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Response

A thousand words is worth a picture

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Abstract

In this response to Ribeiro and Lima's paper on interactional expertise we argue that, by not incorporating the insights of constructivist social science, their analysis goes backwards rather than advancing the debate. We show that much of the evidence they present as antagonistic to the concept supports it. We also critically examine the idea of physical contiguity, which forms a central part of Ribeiro and Lima's position. We show that its meaning is ambiguous. We conclude by suggesting that more research on the nature and influence of physical contiguity would be interesting in its own right but that it would not bear on the notion of interactional expertise.

Keywords

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Critique in context

It is gratifying when a new concept is widely discussed and criticized. It shows that, even if not everyone is completely happy with the concept, it is making an impact. Also, it is only through the to-and-fro of debate that a concept can become fully worked out and refined. In the case of *interactional expertise* (IE), this process is already happening. As Ribeiro and Lima (2016) note, two other recent critiques of IE – Plaisance and Kennedy (2014) and Goddiksen (2014) – call for an extension of the concept, both to make it more inclusive, and also to increase the number of people who could claim to be interactional experts. In contrast, Ribeiro and Lima (hereafter R&L) either want to dismiss the idea of IE as empirically unsupportable, or to claim that all deep understanding must be based on practice.

The timing of R&L's critique is somewhat unfortunate. We replied to Plaisance and Kennedy and to Goddiksen, in two long analyses that bear directly on issues raised by R&L. The first analysis was of the concept of IE (Collins and Evans, 2015), and the second was of the concept of contributory expertise (Collins et al., 2015). In addition, there is a third recent paper by one of us (Collins, 2016), the second half of which is directed specifically at the embodiment thesis and its relationship to the idea of IE. Of course, R&L had no direct access to these papers when writing their own, but Ribeiro was present at several conferences where the ideas from all three of the papers were presented.

Instead of working through R&L's paper line-by-line and providing links to the relevant parts of our responses elsewhere, we will use this opportunity to do two things: first, we will try to clear up some of the more important misunderstandings, in the hope that future discussions can be built on a more secure footing; second, we will try to move the discussion forward, by examining more closely the idea of physical contiguity that is central to R&L's argument, and showing that the idea cannot bear the burden R&L place upon it. As this reply is necessarily brief, we encourage readers interested in a more detailed treatment of the subject to refer to our recent work on IE (Collins and Evans, 2015; Collins et al., 2015; Collins, 2016), as well as the paper by Reyes-Galindo and Duarte (2015) that provides an additional response to Goddiksen (2014). These are all now available in print or electronic formats, and are referenced in the bibliography.

Ribeiro's reverse conceptual trajectory

Much of what R&L argue has a reverse conceptual trajectory, attempting to restore the status of old ideas. Ribeiro completed his PhD under the supervision of Collins and Evans, and during this time co-authored two papers with members of the Cardiff group, (Collins et al, 2006, Ribeiro and Collins, 2007). Ribeiro later arranged to spend a year at Berkeley, working with Hubert Dreyfus, and developing a wider philosophical perspective on expertise. Dreyfus is a leading expert on the philosophy of Heidegger, he has published a very well known 'five-stage' theory of expertise (with his brother, Stuart Dreyfus), and, he is the author of a pioneering critique of the claims of artificial intelligence (Dreyfus, 1972). Unsurprisingly, the phenomenology of Dreyfus plays an important role in R&L's critique, which reproduces much of the long-running disagreement between Collins and Dreyfus over how individuals become experts. For Collins, the individual is essentially *social* and becomes an expert through *socialization*; for Dreyfus, the individual is essentially *embodied* and becomes an expert through *practice*. The importance of this difference is not always apparent, however, since socialization seems to depend on embodiment. Indeed, though he and Dreyfus have had many friendly meetings and discussions over the years the significance of the difference between their world views became fully clear to Collins only in August 2015, when Dreyfus and he presented successive keynote addresses at a conference in Greece (the latter became Collins, 2016). As Dreyfus made plain at that meeting, and as R&L acknowledge, a central source for his phenomenological work is Todes's (2001) *Body and World*, which was originally written as a PhD thesis in the 1960s. Todes is solely concerned with the role of our bodies in our experience of the world and thus writes in the introduction of that work:

> The reader is forewarned that the analyses presented in this study are not of our normal experience in its full complexity. ... Thus, for example, for the purposes of this study of the human body as the *material* subject of the world, our experience is simplified by disregarding our experience of other human beings.' (Todes, 2001: 1, italics in original).

