is interested in tracking the ability of members of an epistemological community loyal to one (emerging rival) paradigm to understand, translate, and even come to adopt the discourses of epistemological subcommunities loyal to other paradigms. This makes Kuhn's model a useful guide to interepistemic translation across all epistemic barriers—especially when those barriers are understood sociologically as barriers not between texts or cognitive schemata but between discourse communities.

The socioprofessional situation in which Kuhn situates his remarks on translation is the interim period of revolutionary science between two paradigmatic periods of normal science. Specifically, he is interested in the intercommunity 'negotiations' by which rival

potential paradigms are tested and compared in the move to the enshrinement of a new normal paradigm.

This of course is not the only situation in which interepistemic translations are pursued. Some situations are purely academic, caught up in the gamesmanship of publishing and winning reputation and prestige: a critical theorist in the humanities may seek to translate interepistemically between two theoretical or philosophical epistemes, such as Marxism and poststructuralism, or queer theory and Deleuzean process philosophy, not to merge them but to supercharge each by means of the other. Some are purely practical, as when a hiking party is lost in the woods and has to find its way out, using epistemic tools that were not designed for use in nature, like buildings as landmarks in big cities. Obviously, the motivations fueling interepistemic translation are very different in those two contexts: in the former 'creating knowledge' (and, let's face it, showing off), in the latter saving the group's lives.

By the same token, the methods used in those two contexts differ markedly as well. The methods used by the critical theorist are textual-hermeneutical, with a bending of each existing theoretical episteme toward the other and a bias toward complexity and surprise; the methods used by the hiking party are visual, tactile, and orientational, with a unidirectional tendency, adapting non-woodsy knowledge to woodsy conditions.

But Kuhn's communal model of socioprofessional interepistemic translation is nevertheless applicable to many situations outside of scientific communities. It is relevant to clashes between any divergent interest groups, such as townhalls, school-board meetings, labor arbitration, international diplomacy, and domestic squabbles over the distribution of chores and rewards. In each of those (and of course thousands more) we find not just a difference of opinion but the conflicting and competing epistemic orientations of the different communities, as between conservatives and liberals, the rich and the poor, management and labor, Orientalists and Occidentalists (see Buruma and Margalit, 2005), women and men, children and their parents.

So let's start by dividing Kuhn's 1969 remarks on translation into nine stages and considering each in turn.

> *Stage 1: What is shared*. The scientists caught up in the "communication breakdown" of a paradigm shift share a great deal: not only neurocultural programming but "a history, except the immediate past" (1962/1970, p. 201). "Both their everyday and most of their scientific world and language are shared." (p. 201)

The 'immediate past' that is not shared is the period of revolutionary science, when the adherents of rival paradigm-candidates have taken up antagonistic positions and intergroup 'communication' has broken down. It is easy to take that antagonism as the sole defining characteristic of the period of the 'communication breakdown,' but Kuhn begins with what is shared—which is almost everything. The 'everyday' world outside science and the language of that world are shared. To the extent that any one 'scientific world' will have been structured for everyone working in that branch of science by at least one 'normal'

paradigm accepted by everyone, and in some cases by a series of such paradigms, that world too and its language will be mostly shared—all, of course, but the current competition to establish the new normative paradigm.

It is significant for the modeling of exploratory/mediatory interepistemological/ intersubcommunity communication as *translation*, of course, that Kuhn highlights the importance of language. Not only the vocabulary but the discursive/rhetorical strategies of each competing community are shaped by the exemplar theory/model (paradigm-candidate) it espouses. It would have been possible, and even, perhaps, expected—especially by humanists, who tend to imagine that scientists are radically different from them—that Kuhn would portray each rival community as primarily shaped by scientific principles, methods, theories, procedures, algorithms. Instead, he focuses on language. That has the salutary effect of expanding his model's applicability to intergroup tensions and conflicts and competitions across a wide spectrum of human social, cultural, and political life. This is not just about scientists.

> *Stage 2: How they differ*. Sharing so much, they can determine how they differ: "what the participants in a communication breakdown can do is recognize each other as members of different language communities and then become translators." (p. 202)

One might want to read "different language communities" (p. 202) there as an interlingual metaphor for proponents of rival paradigm-candidates; but it's not a metaphor. It's a sociological reality. People whose world-views differ speak differently. This for Kuhn is the most salient fact about the sociology of the 'communication breakdown' during the period of revolutionary science—and also, of course, the most salient fact about the sociology of translation that here practically from the outset begins to manage that breakdown. Precisely because those different language communities have so much in common, they have a standpoint from which to identify divergences in language usage—and then to learn to translate across those divergences.

> *Stage 3: Identify "foci of trouble."* "Taking the differences between their own intra- and inter-group discourse as itself a subject for study, they can first attempt to discover the terms and locutions that, used unproblematically within each community, are nevertheless foci of trouble for inter-group discussions." (p. 202)

I read that first direct object, "the differences between their own intra- and inter-group discourse" (p. 202), to mean 'the differences between their own intragroup discourse and intergroup discourse.' (Reading 'intra- and inter-group discourse' as unified by 'their own' confuses things.) The idea is that some "terms and locutions" are "used unproblematically within each community" but become "foci of trouble for inter-group discussions." (p. 202)

What kind of trouble? Semantic trouble, ultimately: people in other groups don't understand us when we talk to them the way we talk to our own people. But the interesting

question is what affective, cognitive, or behavioral effects signal that trouble—or, perhaps, *are* that trouble. Affective trouble; cognitive trouble; behavioral trouble.

