Chapter 4
Actors' and Analysts' Categories in the Social Analysis of Science

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Actors' and Analysts' Categories

Let it be accepted that sociological explanation must begin with the perspective of the actor. The causes that give rise to anything that can be seen as consistent actions among actors turn on regularities as perceived by the actors first and the analyst second. If the analyst brings the idea of a mortgage to the study of the life of a tribe living in the Amazon jungle, then nothing consistent will emerge, for the tribe does not organize its existence around the idea of mortgage. Likewise, if the analyst brings the idea of the poison oracle as used by the Azande tribe to the study of life in Western Europe, nothing consistent will emerge, for western Europeans do not organize their lives around the divination of witches by administering poison to chickens. Insofar as analysts are going to develop categories of their own—analysts' categories—to do the work of explanation, those categories will have to be built upon actors' categories.

But where do actors' categories end and the analysts' categories start? In other words, given the idea of the double hermeneutic, there is still a choice to be made about the role of the two components. I want to start by thinking about how we make the choice in science studies, particularly in the analysis of scientific controversies.

Actors and Analysts in the Study of Science

From the very beginning, science studies have been beset with the problem of how much science you need to know to be able to analyze science. "Science warriors," such as Alan Sokal, insist that to understand the causes that lead scientists to switch from one belief to another one must have a complete grasp of the science itself. As Giles (2006) reports in reference to this author:

> Sokal says he is struck by Collins's skills in physics, but notes that such understanding would not be enough for more ambitious sociology research that attempts to probe how cultural and scientific factors shape science. "If that's your goal you need a knowledge of the field that is virtually, if not fully, at the level of researchers in the field," says Sokal. "Unless you understand the science you can't get into the theories." (p. 8)

P. Meusburger et al. (eds.), *Clashes of Knowledge*.

Some historians of science work this way, and in the early days there was tension, largely dissipated now, between this kind of historian and those sociologists who were less technically proficient (on the broad relations between analysts and science itself and how these lead to different outcomes, see Collins, 2004a, pp. 783–799. For myself, after discovering that my kind of work could in fact be done without a technical understanding of the science sufficient to be able to contribute to the field—and it may well not have turned out that way—the conceptual tension has been finally resolved with the idea of "interactional expertise." Interactional expertise is a deep understanding of the language of the science being studied, and it is gained through immersion in the discursive world of the actors without immersion in their physical world (see Collins, 2004a, pp. 745–782; Collins, 2004b, 2008; Collins & Evans, 2007; Collins et al., 2006; www.cf.ac.uk/socsi/expertise).[1] Interactional expertise is the ability to talk the science even if one cannot do the science.[2]

But if the idea of interactional expertise resolves the problem of how much scientific grasp one needs to be able to do the kind of work my colleagues and I do, it does not provide a rule for when part one of the double hermeneutic gives way to part two. I think that many of us have simply glossed over this problem for years. We have not even noticed that it exists. Certainly, I can say as a participant in the field of science studies that I had never really noticed that it existed until this very chapter began to take shape. In more concrete terms the problem goes as follows: Suppose I am analyzing the way Joe Weber's claims about the discovery of gravitational waves came to be rejected (see, for example, Collins, 1975, 2004a). I immerse myself in the discourse of gravitational wave physics and learn to understand all the arguments that were used by the actors in their debates with one another. Most of these arguments will be reproduced in my account of the ending of the controversy. But at a certain point I will say to the actors: "You don't really understand how your world works. I understand it better." This point becomes clear when the actors tell me things such as are contained in the following remark made by Richard Garwin, Weber's most influential critic in the 1970s:

> I do not consider you "a trained observer of human behavior," so far as concerns the gravity wave field. Science and technology move ahead through advances in instrumentation and publication of results. Not through gossip or "science wars" or deep introspection about what the other guy is thinking or what one is thinking oneself. (Personal communication, March 13, 2001)

This is one of the most important actors in the world that I take it upon myself to describe, and he is telling me that I do not understand that world—his world. My

[1] My apologies for the overwhelming number of self-citations in this paper, but it is a matter of working out the consequences of a brand new program.

[2] I am grateful to Peter Meusburger for reminding me that this point reflects a similar debate in the case of the arts. In Collins and Evans (2007) we do discuss the relationship between the sciences and the arts. We claim that an important difference is that the consumer's role in the legitimation of knowledge is bigger in the arts than in the sciences, so the nonperforming critic also has a more legitimate role from the outset. In science the right of the outsider to comment critically on the content of a science is much harder to establish.

response, of course, is that it is he who does not understand his own world. Here, then, I have thoroughly abandoned the actors' perspective. So far as I can see, I have never before even noticed that what I was doing was abandoning the actors' perspective and substituting my own contradictory perspective. I have certainly never thought about how such a move could be justified, and I do not know of any existing discussion of the matter.