For a sociologist, such a reductive and asocial approach ignores all the by now standard social constructionist accounts of how meanings are made in and through social interactions. Of course, these ideas were unavailable to Todes or to the other important figures in phenomenology who influenced Dreyfus, such as Merleau-Ponty and Heidigger.[1] They are, however, available to R&L, who appear to choose not to use them. Instead, R&L restate the

determinedly individualistic position against which Collins was reacting when he developed a sociology-of-knowledge-based critique of artificial intelligence in the 1980s and 90s.

Why does this matter? Perhaps the easiest way to see the importance of socializing expertise, and to avoid the limitations that over-emphasis on embodiment brings, is to foreground the fundamental problems that the notion of IE resolves, always asking how we would resolve them without it. To give one example, consider the division of labour in technical specialties discussed in Collins (2011), and the kinds of knowledge STS fieldworkers can have of these practices. As we argue below, IE provides a much richer and more consistent account of the ways in which this expertise is shared than accounts that rely on embodied experience and an under-theorized capacity to extrapolate. This is why, among other things, the criminologist can understand crime without having to steal or murder (Collins and Evans, 2015).

Collective and individual embodiment

This failure to deal adequately with the different ways that language and practice combine at the collective and at the individual levels is the main weakness in R&L's critique. The social dimension of human experience, which includes language, is central to the idea of IE. At the collective level, a form of life has to be a combination of language and practice – as is explained in Collins (2011), there can be no tennis language without tennis players. But, this relationship of language and practice at the collective level does not imply that to understand tennis one has to be a tennis player, any more than it implies that to understand what a chair is one has to be able to sit on one (Collins,1996a, 2016; Collins and Evans, 2015). At the collective level, there can be no tennis without tennis practice and no chairs without sitting, but the situation must change at the individual level, or someone who could not sit would not know what a chair was. The distinction between the collective and individual levels may be wrong, but, given the central role this distinction plays in the concept of IE, it is surprising that R&L ignore it.

In fact, recognizing the difference between the individual and the collective would lead R&L to draw a very different conclusion from one of their arguments against IE. Consider their reference to the case of Madeleine, and its relevance for the *minimal embodiment thesis*. Here, R&L had the opportunity to move the debate forward, because the case has been used in two different ways: to show that IE was what allowed Madeleine to become fluent, in spite of her disabilities; and to argue for the *minimal embodiment thesis*. The two uses are somewhat tangled and, inter alia, R&L's arguments go some way to disentangling them.

The minimal embodiment thesis goes like this: If it is the case that an individual can understand a collective practice from language alone, then an individual can understand practice without possessing any more in the way of a body than that required to process speech. Note that this thesis does not apply at the species level, and Collins argues elsewhere (1996b) that a disembodied species could not develop human-like culture. Note also that if one takes into account the ways certain kinds of physical prostheses have developed – consider the case of Stephen Hawking – it is arguable that an individual needs even less than the minimal body to understand speech and therefore practice – just a brain in vat supported by prostheses. But it does not matter if the minimal body thesis is true, insofar as the idea of IE is concerned, and R&L's use of the case does a service by cutting through some earlier confusions. They write:

> Madeleine was a full[y]-fledged social being. Her blindness and disability did not impede her from playing roles as a daughter, a friend and perhaps as a sister or an aunt. She had enough of a body to feel loved, hated, sad or happy. She also participated, in one way or another, when being cared for, fed, washed and carried around. Moreover, we can infer that she was able to ask for food or water when she was hungry and thirsty, to talk about the food and juice people offered her, to appreciate those who were kind and to thank them, to complain about her bad days, to participate in other people's lives by listening and giving her opinion, to request a doctor when feeling sick and so forth. In short, Madeleine knew 'how to do things with words'. (R&L, 2016: 15)

For the sake of argument, let us accept this description of Madeleine and agree that Madeleine has much more than a minimal body, rendering Collins and Evans's use of her case in support of the minimal embodiment hypothesis flawed. In addition, let us accept that, in Selinger's terms, she can extrapolate from those parts of her body that she did possess to abilities that she could not actually experience. If we do this, we understand, in the terms used by R&L, that one can learn to 'do things with words' even if one's practical engagement with the world is severely limited.