Behavioral trouble would be the obvious one: a member of one community suggests a course of action—a method, a testing protocol, an evaluative standard—and a member of another community agrees to try it, but then does something unexpected, something 'wrong.' This scenario already invokes affective and cognitive trouble, of course: the behavioral responses to a given practical instruction within a unified language community are habitualized, which means that members of that community expect the words to lead to the habitualized behavior, and that expectation may not even be conscious. One may only become aware of the habitualized or normalized expectations when they are suddenly and surprisingly not met. Unfulfillment of unconscious expectations may generate first affective trouble—what I've called 'correctness anxiety' (Robinson, 2023a, pp. 56-58)—then cognitive trouble (the attempt to explain what just happened), and 'behavioral trouble' third, as the basis for the explanation.

Take the constant state of 'communication breakdown' (revolutionary science) in translation studies, for example. Arguably our primary keyword is 'equivalence.' But what *is* equivalence?

- The scientifically minded linguists championing Translation Quality Assessment, like Juliane House (1977, 1996), take it to be a stable objective semantic pattern of repetition or reproduction from the source text to the target text ("semantic equivalence"—see Robinson, 2023b, Question 1).
- Back in the 1960s Eugene A. Nida (1964, 1969), Executive Secretary for Translations at the American Bible Society, divided equivalence into 'formal' and 'dynamic equivalence.' Formal equivalence was 'the same meaning' across the source-to-target passage, sticking as closely as possible to the forms of the source text; dynamic equivalence was the similarity not of structure or meaning but of reader response.
- Anthony Pym (2010, pp. 6-8) insists on the Latin roots of the word, saying that an "equivalent" translation is expected to produce not the same meaning but "equal value" with the source text, at all textual levels, from text to sentence and sometimes to word. As the value of consumer products fluctuates with the market, so too does the value of translator decisions.
- And Gideon Toury (1995/2012), arguing for 'target-side priority,' wrote of 'assumed translations' and their 'assumed equivalence.' Anything that is generally taken to be a translation is assumed to be an equivalent reproduction of a source text in another language. No comparative testing for equivalence: the 'proving' of equivalence or nonequivalence and all other comparisons with a source text become irrelevancies.

Juliane House rejected Nida's dynamic equivalence because it depended on target readers' subjective responses and so wasn't scientific. Two linguists, but they disagreed on the correct meaning of 'equivalence'—disagreed on the theoretical and methodological exemplar (paradigm) for the linguistic study of translation. For House the prevailing paradigm was empiricist, scientizing, resting on the systematic exclusion of 'the human factor' (i.e., readers); for Nida it was proselytic, missiological, aimed at the winning of hearts and minds (also readers) for Christianity. Science vs. religion; data vs. salvation.

Pym is a sociologist of translation, and arguably Toury was one as well; but they disagree on the sociological forces or factors determining translational equivalence. Pym situates those forces in the intercultural economic decisions of the professional translation marketplace; Toury situated them in the organizing force of target-side social opinion. For both, the target culture is the sociological group that determines equivalence; but for Pym those economic determinations of semantic and pragmatic value are shaped empirically by a fluctuating cross-cultural market, while for Toury they were a kind of virtual reality, sustained phenomenologically by collective belief. Economics vs. social psychology.

There are translation scholars who still today insist that there is only one acceptable paradigm for translation studies, typically the linguistically aligned equivalence paradigm—semantic equivalence, more or less—and push strongly for the rejection of article manuscripts that do not compare translations with their originals in a test for equivalence. Nothing else is translation studies.

On the other hand, there is a downside to the constant state of what Kuhn calls 'communication breakdown' in revolutionary science. When I was a coeditor at *Target*, I grew extremely frustrated at the overwhelming flood of article submissions adhering to translation-studies paradigms that I hardly knew at all and could not properly judge. A very small fraction of the submissions I processed—say, one single paper among the twenty new submissions that I processed out to reviewers each month—was written in my paradigmatic comfort zone. It would have been nice, I remember thinking then, to serve as editor at a journal organized around the four or five paradigms in which I did feel at home.

In interepistemic terms, I was expected to translate dozens of 'alien' epistemic discourses well enough, at a minimal and very superficial level, to be able to decide whether to desk-reject them or send them out for review. Typically, in most cases I didn't feel competent to desk-reject a submission, which meant that I had to muster enough confidence in my ability to decide who to send them to—what kind of research expertise I should be looking for in tracking down potential reviewers to invite. When the translation-studies paradigm that organized a given submission was one to which I adhered, by contrast, all my editorial decisions came easily and quickly, and I assume competently.

The point is that human beings tend to feel comfortable in their current epistemic regimes, and uncomfortable adapting to some other group's. Thomas Kuhn's 1969 Postscript tracks the slow and arduous process involved in that kind of adaptation.