Nevertheless, I think it is clear that social analysts of science do the right thing when, at a certain point, they abandon the account of the world provided by the actors and substitute their own account. Without this move there would be very little substance to the sociology of scientific knowledge (SSK). What can one say in favor of the move in the absence of a fully worked-out justification? Firstly, as in any science, justification must come to an end and one simply has to do the analysis and look to the outcome as its own justification. This is not an excuse to stop thinking about the problem, but it is a reason not to give up one's apparently successful scientific practice as soon as one has found a philosophical or methodological difficulty. (Collins and Yearley [1992] suggest that paralysis, reminiscent of the fate of logical positivism, follows from too much self-reflection on method.)

Secondly, the move is consistent, not arbitrary: The move is always made at roughly the same point in the investigation with roughly the same consequences, so it does not have a *post hoc* self-serving look about it. Furthermore, the move grows out of epistemological considerations. It is meant to show how the world of science works; the move is not designed to reach any particular substantive conclusion in the case of any particular scientific controversy.[3] The consistency of the move, irrespective of the contents of the science, holds out the hope that some good systematic way of accounting for the move in epistemological terms might one day be found.

Thirdly, as time has gone by, many of the actors themselves have begun to recognize the value of this kind of sociological perspective on their world. They do not have to become sociologists or buy into the entire sociological perspective to see that valuable understandings do emerge from this sociological approach. One might describe the situation in terms of interactional expertise and contributory expertise.[4] Social analysts superimpose their contributory expertise in the analysis of scientific controversies on their interactional expertise in the world of the actors. Sometimes this involves contradicting the actor's understandings of their own world. Those of the actors who have acquired a degree of interactional expertise in the social analyst's world have begun to see the point. They find that, at the very least, social analysts' contributory expertise can enrich their understanding of their world, if not overturn it. The positive reaction of many of the actors, painfully won over the years, is reassuring.

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[3] There are some observers who think the goal should be to strengthen the voice of the weaker party in a scientific dispute. But because it is not always clear who the weakest is, and because sometimes the weak will become strong as time passes, the prescription cannot be applied consistently even if it could be justified, and I have never seen a justification (see Ashmore, 1996; Collins, 1996; Scott et al., 1990).

[4] Contributory expertise is the expertise needed to make a practical contribution to the subject under study. Interactional expertise is the expertise required to talk fluently about it.

Using Symmetry Asymmetrically

So far, it has been "discovered" that a necessary move from accepting actors' categories to rejecting actors' categories is always made in the standard analysis of scientific controversies under SSK and that this move has, as far as can be seen, never been analyzed, warranted, or even remarked upon in SSK (my apologies to those who have made remarks that I have overlooked).[5] Now I raise my gaze from the way individual scientific controversies have been analyzed under SSK to broader patterns of analysis in our analytic community. What has the SSK analysis of scientific controversy been used for?

It seems to me that the SSK analysis of scientific controversy has been most widely used to "deconstruct" scientific authority. Trevor Pinch and I used it this very way in the widely read first volume of *The Golem* series (Collins & Pinch, 1993/1998). There we wrote about "levelling the scientific terrain" and analytically conquering the forbidding peaks of scientific authority such as "Mount Newton" and "Mount Einstein" (p. 141). All this was to be accomplished by showing that the logic of science was not so far removed from the logic of everyday life. In other words, we were weakening scientific authority by imposing the analysts' world on that of the actors.[6] Our typical move was to take a scientific episode that appeared to have been closed by the overwhelming weight of theory and experiment, open it up again, and show that, insofar as it was ever closed, it was closed by "nonscientific" means. The license imparted by this kind of analysis for contemporary policy issues is to show that controversies declared closed by "the scientific authorities" are still open. The viewpoint of those with dissenting voices is reexamined and shown not to have been defeated according to the standards of science. A protoexample from chapter 2 of the first of *The Golem* series is the falsification of the widely accepted notion, enshrined with authority in most physics textbooks, that the Michelson-Morley experiment of 1887 showed the speed of light to be a constant. This is incorrect. In fact it took about 40 years for it to become widely established that the speed of light was a constant. As late as the 1930s papers were being published and prizes awarded for work showing that it was not a constant. If Trevor and I had been around in, say, 1920 and had encountered scientists arguing that Einstein must be right because the speed of light had been shown experimentally to be a constant, we would have been able to reply: "No, it has not—there is still a controversy about that." If some scientist had said to us: "That's not a real controversy, just a few mavericks who refuse to accept Einstein in the face of all the evidence," we would