We can ask, then, what is stopping Madeleine from extrapolating from these experiences to a full understanding of other aspects of social life? As R&L say, Madeline is a 'fully-fledged social being' – there is no suggestion in their account of Madeleine, or in any other description of the case, that Madeleine's discourse is restricted to utilitarian requests related to her own bodily state. So, where does her understanding of 'other people's lives', and the legitimacy of her opinions, come from, if not from her conversations? This reasoning is perfectly consistent with the idea of IE, which provides a way of linking the individual to the

collective. The case is impossible to explain if Madeleine's linguistic abilities are restricted to what she has experienced directly. Ironically, in their determination to show that the minimal embodiment thesis is not supported by the case of Madeleine, R&L make the wider case for us – an embodied agent that is also a social agent can understand things through language that their body prevents them experiencing directly. What, then, is to stop Madeleine learning to understand, say, gravitational wave physics, if she had enough conversations with gravitational wave physicists?

The Imitation Game

Another example of R&L's determination to show that everything about the idea of IE is wrong, that ends up undermining their own argument, appears in their discussion of the Imitation Game, which is a research method developed specifically to investigate IE's reach and significance. We focus on R&L's argument that 'creating a contrast' means that the Imitation Game more accurately measures what 'judges' do, rather than what 'pretenders' do. As with the minimal embodiment hypothesis, there are some useful insights in R&L's analysis, but these are more consistent with the idea of IE than its refutation.

The Imitation Game is a quasi-Turing Test in which a 'judge' from the relevant community, someone who possesses the necessary 'target expertise', sends questions to another member of the same social group (the 'non-pretender') and a member of a different social group (the 'pretender'). The non-pretender answers naturally, while the pretender answers as if he or she were a member of the target social group. The initial hypothesis was that, if the pretender group has many social interactions with the non-pretender group, it would be able to develop IE in the target domain and so provide more plausible answers. In such a case, it would become more difficult for the judge reliably to distinguish between the two sets of answers than in cases in which the pretender has little social contact and therefore no IE in the target expertise.

R&L use the examples of the 'colour-blind' pretending to be 'colour-perceivers' (and vice versa) and 'pitch-perceivers' pretending to be 'pitch-blind' (and vice versa), which were drawn from a paper on which both Collins and Ribeiro were authors (Collins et al., 2006). R&L's criticism of this work makes two claims: IE does not explain the results, but the embodied experience of participants does. Neither of these arguments are persuasive. In the first of these arguments, R&L argue that the *early analysis* of the Imitation Game data is flawed because it focuses solely on the knowledge of pretenders, and hence explains results

purely in terms of what pretenders know or don't know. As we have made clear at many conferences, we are now working with a far more sophisticated understanding of the Imitation Game. We now see that all three participants – the pretender, the judge, and the non-pretender – contribute to the outcome (see Collins et al., forthcoming). We do not, then, disagree with R&L that the judge matters; the disagreement is over what this means for interpreting the data and for the idea of IE.

The main contention of R&L is that successful judging involves creating a contrast between the judge's group and the one from which the pretenders are drawn. One way to do this well is for the judge to alternate between the two social groups and hence to identify the points at which one can be distinguished one from the other. Thus, R&L write:

> because colour-blind people socialize with colour-perceiving people, they already know that the latter have the erroneous idea that colour-blind people 'read' traffic lights based on the location of the lights – top, middle or bottom. Because of this, colour-blind people know that this is a discriminating question [that can be] posed in order to discern who is pretending to be colour-blind. In contrast, when an individual is not subjected to alternation, the difficulty in realizing the taken-for-granted practices of one single form of life translates into a difficulty in articulating such experience. (R&L, 2016: 18)

Here, R&L conflate two separate issues: IE and reflexivity. In the case of the colour-blind judges who know what colour-perceivers think about the experiences of the colour blind, R&L are describing IE; it is knowledge of the form of life inhabited by colour perceivers acquired through social interaction with them. The parallel with Madeleine's ability to understand the lives of others and hold opinions about them based on her social interactions with them seems obvious and yet, once again, R&L do not seem to notice that the behaviour they describe depends on the very ability they want to deny! How else would the judge put themself in the position of 'knowing' what the pretender group 'knows', given that, by definition, they cannot have experienced it directly for themselves?

