The Permeability of Actor and Analyst

Typically, the many criticisms directed at the Third Wave of Science Studies fall into one of two categories. 'Science war' does not seek to engage the original authors but aims to sway a wide audience; in contrast, 'science debate' takes place among specialists and aims to produce a new, shared understanding.[1] Misrepresentations of the opponent's position work well in science war whereas science debate fails without sincerity. Coopmans' and Button's (CB's), 2014, critique is science debate not science war and this means there is a chance to move things forward.

But note – we are already into the substance as well as the style of the disagreement. 'Science war' and 'science debate', as conceptualised here, are 'analyst's categories'. Yet introducing them here has the potential to convert them into actor's categories. Furthermore, it is at least possible that the categories will change the world, perhaps making actors more wary of going to 'war' in case their strategy is recognised (or, sadly, encouraging them to make more use of the strategy). Whatever the outcome, the very possibility shows that actor's and analyst's category shade into each other. Analysts can create categories that later become integral with actors' lives. More examples follow.

CB's detailed description of the skills of Singaporean graders of diabetic retinopathy seems unexceptional and, in so far as we are able to judge it, well done. It looks like a more detailed version of the kind of thing we do when we describe the work of gravitational wave physicists or econometric modellers. But, again, we are into substance. We have just made a judgment

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[1] 'Science war' is here used as a generic term (see also Collins, 2014, p88), exemplified by the notorious, 'Science Wars' of the early 1990s.

about CB's competence in describing the Singaporean workers even though we know nothing about Singaporean grading of diabetic retinopathy and not much about the actual work that CB did. More to the point, hospital administrators must also regularly make such judgments about the staff they employ. How they do this ought to be a puzzle for CB because, by their own argument, the expertise of grading exists only in the practice of grading. In contrast, administrators, assuming they are not omnipresent, ought to fail to understand this expertise, just as we ought to, because, like us, they: 'maintain some distance from how people display, assert, attribute, negotiate, contest, and experience expertise in actual courses of action and interaction'. From a Third Wave perspective, however, the explanation is clear: hospital administrators have to judge expertise in Singaporean diabetic retinopathy by using a 'meta-expertise' resting on their understanding of how society works. Again, it is hard to see who is the analyst and who is the actor, and hard to see why there cannot be sociological studies of meta-expertise now that that the category has been established.[2]

Furthermore, today's societies exhibit complex divisions of technical labour between specialists. By definition, different specialists are not able to know each others' expertises in detail so collaborations are managed through common possession of a 'practice language' (Collins, 2011). This insight rests on the analyst's category of 'interactional expertise'. Without some analytic thinking of this kind it is difficult to explain how CB grasp enough of Singaporean diabetic retinopathy to describe it, since they do not seem to have practised it themselves; what is going on within their own study? This is not to say they cannot achieve the analyses rather in the fashion that Richard Feynman proclaimed that scientists could do

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[2] As far as we can see the ethnomethodological term 'unique adequacy' merely comforts the analyst, relieving him or her of the responsibility of working out how far the ability to analyse extends from very local practices. This, interestingly, is a problem we have to solve in life as well as in analysis if we are to get about in the world with confidence. Elsewhere, we discuss the question of how far expertises extend under the heading of the 'fractal model' (Collins, 2011).

science without analysing what they were doing: `Scientists need philosophers of science like birds need ornithologists'. One would have hoped, however, that social scientists would be more ambitious in respect of understanding their own practices.

As the idea of interactional expertise becomes more widely understood and adopted, still more changes could come about. The sociologist-expert on the science of fingerprint analysis who tends to be 'rubbished' in court because he cannot identify fingerprints could be honoured if the notion of interactional expertise were widespread (Collins and Evans, 2007). The idea could also bear on who contributes to technological policy-making (Collins, 2011, Collins and Evans, under submission). In sum, analyst's categories bear frequently and unavoidably on aspects of expertise and are, in any case, difficult to separate from actor's categories.

Of course, to work this way, analysts' categories cannot be pure theoretical conceits; they must be founded in actors' ways of being in the world. They are what Winch (1958) called 'technical concepts' growing out of an understanding of actors' forms-of-life. That does not mean actors can simply provide everything that needs to be known about their worlds. Actors often work with quasi-analytic categories which are not based in their own practices. For example, scientist-actors tend to work with false models of the process of science, something that sociologists began to understand in the 1970s. Scientist-actors tend, or tended, to resist sociologist-analysts' re-descriptions of their world. In his study of gravitational wave physics Collins finds he sometimes has to claim that his analyst's categories are superior to the actor's categories. He claims that given enough time it is possible to persuade actors that this is the case so long as the analyst's categories are truly based in the actor's world. Part and parcel of

verifying a sociological analysis is interacting with actors and persuading at least some of them to see the world in the new way.[3]

Our model, then, is one of positive feedback between analyst and actor. The analyst must acquire actors' categories (often via interactional expertise) but then use *sociological expertise* to reflect upon them, correct and refine them, and categorise them. The sociologist's model of the world can feed back into the actors' world and sometimes change it. For some reason, CB want to short-circuit the feedback, cutting out the reflective part and limiting discussion of actors' worlds to the actors alone (though, of course, 'brought to you by analysts').