[5] The move is essentially the same thing as the kind of imperialism that many anthropologists try to avoid. As I understand it, evaluation of actors' worlds is considered incompatible with analysis of the actors' worlds (though the anthropologist can, of course, express an opinion in his or her time off as it were). Peter Meusburger points out that a similar debate has gone much further in the study of religion.

[6] Cleverly using our rhetorical nous to describe our project as merely "display[ing] science with as little reflection on scientific method as we can muster" (Collins & Pinch, 1993/1998, p. 2)

have said: "You don't understand your own world." There are also more recent cases in which this kind of logic has been put to use:

1a Scientists working for the plant-breeding industry say that genetically modified crops are safe to plant, but the analyst says that, no, there is still a scientific controversy going on about that.
1b According to the British government, scientists say that Bovine Spongiform Encephalopathy (BSE) cannot be transmitted to humans, but the analyst says that, no, there is still a scientific controversy going on about that.
1c The British government says that scientists have shown the combined mumps, measles, and rubella (MMR) vaccine to be safe, but the analyst says that, no, there is still a controversy going on about whether the MMR vaccine causes autism in some children.

So far so good, but a warning alarm is sounded by the existence of another set of arguments:

2a The U.S. government says that scientists cannot agree about whether global warming is a real threat. The analyst says that, yes, they can and that those people who say it is not a real threat are a small minority who should be ignored and that they are serving the interests of the government.
2b The tobacco industry says that scientists cannot agree about whether tobacco causes lung cancer. The analyst says that, yes, they can and that people who disagree are a small minority who should be ignored and that they are serving the interests of the industry.
2c The motor industry says that scientists are unable to agree over whether lead in the atmosphere caused by exhaust emission from cars lowers the IQ of children. The analyst says that, yes, they can and that people who say it does not are a small minority serving the interests of the motor industry.

The two types of argument are set out in Table 4.1.

Unlike the move toward disagreeing with the actors' categories at a certain point, which is consistent with saying that each controversy studied was settled by *nonscientific* means, this argument sometimes goes one way and sometimes another. Only sometimes does the analyst overrule the actor's categories and say the controversy was not closed "scientifically." At other times the analyst says that, scientifically speaking, the controversy is closed. As with the other type of case, there is no explicit justification for the way the relationship between actor and analyst goes, but this time it is more worrying. If an argument sometimes goes one way and sometimes another, without an external justification, it can be self-serving. It could

Table 4.1 Two types of argument used by social analysts when looking at controversies

TYPE	Government/industry claim	Social analysts' claim	Social analysts' conclusion
1	Consensus over P	Significant disagreement	No consensus over P
2	No consensus over P	Disagreement insignificant	Consensus over P

be that analysts decide in advance whose side they are on and then choose the direction of the argument according to the way they want it to come out. My impression as a participant in the broad field of science and technology studies (STS)[7] over recent years is that there is some self-serving in the way the argumentative strategy is chosen. If my impression is correct, STS is changing from a discipline concerned with the nature of knowledge to a social movement concerned with defense of the powerless and support for green issues, with the epistemology being plugged in each time in whichever way gets the political job done best.

My impression as a participant could be backed up by a survey of the content of recent presentations at conferences and of recently published papers. I suspect that such a survey would reveal that the large majority of such papers and presentations argued in favor of environmental issues and the like, the relationship between analyst and scientific actors sometimes going one way, sometimes another, depending on the analyst's preferred political stance. It is a case where Max Weber's entreaty to confirm adequacy at the level of meaning with causal adequacy in explanations would be useful. Unfortunately, I do not have the data to hand or the means to collect it, but we can do a little more analytical work before we finish.

The analysis seems to show another consequence of a shift from a concern with scientific knowledge to a concern with policy.[8] The additional consequence is that policy concerns and social-epistemological concerns have a different logic when it comes to the analysis of scientific controversies. To do scientific knowledge work, one always reopens scientific debate; to do policy work, sometimes one reopens what people take to be closed, and sometimes one closes what people take to be open. That is a consequence that we should embrace. But how might we embrace it while avoiding the charge of being *post hoc* and self-serving?