> *Stage 4: Translate homophonically.* "(Locutions that present no such difficulties may be homophonically translated)." (p. 202)

In an interlingual context homophonic translation generates playful incomprehensibility; presumably for Kuhn, in the context of a paradigm shift 'homophonic translation' is a metaphor for something else—but what? *Ease* of translation, most likely; but what would make 'homophonic' a useful metaphor for that ease?

Almost certainly Kuhn is writing about interepistemic translation that is also *intralingual*: the 'terms and locutions that, used unproblematically within each community, are nevertheless foci of trouble for inter-group discussions' are probably all in English, the global language, or in whatever local vernacular scientific discourse is conducted. In that intralingual translation situation, we might imagine that a homophonic translation of 'Locutions that present no such difficulties may be homophonically translated' into English would be 'Locutions that present no such difficulties may be homophonically translated': the exact same words.

But if that is what Kuhn means here, he's wrong, because my reading of his 'homophonically' as a 'homophonic translation' of 'homophonically' manifestly obscures his meaning. 'Homophonically' is precisely a 'focus of trouble.' The way it is used in translation studies, to mean reproducing the sounds of the source text's syllables in the target language, makes Kuhn's application of the adverb to 'Locutions that present no such difficulties' seem nonsensical.

But perhaps my example of 'equivalence' in translation studies might help. It is precisely because different scholars applying different translation-theoretical paradigms use the exact same word—or, perhaps, a word that only homophonically *sounds* the same—to mean very different things that 'equivalence' needs to be translated interepistemically, as 'accuracy' or 'equivalence of response' or 'equal value' or 'assumed equivalence.' Those unpackings of the differences in cross-paradigmatic usages highlight the differences between the various usages in the different paradigms. It's only, then, when a term is used in exactly the same way across all paradigms that no interepistemic unpacking is necessary.

For example, 'source text' and 'target text' seem to be fairly unproblematic terms in translation studies these days. That is certainly true in English, and in every other European language I know; I'm not sure it is globally unproblematic. Wherever those terms or their local counterparts are well established across translation-theoretical paradigms, they can be 'homophonically translated' in the sense of using the same terms without interepistemic 'translation' (or explicitation).

But compare that situation with this passage in Friedrich Schleiermacher's 1813 Academy address on the different methods of translating:

> Diese beiden Beispiele von den äußersten Enden der Wissenschaft und der Kunst hergenommen zeigen deutlich, wie wenig der eigentlich Zweck alles Uebersetzens, möglichst unverfälschter Genuß fremder Werke, durch eine Methode erreicht werden kann, welche

> *dem übersetzten Werke* ganz und gar den Geist einer ihm fremden Sprache einhauchen will. (1813/2002, p. 90, 38-43; emphasis added)
>
> These two examples from opposite poles in scholarship and the arts clearly demonstrate just how poorly translation's true goal of unadulterated pleasure in foreign works is attained through a method that would breathe into *the translated work* the spirit of a language alien to it. (Robinson, 2014, p. 236; emphasis added)

What exactly is 'das übersetzte Werk' / 'the translated work'? Is it the source text (the work that has been translated)? Or is it the target text (the translated text)? As I wrote in *Schleiermacher's Icoses* (2013, p. 111):

> I take it „der Geist einer ihm fremden Sprache" / "the spirit of a language alien to it" means the spirit of the *target* language? It is a confusing statement, because surely the language whose spirit is alien to the target text is the spirit of the source language, making it seem as if "a method that would breathe into the translated work the spirit of a language alien to it" refers to foreignization: breathing into the target text the spirit of the source language. But since Schleiermacher is specifically attacking domestication here, he must mean by „dem übersetzten Werke" / "the translated work" not the target text but the source text, or something like "the work that has been translated."

Translation studies was very far from an established academic discipline in 1813. The German Romantics were among the most influential voices in the development of that academic discipline a century and a half later, but Schleiermacher's wording is confusing here in much the same way Kuhn says the discourse of communities loyal to rival paradigm-candidate A is confusing to those who are loyal to rival paradigm-candidates B, C, and D.

Or perhaps there is a difference: if, Kuhn seems to suggest, in modern scientific communities rival lexicons are developed that coach their users in the *confident* use of shared words in novel and divergent ways, Schleiermacher's terminological confusion may reflect a pre-paradigmatic state—the state before "nature" has been forced into "the conceptual boxes supplied by professional education" (1962/1970, p. 5). "Normal science," Kuhn adds, "the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like" (p. 5)—and in 1813 there was no scientific (or scholarly) community that knew what the world of translation was like. There was no professional education to supply the conceptual boxes into which the 'nature' of translation could be forced. Schleiermacher, like his fellow Romantics Novalis, the Schlegel brothers, and Humboldt, was making it up as he went along.