The problem of reflexivity, which might explain why judges from dominant or hegemonic groups struggle to come up with discrimination questions, is quite different. The problem might be due to the judges' inability to alternate – and hence due to an absence of IE – caused by limited or asymmetric social relations, through which judges learn little or nothing about the other community. In such circumstances, judges might well struggle to create a contrast, as R&L suggest. Even so, the pretender still has to provide plausible answers, based on their understanding of the dominant culture for the chance outcome we observe in the data to

occur. Again, nothing that R&L say provides a better explanation for this than the argument that the colour blind, or any of the other populations in which we observe a similar pattern (see Collins and Evans, 2014), have developed IE about the target culture by virtue of their repeated social interactions with its members.

Had R&L approached the problem differently, there is a question here that could be pursued more fully: How does the balance between IE and social interaction vary across topics? Three possibilities spring to mind. First, we can imagine Imitation Games in which judges could succeed without needing to pose questions that draw on the in-depth social interactions that IE requires. Imagine that the Game was being used to distinguish native English speakers from unilingual Chinese speakers: The judge would not need to speak Chinese to recognize that the pretenders were failing to speak English. Some Imitation Games concerned with social issues work that way, suggesting that some Imitation Game judges' ability to discriminate ments can be explained without much reference to IE. Second, there are the cases, like that of the colour blind, where judges have some understanding of pretenders' form of life in virtue of their social interactions. Here, judges are able to use this understanding – based as we see it on IE – to alternate and identify points of contrast. Finally, there are cases, illustrated by the colour perceivers in the studies that R&L cite, in which there is a high degree of social interaction but little or no ability of the part of the judges to alternate; pretenders, however, would still need IE to provide plausible answers. Thinking through these dynamics would lead to a richer set of a priori predictions but it does not seem to challenge the idea of IE in any significant way.

Confusion of 'necessary' and 'efficient'

The previous two sections dealt with those elements of R&Ls critique that could be seen as offering some useful clarification or extension of the published material on IE. There is, however, a key element of R&L's argument – the division of IE into different levels based on the type of immersion – that we cannot approach in the same way. This is a shame, as it would be very useful to develop a classification of types of IE, as it is obvious that successful socialization cannot be a flip-flop process and there must be some in-between stages that fill the gap between the halting utterances of the novice and the fluent discourse of the expert.

Unfortunately, R&L's attempt to create levels of IE is fatally flawed in both conception and implementation. What they say is the 'level' of IE depends on the 'type of immersion' experienced by the learner. The idea of types of immersion, which was developed by Ribeiro

(2013a), has three central classes: 'linguistic socialization' (alone), 'physical contiguity' (linguistic plus visual) and 'physical immersion' (doing the practice). On our account, the first two could give rise to IE, with the second being more efficient than the first, whilst only the third could give rise to both contributory expertise and IE. In principle, however, according to the way that IE was initially defined, all three types of immersion could lead to exactly the same level of IE (i.e., full fluency in the practice language). In contrast, R&L make the very different claim that 'the type of immersion individuals undergo within a practice defines their abilities and understanding with respect to that particular practice'. That, of course, is exactly contrary to the quintessence of the idea of IE, the surprising and counter-common-sensical feature of which is precisely a mismatch between the level of immersion and the expected level of expertise (one can gain practical understanding with no practical experience). This makes their proposal to '… adopt the terms *pure*-IE, *special*-IE and *typical*-IE here in order to link IE to how it is developed' (R&L, 2016: 10) at best tendentious, as the definitions are incompatible at the outset with the ideas they are criticizing.

There are, in fact, two problems with the classification. The other is that the data presented to support it – and particularly to support its use as refutation of the bolder conjectures made to drive IE research – seem very thin and do not answer the crucial question. For example, although their Tables 1 and 2 contain many examples and quotes that appear to fit R&L's typology, it is hardly surprising to find that trainees found a site visit in which they were able to see their future place of work helpful (R&L, 2016: 8, 11). Unfortunately, this tells us next to nothing about the extent to which a longer, more intensive immersion in linguistic discourse could have achieved similar results if the aim was purely to learn the language and not master the practice to which it refers. All we can conclude from R&L's tables is that those elements of the training programmes that relied on more restricted types of immersion did not result in participants gaining high-levels of IE. But, as they were not designed to do this, it is hard to know what – if anything – should follow. The empirical evidence appears to miss the philosophical point. It is no surprise that exposure to practice is an *efficient* way of learning a language, but the question is whether such exposure is the *only* way to learn a language.