It is often useful to start with an extreme case and work back to less clear-cut and more difficult examples. Let us begin, then, with "green-ink letters." Scientists (and here I can include myself), often receive letters from those who believe they have found a fundamental flaw in the theory of relativity or have developed some new all-inclusive theory of the universe. After I publish something in the science news journals, or after one of my books is reviewed in the scientific press, I often receive three or four such items, recognizable by certain characteristics. They are often rich in mathematical symbolism and, in the old days, when they came by post, they were mostly characterized by peculiar formatting. They might be written in green ink, or closely typed on both sides of the paper with no margins, or written on lined paper with no introduction or conclusion. These communications are what I call green-ink letters. Among them there may be one or two that really are of world-shattering importance, but for practical purposes one has to assume that they are not. Again, in practice there is insufficient time (even if one had the competence)

[7] STS is a much broader study of science, technology, and its relation to society in which sociology of scientific knowledge is subsumed.

[8] Collins and Evans (2002) try to put this shift on a systematic footing.

to track down the flaws in each case to the point where one could be certain that there was nothing in them.

I believe that someone who felt it interesting could take any one of these communications and apply the tools of SSK to reveal that the kind of process scientists use to reject green-ink letters is not scientifically pure or decisive. It would then be possible to resurrect the logic of any one of the claims, showing it to be not completely unworthy of consideration. This effort would be a perfectly proper and revealing exercise in SSK (though perhaps only suitable as a training exercise nowadays since we know in advance that it could be done and that therefore the outcome would not count as a discovery but merely a display of competence). The point is, however legitimate and valuable an exercise it would be in SSK, it would not be a proper and valuable exercise in science policy. Today's routine technical decisions cannot be made on the basis that relativity might be wrong and that all the money going into orthodox research based on relativity should be put on hold until the matter is resolved. This case is one where the policy analyst has to say that, even though some people want to say that the argument about relativity is still open, it is "really" closed. It is a Type 2 case, not a Type 1 case.[9]

Or consider the following imaginary example. I wake up one morning and decide that cancer is caused by drinking coffee. I point out the long-term correlation between the massive increase in coffee-drinking in my country and the increase in cancer as the recorded cause of death. Furthermore, there is a rough correlation at the level of whole societies between high consumption of coffee and expenditure on cancer therapies. I send out a press release, and the newspapers pick it up and run the story. Members of the public report a number of incidents in which someone was diagnosed with cancer a few months, or years, after they increased their consumption of coffee. After a short while, the existence of a connection between coffee-drinking and cancer becomes widely accepted. The relationship between coffee-drinking and cancer becomes part of the actors' perspective. Many coffee growers are bankrupted, and their laborers, deprived of wages, become weak and ill.

Does such a train of events constitute a scientific controversy? Once more, the sociologist of scientific knowledge could treat the matter symmetrically and use it to explore the ways in which one scientific idea gets promulgated and another does not. Such an investigation would show that there is no certain proof that coffee

[9] This, incidentally, is one of the problems for the position adopted by Brian Josephson as expressed at the Heidelberg conference that is the source of this volume. Josephson has discovered that the arguments deployed by his scientific colleagues to dismiss the likes of cold-fusion or homeopathy are not up to the standards of the canonical version of science. He correctly infers that there remains a small chance that there is something in them. What does not follow, however, is that the chance is large enough to make them worth pursuing. Josephson is right to fault the rhetoric in the dismissal of these maverick claims but wrong in drawing the conclusion that the associated controversies are not over for nearly all practical purposes. If it is true that absence of evidence is not evidence of absence, it is equally true that absence of disproof is not disproof of absence.

ingestion does not contribute to the onset of cancer. But, again, for policy purposes, this case cannot be treated as Type 1 but must be treated as Type 2. For policy purposes, there is no scientific controversy here. For policy purposes we have to say that this kind of thing is not a scientific controversy or anyone would be able to start a scientific controversy whenever they wished.[10]

How might one argue that these two cases are Type 2 rather than Type 1 given that it is known from the analysis of scientific knowledge that every controversy can be reopened? It is a hard problem. Perhaps one solution, admittedly not a very satisfactory one, is to look at origins. When it comes to policy, the charge "genetic fallacy" should no longer be treated as a decisively damning criticism. For policy purposes the origin of a controversy can play a part in the decision-making process. In the case of green-ink letters, it is precisely the origin that warns against taking their policy implications too seriously. In the case of coffee and cancer, it is again origins. "I wake up one morning and decide …" is the giveaway.