On the other hand, it may also be that in times of revolutionary science the very fact that the next paradigm shift is *underway*, not yet complete, not yet normalized, means that vocabularies are not yet set in stone either. "The early developmental stages of most sciences," Kuhn writes, "have been characterized by continual competition between a number of distinct views of nature, each partially derived from, and all roughly compatible with, the dictates of scientific observation and method" (p. 5). That was certainly the

'translation studies' situation in Romantic Germany—with a loose and forgiving reading of 'the dictates of scientific observation and method.' Remarkably, in many ways it is still the translation studies situation in the world today, where dozens of theoretical paradigms coexist—*and yet* translation studies has been academically institutionalized, to the point where you can get a Ph.D. in Translation Studies in just about any country in the world, and terms like 'source text' and 'target text' have been semantically stabilized. It is unthinkable today, despite the mad proliferation of competing translation-theoretical paradigms, that anyone with a Ph.D. in Translation Studies could write today the confusing paragraph that Schleiermacher wrote in 1813 about 'das übersetzte Werk' / 'the translated work.'

> *Stage 5: Enculturation.* "Having isolated such areas of difficulty in scientific communication, they can next resort to their shared everyday vocabularies in an effort further to elucidate their troubles. Each may, that is, try to discover what the other would see and say when presented with a stimulus to which his own verbal response would be different. If they can sufficiently refrain from explaining anomalous behavior as the consequence of mere error or madness, they may in time become very good predictors of each other's behavior. Each will have learned to translate the other's theory and its consequences into his own language and simultaneously to describe in his language the world to which that theory applies." (p. 202)

Here the scientist who remains loyal to rival paradigm-candidate A begins to feel increasingly at ease, perhaps even at home, in the discourse of the scientific subcommunity that swears loyalty to rival paradigm-candidate B—and vice versa. This is obviously a process of enculturation, and this time Kuhn specifically outlines *behavioral* cues—even perhaps behavioristic cues, such as 'responding' to a 'stimulus'—as indicators of each 'foreigner's' progress.

It is striking that Kuhn imagines this enculturation process as mutual, or reciprocal: 'Each may, that is, try to discover what the other would see and say'; 'Each will have learned to translate the other's theory and its consequences into his own language.' Not being a scientist myself, I'm not sure whether members of rival scientific communities vying to make their approach paradigmatic for the entire branch of science would really be this eager to become thoroughly familiar with the discourse(s) of their competitors. Certainly, it doesn't seem likely to be the standard situation in the humanities, where this welter of competing theoretical paradigms is a continuing reality. Critical theorists in the humanities (including translation theorists) tend to familiarize themselves with a great many theoretical projects; but it doesn't seem likely that a theorist strongly oriented to the critical theories coming out of the Kantian and post-Kantian tradition—hermeneutical, phenomenological, poststructuralist—would consider it worth the effort to become comfortably conversant in positivist theoretical orientations. In translation studies, the empiricists studying corpora and eye-tracking and so on don't seem inclined to waste their time reading Judith Butler and Michel Foucault and Jacques Derrida and Deleuze and Guattari—and that feeling is entirely mutual. Are scientists really that different? Or is Kuhn simply romanticizing the communal spirit among scientists?

It may be that Kuhn is simply assuming that the stress and distress caused by the critical "communication breakdown" (p. 202) brings a relentless pressure on everyone to move toward consensus on the emergent shaping of the next normative paradigm. As Peirce would put it, doubt is rampant, and therefore so is the push toward belief. Perhaps that pressure works on all members of all rival subcommunities to remain open to learning what the competition is saying and doing and why.

> *Stage 6: Translation as a tool for persuasion and conversion*. "Since translation, if pursued, allows the participants in a communication breakdown to experience vicariously something of the merits and defects of each other's points of view, it is a potent tool both for persuasion and for conversion." (p. 202) "Translation is not necessary for persuasion, but "in its absence many of the explanations and problem-statements endorsed by the members of one scientific group will be opaque to the other." (p. 203)

It is interesting there to see translation mobilized by "the participants in a communication breakdown", as if a 'communication breakdown' were a principled negotiation, with rules that must be obeyed and established strategies that participants will customarily deploy. On a superficial reading, either revolutionary science is not so much a communication *breakdown* as it is an organized communication *restructuring* or *reorganization*, or else it is not a negotiation or other meeting or organized activity with *participants*.

Working out those apparent tensions on a deeper level might require an edit of Kuhn's sentence.

Let's start at the end, with 'persuasion and conversion': each rival paradigm-candidate has its loyalists who want to see their approach win out over all the others and become the basis for the new period of normal science, and to that end they will be motivated to persuade and convert loyalists of other scientific subcommunities.

Second, 'potent tool': each subcommunity seeking to persuade and convert members of other subcommunities will need to *develop* strategies for doing so, and to that end will be looking for effective rhetorical tools.

Third, 'communication breakdown': thinking of revolutionary science not as a disorganized free-for-all competition with no rules but as a *breakdown of* ***communication*** may help direct the strategic thinking of those who are looking for potent tools to aid in persuading and converting opponents. If what one discovers is a breakdown not of communication but, say, of methodological credibility, then one's strategic eyes are likely to be turned to possible methodological adjustments: we need to rethink our research questions, our hypotheses, our variables, our testing protocols, and so on. But if what has broken down is not our research methodology but our communication, then our thoughts turn to rhetorical strategies, one of which might be translation.