In fact, a classification of this sort is of no help for anything but the most trivial of questions. Imagine that I am a sociologist wanting to acquire IE in a new technical domain. Where

should I go to acquire it? Obviously, I am going to go where the experts are and the experts will be practising their crafts, so I am virtually certain to experience some of what R&L would call 'physical contiguity' regardless of whether or not physical contiguity is strictly necessary to the acquisition of IE; one cannot imagine one doing one's fieldwork any other way. And if actively working with the participants were an option (as it was in the case of Collins and Harrison, 1975), one would be a fool not take advantage, as it would increase one's immersion in the discourse of the group one was studying. A picture is worth a thousand words, but the helpfulness of seeing things does not make seeing *necessary*. After all, the corollary is that a thousand words is worth a picture, and it is worth noting in this context that, in his early work, Collins (1974) showed that, where laser scientists did feel themselves bound to allow their competitors to see their working lasers, they were very careful about what they *said*! Without additional kinds of argument, R&L's classification of IE cannot be taken as more than a description of approaches to the efficient acquisition of IE by both fieldworkers and novices. It is no surprise that translators felt their acquisition of IE was aided by their being able to get themselves close to the practice – it probably did – but that doesn't make closeness to practice necessary.

R&L seem to misunderstand the philosophical nature of the idea of IE. As we explain in various places, the 'Strong Interactional Hypothesis' (SIH) is a Popper-type 'bold conjecture', meant to push forward the frontiers of research (Collins and Evans, 2015). Like a thought experiment, a bold conjecture does not have to be directly empirically grounded – we don't need cats in boxes, with flasks of cyanide and radioactive isotopes, to see the point of the Schrodinger's Cat thought experiment, or for it to have empirical consequences. It certainly is hard to prove the existence of what R&L call 'pure IE', IE that has been gained through linguistic socialization alone, without any physical contiguity. As we have recently written: 'It is hard to prove or disprove the SIH because one must show that persons failing tests did have the opportunity to gain maximal interactional expertise, while it also has to be shown that persons passing the test had gained their understanding purely from linguistic sources. Both conditions are rare, hard to fulfil and very easy to forget' (Collins and Evans, 2015: 116).

That the idea has, nevertheless, empirical consequences and great explanatory power is what is shown in the various papers. But the difficulty of proving it could easily leave us open to the charge made by R&L (2016: 11): 'There is no empirical evidence – not a single example

– of pure-IE'. To establish this claim, they list what they say is all the fieldwork support there is for the notion. Unfortunately, they do not mention the most relevant experiment, which is reported in a paper they cite (Collins and Evans, 2014): the experiment with the blind.

Though it is extraordinarily difficult to bring empirical evidence to bear on the SIH, the fieldwork on the blind comes close. Gaining IE nearly always involves physical contiguity but the blind, if faced by the right questions, cannot have relevant physical contiguity. The field study on the blind is particularly relevant because it was set up to disprove the SIH. Not believing it could be more than a guide for action, we set out to show where it broke down. The surprise was that even under these extreme circumstances, it did not break down. Here is a question from a sighted judge and the answer provided by a blind person pretending to be sighted:

> Q: How accurately would you say a human can judge the flight of a tennis-ball? I mean, would you say they could tell the difference between touch[ing] the line and 1 mm out 2 mm out 1 cm out, 2 cm out, or what, and what would it depend on?

> A: I think often a tennis player is not in a position to judge accurately as they are not usually parallel with the line. I think that if you set up a test for a line judge with two balls one which landed on the line and one which landed 1 mm away from the line, I don't think they could tell the difference. If you think how small 1 mm is then it would be so hard for them to judge.

Let us point out that these experiments were done with participants who were not congenitally blind but who became registered blind in the early years of their lives. Judges were informed of this and were instructed to ask questions that pertained to adult life only. In this case, the participant would not have encountered tennis in their very early sighted life nor, given their age and nationality, would they have encountered millimetres. This may not be a perfect experiment, but it gets close.

One can see why experiments on those with unusual physical and perceptual abilities are particularly salient when it comes to what R&L call 'pure IE'; everyone who is able will be getting as much physical contiguity as they can while they are acquiring IE, so cases where physical contiguity is necessarily limited are particularly interesting. What the experiment with the blind shows is that whilst physical contiguity may be efficacious for acquiring IE, it is not necessary.