The invocation of origins can be used only in extreme cases, however.[11] The courts typically assess the credibility of expert witnesses by references to their origins, and, of course, as SSK has shown, scientists do this on a regular basis as a means of finding a resolution to the problem of "experimenter's regress" (see Collins, 1992, for example). It is not analysis of origins of this relatively subtle kind that I am putting forward as a possible policy choice. That subtle kind of discussion of credibility belongs *within* a scientific controversy. The decisions being looked at in this context are about whether a scientific controversy even exists. It is being suggested that a certain scientific credibility is required in order to provide a license for starting a scientific controversy. A certain amount of scientific work by a reasonably credible scientist has to be done before the analyst should say, "This is a scientific controversy." Consequently, the analyst can sometimes say, "This is not a scientific controversy" and press the case that not just anyone should be able to dream one up.

Of course, even a dreamed-up medical controversy, if it gets going, has to be dealt with. As every social scientist knows, to deal with such a thing one must start by understanding the actors' perspective. In this case it might well be discovered that the actors do believe there is a genuine scientific problem and will treat denials by the authorities as a cover-up intended to save, say, the coffee industry (in this case). People whose first reaction is to take the side of the powerless will side with the actors and plug in the social epistemology in the style of a Type 1 controversy. Such a response implies that there is a scientific justification for the abandonment

[10] Of course, they would need power with the media, but in this context I am dealing with the logic of the analysis of science, that is to say, the logic of how sociologists exercise power as analysts. Whether it is significant power is another matter. We must always write our books and papers on the assumption that they will have the same political impact as, say, Marx's *Capital*. It is worth noting that so little is known about the detailed causal structure of the medical world that there is ample scope for dreaming up medical controversies, and it seems to happen quite frequently.

[11] Thanks to Martin Weinel for pointing out the possible confusion discussed in this paragraph.

of coffee-drinking. It is one thing to understand the actors in order to subvert their actions and persuade them that they are partaking in a moral panic rather than a matter of serious concern; it is another thing to justify their actions on the grounds that the scientific controversy is "real."

Going back to the controversies summarized in Table 4.1, I find that it looks very much as though case 1c—the debate about the MMR vaccine—is rather like the imagined coffee-cancer controversy. The difference is that the person who "woke up one morning and decided that autism was caused by the MMR vaccine" was a medical doctor who had published results showing that the measles virus might be associated with autism. The doctor first announced the connection between autism and the combined MMR vaccine *per se* at a press conference. However, even he recommended that parents continue with the single-shot measles vaccine. There seems to be no scientific evidence, only anecdotal reports by parents, that the MMR vaccine *per se* was associated with autism. These observations are sociological, not scientific. One need know nothing of the biology of the gut, the nature of vaccines, the etiology of autism, or the methods of epidemiology to recognize that this case was not a "real" scientific controversy. An analysis of the origins of the controversy is good enough. Case 1c, then, should really be case 2d. In the absence of a full survey, this case does seem to illustrate the dangers inherent in the situation represented by Table 1. It does appear that the position adopted by some social analysts was self-serving, and it does suggest that social studies of science might be becoming a social movement rather than a discipline concerned with epistemology.

It is fitting that a contribution to a book emerging out of a workshop held in Heidelberg, the home of Max Weber, should be concerned with the tension surrounding the idea of understanding the actors' perspective. For decades I have described myself as an interpretative sociologist, never quite noticing the violence I was doing to actors' categories as an integral part of my analysis of how science "really works." But I think I now see that Weber was right and that interpretation alone is not enough. I have discovered the aforementioned violence in my own work. I have at one point suggested a survey as a useful supplement to the *verstehende* (interpretive) method. In this case it would be useful if adequacy at the level of meaning were topped up with a bit of causal adequacy. But most important, I have argued that in the case of policy analysis of the sciences, as opposed to knowledge analysis, a still more brutal choice has to be made between groups of actors. This choice cannot be avoided if sociology is to be practiced as the kind of science for which Weber argued and if social analysts of science are to avoid slipping into the politically appealing rhetoric that he warned against. The appeal simply to take the actors' perspective merely sidesteps this necessary choice.[12] The next task is to find a better way to separate scientific controversies into their two types—a way that does not refer to the political desirability of the outcome. I have suggested that an examination of the origin of a controversy is one such means, but this is just a start.

[12] The abdication of responsibility is still more clear in cases like that of AIDS treatment in South Africa (see Weinel, 2008).

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