Fourth, *if* translation is 'pursued'—that 'if' implying that, while translation is the only rhetorical strategy that Kuhn mentions, it is really only one of many strategies, rhetorical and otherwise—that pursuit has implications for how we go about imagining and

structuring possible remedies for the communication breakdown. As Kuhn presents it, after all, translation is a *conciliatory* rhetorical strategy. One doesn't go on the offensive, ridiculing and insulting one's opponents; one learns their language, learns to talk and ultimately think like them, so as to enable persuasion *on and in and by their terms*. Conversion, in other words, is not just in your scientific best interests: it is the obvious commonsensical outcome of your own discourse. Your own vocabulary opens the door to you joining us.

And fifth, this conciliatory rhetorical strategy would seem to require that we (all) frame what we're doing not as squabbling over the spoils, trying to win an adversarial zero-sum victory that will devastate the losers, but as participating in a principled negotiation with the goal of benefitting everyone equally. Thinking of the enterprise of revolutionary science along those lines makes a 'communication breakdown' sound like a minor speed bump that we can all get past with relative ease if we just work together and find a common language to facilitate better communication. That would be the sense in which we all are—or perhaps all come to adopt the rhetorical strategy of envisioning ourselves as—'participants in a communication breakdown.'

Thus: "Given the importance in a communication breakdown of pushing for persuasion and conversion, and thus of developing potent rhetorical tools to effect those desiderata, it becomes pressing for participants to pursue conciliatory rhetorical projects aimed at mutual understanding, especially translation." (p. 203)

Ultimately of course the new paradigm must be chosen by consensus. There is no one in a position to decide the new paradigm by fiat—and even if there were, the majority of the relevant 'participants' in the process would have to go along with that decision. There are likely to be some holdouts, of course; but given the basic assumption that normal science is for the most part *what science is*, and that normal science means trying to prove a single dominant paradigm right, holdouts will either have to give up their resistance and rejoin the work on the new paradigmatic terms or retreat into beleaguered and isolated idiosyncrasy, which means working without grants, without a lab stocked with Ph.D. students and lab assistants, without conversations with colleagues, without publishing one's results, and so on. To be a maverick in science is to be branded a crank at best and dangerously insane at worst.

One thinks of the 'evil genius' or 'mad scientist' stereotype on which so many spy-thriller villains have been based: the stereotype reflects society's attitude toward scientists who refuse to do normal science by the rules.

As Kuhn puts it, though, eventually the holdouts all die, and resistance to the new paradigm evaporates.

To be sure, world-changing breakthrough discoveries have been made by such 'cranks.' The theories of such 'cranks' have sometimes survived and become the next paradigm. But for that to happen the 'holdout' must be either able to work alone, or else independently wealthy and so able to staff a lab with researchers who need a job and lack professional ambition, and possibly also to fund a new journal and a new wing of the

science building. The only other option is to be publicly compliant (willing to maintain a lab and grants and so on under the new paradigm) and privately resistant. And, of course, one must be long-lived enough to survive until the new paradigm begins to develop the crippling anomalies that instigate a new paradigm shift.

In the humanities, by contrast, the reigning assumption is that we all work alone and are entitled to espouse whatever theories we like. But we too have our orthodoxies. Racist theories, misogynistic theories, and other unabashedly exploitative theories are so fervently frowned upon by pre-publication reviewers that reputable academic presses and journals will be unlikely to publish them, and publication by disreputable presses and journals will probably not count in promotion and tenure decisions.

There are also radically progressive counterparadigms, especially for qualitative research, experimental paradigms designed to overcome the silencing of 'respondent' communities and heroization of the white male researcher—and they may be uneasily tolerated but quietly shunned as 'woke' by more conservative (especially white male) scientific communities.

Also, while it is true that in the humanities there is no single dominant paradigm that everyone must support or else face career-derailing isolation, it remains the case that work done recognizably and intelligently in one of the eight or ten most respected paradigms of a humanistic field will serve your career far better than brilliant maverick work that no one knows how to categorize. Scientific and scholarly communities tend to be quite conservative. Pigeonholability is a plus.

And of course humdrum work in a peripheral and disparaged paradigm will serve your career hardly at all. In a modern-language department, for example, a literary study of a minor writer had better adhere to some major paradigm, like Deleuze and Guattari retheorizing 'minor writers' as minoritarian disruptors of the major language, or Jacques Lacan theorizing the Other-capital-O (see e.g. Robinson, 1992). Otherwise, the study of minor writers will tar the scholar with the brush of 'minor scholar,' which will tend to keep the colleague on the sidelines in promotion and tenure decisions—except of course at fourth-rate teaching institutions where hardly anyone is publishing anything at all.

Ultimately in the humanities the judge and jury are readers—academic readers, of course, who buy your books and cite your publications and invite you to give guest lectures and conference keynotes, but notably either readers who already adhere strictly to the paradigm your work supports or cross-over readers who are attracted by your name or the quality of your argumentation to wander outside their paradigmatic comfort zone. The former are the humanistic counterpart of what Kuhn calls normal scientists; the latter are examples of the scientists in a 'communication breakdown' who cross the lines, who learn the other side's language, learn to translate one partisan idiom into another.

In the humanities working innovatively, creatively outside a well-established paradigm is a recipe for success only in a 'revolutionary science' context where there are enough cross-over readers who read and cite you (and invite you to give keynotes and guest lectures, offer you jobs, etc.). That is the theoretical overlap between Kuhn on

revolutionary *science* as a 'communication breakdown' and critical theory in the humanities.