Is there a positive direction in which R&L's analysis can take us?

Ribeiro's idea of levels of immersion applies well to his superb fieldwork on the training of novice mining operatives (see e.g., Ribeiro, 2013b), or, generalizing, could apply to the pedagogy of practice. But this typology of immersion does not give rise to a philosophical critique or a classification of IE, any more than an analysis of educational practices in schools would give rise to an epistemological critique or a classification of types of knowledge.

Seen as an exploration of how best to transfer knowledge, one very interesting question that follows from Ribeiro's classification of types of immersion is to ask what is meant by 'physical contiguity'.[2] Ribeiro has observed that though Collins has passed an Imitation Game test in the esoteric domain of gravitational wave physics, he acquired his IE in conditions of high physical contiguity. But what does high physical contiguity mean?

The question was partly motivated by a coincidence – on the same day that Collins and Evans received their copy of R&L's critique, Collins was told an interesting story about Francis Halzen, the instigator and Director of the IceCube project. IceCube is a cubic kilometre of ice at the South Pole instrumented to detect neutrinos; it is a $270M project. Collins was told that Halzen had never visited the South Pole and the relevance of this to the notion of physical contiguity struck him. Being acquainted with Halzen through previous email exchanges regarding his book, *Gravity's Shadow*, and its account of 'big science' which Halzen had found useful, arranged a telephone conversation. For Collins, the conversation was initially disappointing, since Halzen explained that, though he had not visited the South Pole, he had been present when the components of the apparatus were being built near his home university, the University of Wisconsin. Most of the work had been done at the Physical Sciences Laboratory, 20 miles south of Madison, and Halzen had visited it frequently. Collins and Halzen agreed that the latter's not visiting the South Pole was no more remarkable than if a scientist had built a seismic detector to be placed on the Moon and had not travelled to the Moon with the astronauts to witness its installation. Note, however, that we have already accepted as a matter of course that Moon scientists do not have to visit the Moon and so we have at least begun to open up a question about the necessity of physical contiguity. But Halzen also stressed that it was a remarkable thing that he was a theoretician. The experiment was a matter of new kinds of apparatuses making new kinds of measurements, involving work for which he had no special skills. He made visits to the lab because he had to talk to the engineers, not because he needed be close to the apparatus itself.

This is a useful anecdote but, perhaps, not decisive. But it raises the question of exactly what physical contiguity is and what it does. Consider, however, what one could learn from a visit to a gravitational wave detector site – they have many visitors on their outreach programs.

If you visited such a site, you would see some concrete covers of roughly semi-circular cross-section about 20 feet high, stretching for two-and-a-half miles in each of two directions at right angles, from a large central building and with smaller building at each end. Here and there would be doorways into the concrete covers, and if you entered you would find a tunnel filled with debris and long tubes four or five feet in diameter, encased in yellow fibrous insulation. If you went into the big building you would find meeting rooms, places to eat, workshops, and so on. You could look into the control room and see large screens on the wall showing all kinds of displays that you would not understand (Collins doesn't understand most of them) and scientists sitting at consoles looking at the screens and adjusting things. If you were allowed into the heart of the machine, after donning overshoes and laser-proof glasses, you would see a hangar-like structure with the ends of the stainless steel tubes entering large domed structures with access ports bolted shut (see, e.g. Collins, 2004). You would see nothing of the meaning of laser-interferometric detection of gravitational waves. It would not be dissimilar to what visitors learn about Higgs Bosons by looking at the beam tubes at CERN. ~~Do such a site visits involve physical contiguity, and if so what does physical contiguity do?

One thing site visits have done for Collins, as for Halzen, is to get him close to the people with whom he has needed to talk. It has also given him 'street credibility' which could help those conversations along. Whenever he has met a scientist, at a site, or at a remote conference, he has been able to say: when I was at Hanford I often crossed the bridge and sat down on the inside of the 'L' where it was completely quiet. Then I could watch the sun set over Rattlesnake Mountain; it was beautiful.' Or he could say, 'have you noticed that in the conference room the backs of the chairs all have sticky labels on them saying "Super-Conducting Super-Collider"?' All that is establishing the conditions for easy social interaction but it does not seem like a *necessary condition* for the development of gravitational wave IE, nor would its content be technical – something that would directly help one pass an Imitation Game test (just look at the questions and answers in articles by Collins and Evans (2007: 105) or Giles (2006)).