Taxonomies for All

CB's starting point is to 'respecify' the sociological problem of expertise as one of how expertise is produced and made accountable within a specific context. This allows them to create the contrast between a 'formal analysis' (i.e. our supposed 'theoretically derived classifications' and/or 'abstract definitions') and their own approach. Ironically, in explaining how the graders perform their expertise, CB make a strong argument for the value of a 'Third Wave' approach to expertise. Consider CBs 'Expertise and its Constituents'. In the passage they display a rich account of graders' expertise as constituted and displayed through their use of a diagnostic classification. Of this grading protocol, they say:

> Part of the graders' expertise is displayed in their making their work accountable
> to a grading taxonomy of retinopathy. It is the taxonomy that provides a structure
> for what the graders should look out for and treat as notable, or disregard and treat
> as unremarkable. In doing their work, graders display knowledge of the taxonomy
> as well as how to work within that taxonomy. This knowledge and work are thus

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[3] The position is discussed in Collins, 2013, pp. 343-4

on display, and can consequently be recognized by others working within the taxonomy who can judge the expertise of a grader by referencing his or her activities to the terms of the taxonomic structure.

In the same section, CB go on to describe how this taxonomy supports a number of activities that constitute the expert practices of the graders including: *naming* (including 'standardisation', 'economic and efficient expression', and 'objectivity'); *classification* and *displays of commonsense knowledge*. But this demonstration can be equally applied to the study of expertise itself. As CB might have said:

> Part of the [sociologist of knowledge/expertise's] expertise is displayed in their making their work accountable to a [Periodic Table of Expertises]. It is the taxonomy that provides a structure for what the [sociologist/STS scholar] should look out for and treat as notable, or disregard and treat as unremarkable. In doing their work, [sociologists/STS scholars] display knowledge of the taxonomy as well as how to work within that taxonomy. This knowledge and work are thus on display, and can consequently be recognized by others working within the taxonomy who can judge the expertise of a [sociologist/STS scholar] by referencing his or her activities to the terms of the taxonomic structure.

That is treating sociologists as experts on expertise and it is very hard to see why it is not a better way to understand expertise than the ad hoc and ill-defined distinctions between novice, neophyte, and experienced graders that are used by CB. But the same also applies to non-sociologist-actors. Consider this relatively new manager of the 30-meter telescope talking to Collins:

> I was afraid that I would not understand adaptive optics well enough. ... But I've found that, remarkably, *what you call interactional expertise* was not hard to

achieve. I couldn't design an adaptive optics system but I really do after six to nine months in the field, I really do understand the different kinds of adaptive optics in the field and the way that they work and I can draw a schematic and define the algorithm, and understand the technological readiness of the different techniques—which ones are really ready to apply to the sky and which ones need to be demonstrated, and certain components have to be developed. (Gary Sanders, Project Director of the 30 Meter Telescope talking to Collins and quoted in Collins and Sanders, 2007, p. 628-9, emphasis added)

Is this not a manager making his work accountable to a taxonomy of expertise? Once more, who are the analysts and who are the actors?

To illustrate the difficulty of separating actor and analyst still further, by pure coincidence, after the first draft of this piece was complete, Jay Marx, Executive Director of the Laser Interferometer Gravitational-Wave Observatory (LIGO) from 2006 to 2011, wrote a friendly letter to Collins (email, of June 17th 2014) asking him what he was making of the controversy over 'BICEP' and saying in passing that *Gravity's Shadow* (Collins, 2004) had helped him perform well when he had applied for the job of Director. Collins, having the actor/analyst problem in mind, asked him to expand. On June 23rd he wrote:

Many fields of science now involve collaborations of significant size. The ability of these collaborations to function requires that the leadership (and membership) be able to understand and function effectively in the culture and sociology (yes, that's the term we use) of the collaboration. In my case, Gravity's Shadow was the primer I used, before deciding to become a candidate for the LIGO Lab director's job, to understand the culture and sociology of LIGO and the GW field. The book made three important contributions to me. First, it convinced me that

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there are enough familiar elements in the GW culture and sociology so that my previous experience was relevant. Second, it provided me enough background that I feel I was able to be a credible candidate for leadership in the interview process. And third, the book gave me important backgrounds about my new colleagues in LIGO and the rest of the field. So if I contributed to the field during my time as LIGO Director, Gravity's Shadow helped make it possible.