Interestingly, 'interepistemic translation' is an emergent paradigm-candidate *in and beyond* translation studies—on into what has been called 'post-translation studies' (see Arduini and Nergaard, 2011; and Gentzler, 2016)—and what this article is designed to do is to win support for that candidacy, for that emergence. To the extent that interepistemic translation is an umbrella term that remains within the well-trodden confines of intralingual, interlingual, and intersemiotic translation (Jakobson, 1959), it is a 'new' paradigm-candidate (or terminological shift) only within translation studies. What this exploration of the parallels between Kuhn on 'translation' in a revolutionary paradigm-shift context and other intercommunity accommodations seeks to establish is the broader application of 'interepistemic translation' as an emerging paradigm-candidate for institutional and intercultural communication studies beyond the translation of verbal texts.

> *Stage 7: Research results as engines of persuasion and conversion*. "But each language community can usually produce from the start a few concrete research results that, though describable in sentences understood in the same way by both groups, cannot yet be accounted for by the other community in its own terms" (p. 203), and this may lead to persuasion and even conversion—especially among scientists "just entering the profession, for they have not yet acquired the special vocabularies and commitments of either group." (p. 203)

Significant here is the psychosociology of those epistemological communities organized around a stance or a policy—excluding, in other words, those communities of practice that are organized more or less unconsciously and so tend not to be thought of by their members as a community of any kind. Still, it's possible for the membership of say a poker game to overlap significantly with a group of buddies drinking beer at the local watering hole, and for tensions to surface between the drinkers and nondrinkers at the poker game ('We're here to play poker, not to get drunk!') or between the drinkers at the bar who want to introduce a poker game and those who don't.

Still, an epistemological community organized by overt affective-becoming-cognitive loyalty to a specific rival paradigm-candidate will feel these tensions with a higher level of awareness. Similar dynamics surface in political parties, as when loyal members of the U.S. Republican party in the era of Donald Trump either were converted to violent Trumpist fascism or clung tenaciously to traditional law-and-order Republicanism, and were branded by the opposite faction as either 'extremists' or 'RINOs' (Republicans In Name Only), respectively.

As Kuhn notes, scientists who enter the profession in times of paradigm uncertainty—'communication breakdown'—are especially susceptible to persuasion. But of course the ultimate establishment of a new paradigm requires 'persuasion and even conversion'

among all or most members of the epistemological subcommunities that were resisting the new paradigm the most strenuously.

> *Stage 8: Translation as threatening.* "For most people translation is a threatening process, and it is entirely foreign to normal science." (p. 203)

This is possibly the most intriguing proposition in Kuhn's 1969 Postscript. Interepistemic translation, of course, is (so far) 'entirely foreign to normal science' because normal science is predicated on universal loyalty to a single paradigm—the now normative one. Translating the epistemic assumptions governing the scientific activities keyed to support for that normative paradigm into the language of some other epistemological subcommunity would be either puzzlingly irrelevant (if to the discourse of an epistemological community whose assumptions had no bearing at all on normal science) or a threat to normativity (if to the discourse of an epistemological community whose assumptions challenged the normative ones).

But isn't an argument for interepistemic translation designed to normalize it, normativize it, establish it as the new norm, the new paradigm, and thus the basis for a new normal science? To the extent that Kuhnian thought is not science but the philosophy and history of science, no, it is not designed to be installed as a 'normal science.' It is at most a metascience. If we assume that Kuhn won the 1965 debate with Karl Popper at the University of London's International Colloquium in the Philosophy of Science, and utterly defeated not only Popper but all philosophy-of-science justificationism, we might declare it the new 'normal metascience.' But if (as indeed is the case) other influential groups still today declare Popper the winner of that debate, then what we find is obviously not normal but revolutionary metascience: the continuing clash of competing paradigm-candidates. As I've been suggesting, that clash is the 'norm' in the humanities.

Kuhn's observation that "For most people translation is a threatening process" also rang true for the anti-Trump messaging that translated the discourse of Trump and Trumpism politically as fascism, legally as criminality, and medically as cognitive decline. But I assume that Kuhn's "threatening process" refers mostly to the competitive situation in the midst of a paradigm shift. In that context, translation is only vaguely threatening; the threat may be experienced as a general nonspecific unease.

That unease is familiar to historians of interlingual translating and interpreting. Translators and interpreters have always been suspect to people in entrenched cultural positions—to what Sakai Naoki (1997) would call 'homolingual' subjects, people who believe that speakers of other languages and members of other cultures than their own are intrinsically suspect, and therefore question the loyalty of translators and interpreters who have lived abroad long enough to pick up proficiency in a foreign language and culture.

A good example of that homolingual unease is Friedrich Schleiermacher's insistence in his 1813 Academy address that translators into German should leave a *Gefühl des Fremden* / 'Feeling of the Foreign' in their translations, so as to signal the foreignness of

the translated text to patriotic Germans. The goal was not to make native speakers of German *hate* the foreign works, but simply to build a kind of subconscious signal of foreignness into all literary and scholarly translations, to induce target readers to maintain a certain protective affective distance from those works.