But there is a case described by Collins (2004: 773) where it seems that physical contiguity was directly important, and it is a significant case that helped to establish the idea of IE. Collins visited the relatively small (100 meter arms) interferometer known as TAMA in Tokyo. His host explained to him that TAMA was suffering from unexplained noise and that many people thought this was because scattered light was reflecting off the inside walls of the beam tube and interfering with the light-sensitive feedback loops. But his host also said that it was not true and this could be demonstrated because the beam tube could be knocked or shaken without affecting the output trace. Collins cannot remember whether or not he tried the experiment himself. Subsequently, at a conference in Kyoto, Gary Sanders, the project manager of LIGO, told Collins that TAMA suffered from stray light scattered from the inside of the beam tubes. Collins, however, was able to convince Sanders that this was not true, by referring to the shaking of the beam tube. This incident reinforced the idea of IE, because Collins, a non-physicist, had been able to convey credible physical understanding to Sanders. The point is that it doesn't matter whether Collins actually shook the tube or merely heard an account of the experiment. What is sure is that Sanders did not shake the beam tube but changed his physical understanding just from hearing about it. Sanders may well have never visited TAMA. Both Collins and Sanders only had to describe the experiment for it to be as though they had actually shaken the beam tube themselves and perhaps the same goes for the readers of this article: It may be that one element of IE is the countless visualisations of this kind that verbal interactions make possible. So, the question is: In terms of this specific example, who has physical contiguity and what is physical contiguity doing? What is there that you could not learn from this description of beam-tube shaking, if you had never seen a beam-tube? What more could you understand about it by visiting one of the LIGO sites, and in terms of this piece of physical understanding, would a visit be an increase in physical contiguity or not?

Thus it is unclear, contrary to the claim of R&L, that Collins had lots of physical contiguity with the practice of gravitational wave physics. In fact, it is not so clear what the claim means. Collins certainly visited all the sites and wrote descriptions of visits to Louisiana, the Australian bush, the corner of a field in Hanover, and so on, but the nearest thing to physical contiguity with the heart of the technology that he had was the encounter in Tokyo. As for the rest, he might just as well have been a tourist, just like visitors touring a gravitational wave detector site. The aspects of these visits that were different from those pertaining to tourist were intimate and, to some extent, confrontational, conversations, commensality and so forth.

We do not think the above passage provides answers to the question of what physical contiguity comprises but instead raises interesting questions about the very concept itself. It turns out that when we look closely at the concept, we do not know what it means. This is usually an indicator that there are interesting things to be worked out. We certainly hope that R&L will try to take the matter forward rather than back.

The fundamental questions

To repeat, perhaps the easiest way to avoid being sucked back into the ideas of the importance of embodiment that do not benefit from the more recent 'socialization' of the notion of knowledge is, once more, to keep in mind the fundamental problems that the notion of IE resolves and always to ask how we would resolve them without it. Consider the esoteric expertises and controversies that are the mainstay of STS research. If we are to have specialists who can co-ordinate their actions or criticize the work of competitors without engaging in each other's physical practices – and that is what having specialists implies – how can they do it other than by understanding each others' form of life though the medium of language – interactional expertise? It would be good if every specialist could spend time at the home laboratory of every other specialist, so as to speak with them in conditions of high physical contiguity but this is logistically impossible. Fortunately, just as Francis Halzen does not have to visit Antarctica, and a Moon scientist does not need to step on the Moon, a cooperating community can use the thousands upon thousands of words spoken at common meeting places to develop the common language that enables co-ordination of judgements and actions.

Notes

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[1] A reviewer of Todes's book writes:

> the book bypasses entirely the fundamental human experiences of sociality and language – instead one could read Todes thinking that humans are hermits working out the meaning and efficacy of their participation in the world. The kinds of insights later hermeneuts and constructionists offer – that the categories we use to make our experience know-able and habit-able are accessible human and cultural constructions – were not available to Todes. (Strong, 2004: 521)

[2] Oddly enough, Collins provided the *term* 'physical contiguity' to fit Ribeiro's idea.

Author biographies

Please fill in short bios. Thanks.

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