Pressed further he wrote on 28 June, utilizing the terms, 'big and small science', which are, or at least initially were, analyst's categories:

> Here's the most important sociological insight I gained from your book— it was that the GW field is made up mostly of scientists whose background was "small" science, (i.e. done in a lab with a few or no colleagues) and these people were struggling to adjust to the culture, constraints and problems inherent in "big science" and large collaborations. Without this insight I would have assumed that the GW field was populated by people who were well adjusted to big science, because those in LIGO that I did know—Barish and Sanders were also high energy physicists. And so I would have undoubted screwed up the interview. As it was, I was able to cogently address questions about the real culture of the GW field, how I would handle certain situations, and I was then able to point to some of my previous experience dealing successfully with people from a small science background trying to utilize big science facilities.

In sum, the sociological study of expertise is not the odd and abstract thing described by CB. It is the sociologically informed understanding of how expertise works in different domains. Analytic categories are used but they are illustrated with empirical examples and, as shown above, are capable of becoming understood and adopted by actors. Indeed, what we want for

every category of the Periodic Table of Expertises is success by absorption into the commonsense world – the sociologist's equivalent of the Nobel Prize. Successful sociological concepts *become* actors' categories.

Ubiquitous Expertises: a surprising mistake

CB describe what experts in diabetic retinopathy bring to the task from the outside reflecting that, as is sometimes forgotten, Garfinkel, in the context of his juror study, stated that 'a person is 95 percent juror before he comes near the court' [Garfinkel 1967 p 110].

> Another example of commonsense competencies that relate to the work of grading is 'knowing how to determine whether two instances are similar to or different from each other'. Similarity judging comes into play when graders assess a red dot in one part of the image, which they may decide is a microaneurysm, in relation to a red dot in another part, which they may decide is an artefact. It also comes into play when graders look back and forth between the image and the SP to identify blood they can match. Like making 'more or less' judgements, similarity judging is an ordinary competency deployed to achieve many things. For example, printers judge the quality of a printed colour against a pantone standard, looking from the colour of the printed page to the standard and back again to judge similarity and dissimilarity in tone, hue, and saturation of colour. Outside of work activities, people may amuse themselves with spotting dissimilarities in seemingly similar cartoons in the newspaper.

We too assemble widely spread expertise into another analyst's category (soon to be an actor's if all goes well), labelled 'ubiquitous expertise'.

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If we knew how to 'weigh' the total amount of expertise we each possess we would find that our ubiquitous expertises hugely outweigh our other kinds of expertises. Thus, if you are the most brilliant physicist such as John von Neumann or Albert Einstein, you will have learned to speak a natural language, learned how often to wash and how close to walk to others. You will probably be literate and, nowadays, know how to use the internet. All these are remarkable accomplishments and most seem impossible to reproduce unless you are a human. (Collins, 2014 p 63)

What seems strange to us is that 'judging more or less' and 'judging similarity and difference' are utterly misplaced as examples of ubiquitous expertises. 'More or less' and 'similarity and difference' are quintessentially judgments that can only be made in an immediate context. In case this isn't obvious consider that two retinal images that are different in respect of diabetic retinopathy might well be identical in terms of, say, macular degeneration and vice-versa. Or that a significant difference in terms of the quantity of a chemical in the domain of experimental semi-conductor crystal growing (Collins, 1990) would be no difference at all in the domain of agriculture. If CB had started by reflections growing out of the category of ubiquitous expertise they would not have made that mistake because actors would never claim 'that person is a real expert in judging more or less' or 'he/she is an expert in similarity and difference' outside of a context in the way one could say it of 'riding a bike', 'grading diabetic retinopathy' or 'analysing and classifying expertise'. Nor would any amount of persuasion lead an actor to realise they understood their world better by having it pointed out to them that it was ordered by skill in judging more, less, and similarity. Indeed, we can create a new analyst's category here and now by pointing to the partition between skills that can be said to be possessed out of local context and those that can be said to be possessed only within local context. It is only the later that lend themselves to the claim that they cannot be separated from their practice.

Conclusion: methodological antisepsis

Gore Vidal said, 'it is not enough to succeed, others must fail', and it may be that many ethnomethodologists feel their contribution to the social sciences would be considered too modest if it did not displace all other approaches. It is certainly the case that ethnomethodology has defined itself as an 'alternate, asymmetric and incommensurable sociology' that can respecify the questions of more formal sociology but which does not translate back. It is this asymmetry that seems peculiar. Analysts like ourselves, who base our work in actors' categories find merit in ethnomethodology's craft practices though none in the self denying ordinance against theoretical reflection and feedback.

We are grateful to CB for providing this opportunity for revisiting our work in the context of ethnomethodology. We think we have shown that the boundary between actor's concepts and analyst's concepts is too permeable to support the antiseptic-like precautions that some ethnomethodologists apply to analysts' categories, especially given that analysts' categories sometimes provide a superior resource for understanding and they can change the world as well as describing it. Here we have revealed the permeability and feedback by discussing 'science war', 'meta-expertise', 'ubiquitous expertise', 'interactional expertise' and the influence of the last half-century of science studies.

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