> *Stage 9: Going native*. "To translate a theory or worldview into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign. That transition is not, however, one that an individual may make or refrain from making by deliberation and choice, however good his reasons for wishing to do so. Instead, at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made." (p. 204)

Kuhn's point in this last stage is temptingly close to Schleiermacher as well: the idea that learning to translate an alien discourse into one's own inevitably involves a certain distance is reminiscent of Schleiermacher's insistence that literary and scholarly translators deliberately *maintain* that distance by simulating in the target reader's response to the translation the Feeling of the Foreign experienced by the intermediate learner of the foreign language. The obvious difference is that Kuhn's interepistemic translator *is* that intermediate learner, not an expert mediator who can control and so choose among simulatory strategies. More precisely, Kuhn's interepistemic translator is that intermediate learner *until s/he is no longer*—until s/he has 'gone native, discover[ed] that [s/he] is thinking and working in, not simply translating out of, a language that was previously foreign.' Kuhn's interepistemic translator, in other words, doesn't control that all-important foreignizing affective-becoming-cognitive distance from the 'source language'—the other community's discourse. In an important sense Kuhn's whole discussion of interepistemic translation tracks what Schleiermacher would call 'bringing the author to the reader' (domestication): at first, one gropes one's way forward cautiously, exploring what feel like differences, learning the 'language'; the more one learns, the easier it becomes to translate between the two communities' discourses; eventually one "goes native," or in Schleiermacher's terms becomes a polyglot.

4. Conclusion: Back to Peirce

However, one thing is missing in all this so far—or two, depending on how we count: namely, *how and why* we get started in interepistemic translation.

In 1877 Peirce says that we are launched onto the path of scientific reasoning by doubt, and more specifically by the habit of mind that makes us prefer belief to doubt, and so makes us move determinedly toward belief by resolving doubt; but he doesn't consider the 'metadoubt,' as it were, the secondary level of doubt that is aroused when competing subcommunities have developed different pathways from doubt to belief, and thus different normative constructions of doubt, of belief, and of the proper pathway from the one to the other.

One way of mobilizing Peircean thought to address the question before us of how and why we get started translating interepistemically might be to read one paragraph in 'The fixation of belief' in the light of Peirce's later (1901-1902) abduction-induction-deduction triad:

> We are, doubtless, in the main logical animals, but we are not perfectly so. Most of us, for example, are naturally more sanguine and hopeful than logic would justify. We seem to be so constituted that in the absence of any facts to go upon we are happy and self-satisfied; so that the effect of experience is continually to contract our hopes and aspirations. Yet a lifetime of the application of this corrective does not usually eradicate our sanguine disposition. Where hope is unchecked by any experience, it is likely that our optimism is extravagant. *Logicality in regard to practical matters* (if this be understood, not in the old sense, but as consisting in a wise union of security with fruitfulness of reasoning) is the most useful quality an animal can possess, and *might, therefore, result from the action of natural selection*; but outside of these it is probably of more advantage to the animal to have his mind filled with pleasing and encouraging visions, independently of their truth; and thus, upon unpractical subjects, natural selection might occasion a fallacious tendency of thought. (1931-1958, vol. 5, section 366; emphasis added)

Now let us ask: in what does 'logicality in regard to practical matters' consist? Logicality in some general sense? Any kind of logic? It is 'the most useful quality an animal can possess,' but—any animal? Should we imagine rats reasoning syllogistically? Of course not: they have no language, and therefore no propositional logic. In 1877 Peirce imagines 'logicality' as what propels us from doubt toward belief; and we might imagine that for a rat that logical propulsion must take the form of trial and error. If we thematize trial and error as induction, the testing ('trialing') of hypotheses, then we might find ourselves imagining with Peirce that induction 'result[s] from the action of natural selection'—but note that for Peirce the *action* of natural selection is not some static biological 'instinct.' Natural or not, the action of *selecting* would always be triadic—which for Peirce means it would have a semiotic history. Specifically, if an animal is *inclined* to forage inductively for food and water, that inclination—what Peirce identifies as a First, which is to say an abstract potentiality—has to come from somewhere and lead somewhere else. Where it leads, of course, would be the trial-and-error experience of foraging, a Second—a brute engagement with the real world—but where would it come from? Peirce would say that, if successful, the experience of searching by trial and error leads to the development of *habits*, and habit is a Third: pattern, a precept, etc. This would be Peirce's instinct-experience-habit triad.[5] As we continue to cycle through the sign-action, habit comes to seem like instinct: that is where the Firstness of 'instinct' (or inclination) comes from. Experiential Secondness devolves into the habitualized patterns of Thirdness, which feels like instinctual Firstness. We—whatever animals we are—'forget' the triadic history leading from successful

[5] Actually Peirce wrote of an instinct-experience-*form* triad (1931-1958, vol. 8, section 374); I borrowed 'instinct-experience-habit' from Gorlée (1994) for all four editions of *Becoming a Translator* (the fourth in 2020). I engage the terminological shift in Robinson (2016, p. 13).

experience to habit, and what we perform habitually comes to seem as if it had always been with us, always been our stable and reliable resource.

Experience is obviously the realm in and through which doubt propels us toward belief. But is that all induction? As induction leads to deduction, it becomes a Third that is conducive to the formation of habit. But if induction is the testing of hypotheses, where do those hypotheses come from?

In his 1901 article 'On the logic of drawing history from ancient documents especially from testimonies (logic of history)' Peirce first introduces the abduction-induction-deduction triad, arguing that abduction is the indispensable process of *guessing* that generates hypotheses:

> Abduction and induction have, to be sure, this common feature, that both lead to the acceptance of a hypothesis because observed facts are such as would necessarily or probably result as consequences of that hypothesis. But for all that, they are the opposite poles of reason, the one the most ineffective, the other the most effective of arguments. The method of either is the very reverse of the other's. Abduction makes its start from the facts, without, at the outset, having any particular theory in view, though it is motived by the feeling that a theory is needed to explain the surprising facts. Induction makes its start from a hypothesis which seems to recommend itself, without at the outset having any particular facts in view, though it feels the need of facts to support the theory. Abduction seeks a theory. Induction seeks for facts. In abduction the consideration of the facts suggests the hypothesis. In induction the study of the hypothesis suggests the experiments which bring to light the very facts to which the hypothesis had pointed. (1931-1958, vol. 7, section 218)

This expansion of Peirce's 1877 suggestion that 'logicality might 'result from the action of natural selection' would thus imply that, thought triadically, 'the action of natural selection' would be the semiotic action of experience forming habits and habit *feeling like* instinct. But now more specifically, when the Thirdness of habit gives rise to the Firstness of the instinctive impulse to launch and manage the Secondness of experience through trial and error, that experiential Second itself moves triadically through the process of guessing (abduction, a First), testing guesses (induction, a Second), and drawing conclusions (deduction, a Third).

This is of course the schematic framework behind my Wheel of Experience in *Becoming a Translator* (2020, p. 60), which we might adjust for the study of interepistemic translation. The interesting contribution Peirce's abduction-induction-deduction triad can make to Kuhn's theory of inter-epistemological-community translation, as I began to suggest earlier, begins in the fact that the 'metadoubt' or secondary doubt experienced in the encounter with the discourse of a competing epistemological community launches a search for belief, and that belief takes a progressive series of forms that culminate in the selection and formation of a new normative paradigm. But each of those doubt-seeking-belief trajectories might be thought of as an abduction-induction-deduction triad in its own right, with reference to Kuhn's nine stages:

- *First trajectory: isolating areas of difficulty in inter-community communication*. The observed fact of divergent epistemological communities (stage 2) gives rise to doubt, which generates the abductive guess that there are discursive differences as well, leading to the '[inductive] attempt to discover the terms and locutions that [can] nevertheless [be deductively identified as] foci of trouble for inter-group discussions.' (stage 3)
- *Second trajectory: becoming good predictors of other communities' behavior*. The fact established in the first trajectory that there are areas of communicational difficulty between communities gives rise to uncertainty (doubt) regarding the very possibility of communication, which generates the abductive guess at the possibility of understanding and even translating the alien discourse, leading them to 'try [inductively] to discover what the other would see and say when presented with a stimulus to which his own verbal response would be different,' with the deductive result that 'they may in time become very good predictors of each other's behavior.' (stage 5)
- *Third trajectory: the possibility of persuasion and conversion*. The observed fact that without translation 'many of the explanations and problem-statements endorsed by the members of one scientific group will be opaque to the other' creates doubt, which generates the abductive postulation of translation as a possible solution, and the inductive testing of hypotheses generated by that abduction 'allows the participants in a communication breakdown to experience vicariously something of the merits and defects of each other's points of view,' leading deductively to the possibility of 'persuasion and for conversion.' (stage 6)
- *Fourth trajectory: going native*. In this final trajectory, which effectively means persuasion and conversion, the abduction-induction-deduction triad is largely unconscious: 'at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made' (stage 9). Somehow one has continued to move from doubt to belief by responding to uncomfortable facts first abductively, then inductively, then deductively, without the 'individual … mak[ing] or refrain[ing] from making [the transition] by deliberation and choice, however good his reasons for wishing to do so' (stage 9). The fuller title of this trajectory would be 'go[ing] native, discover[ing] that one is thinking and working in, not simply translating out of, a language that was previously foreign.' (stage 9)

The fourth trajectory there is effectively the one that brings the competing epistemological communities into alignment, generating the new normative paradigm. That is of course not the telos of every interepistemic translation project/process. Some will want to stop at the second or third. Others—especially those labeled 'experimental'—may want to stop at the first, exaggerating and playing with the 'the terms and locutions that [can be deductively identified as] foci of trouble for inter-group discussions.'

Still, it is arguably useful to have the full model for reference. Negotiations and arbitrations aimed at consensus—or at making sure both or all contending parties are at least equally unhappy—would fit Kuhn's model from the history of science. The idea that experimental translation, might be thought of as harrying those 'foci of trouble' is intriguing. And if most interepistemic translation projects tend to fall into the middle, into trajectories 2 and 3—which may or may not be the case, but imagine that as an abductive guess—that would only suggest that interepistemic translation tends to be more exploratory than conciliatory (trajectory 4) or disruptive (trajectory 1).

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