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Paper 120: Counterfeit Scientific Controversies in Science Policy Contexts

Martin Weinel

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Counterfeit Scientific Controversies in Science Policy Contexts[1]

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Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the minds of the general public. It is also the means of establishing a controversy.
Tobacco Industry Executive (quoted in Michaels 2008: 11)

Abstract

Experts disagree for many reasons and it is generally accepted that there is no `rational' way to make them agree. As Michaels (2008) has demonstrated with regard to the activities of the tobacco industry, however, expert disagreement can be 'manufactured'. This suggests a distinction between 'genuine' and 'counterfeit scientific controversies.' I argue that it is necessary and possible to distinguish between these two forms of expert disagreement. It is important for policy-making to know which disagreements to take seriously. 'Counterfeit scientific controversies' can delay or impede policy-decisions that depend on scientific knowledge. One way for Science & Technology Studies to contribute to science policy-making is to develop a consistent and reliable way to demarcate 'genuine' from 'counterfeit scientific controversies'. This paper proposes four sociologically derived demarcation criteria.

[1] I'm indebted to Harry Collins and Rob Evans for their constant encouragement, for their comments and for their generous allocation of time for discussions. I also want to thank the following people and institutions for critical comments, suggestions and/or support: members of the KES group at the School of Social Sciences, the wonderful people in South Africa who took the time to talk to me about the AZT case, Howard Barrell, Tammy Boyce, Nicky Priaulx, Lin Ying-Lin, the Cardiff University School of Social Sciences, my family and my friends. For all mistakes, however, the responsibility lies entirely with me.

Introduction

A myth about science, which has been thoroughly dispelled over recent decades by Science and Technology Studies (STS), holds that 'experts can be expected to agree'. But as, for example, Collingridge and Reeve (1986: 16-19) emphasise, the 'reality of science' is that experts can indeed be expected to disagree (see also Martin and Bammer 1996; Goldman 2001). Furthermore, Collingridge and Reeve argue that expert disagreements will become even fiercer if the contested issue is of import to science policy making.[2]

The reasons for expert disagreements are manifold; they might be caused, for example, by differences in values and interests held by experts or by different methods to generate facts or they might come about as a result of diverse framings of issues (e.g. Jasanoff 1991; Lindblohm and Woodhouse 1993; Rushefsky 1982; Stirling 2008). It cannot be expected that disagreements between experts can be resolved rationally or in some structured and organised way.[3] Value commitments, for example, which might lead to disagreement, are in some cases held tacitly by actors, which means that they are not even aware that they are committed to particular values.

But as the quote above from the tobacco industry executive suggests, there is also another form of disagreement between experts. This kind of disagreement is specifically manufactured to create doubt and controversy and to replace relative certainty and consensus. The tobacco industry has mastered the art of 'manufacturing doubt', but so have other actors, most notably oil companies with respect to global warming and climate change (see Michaels 2008).

Thus, when scientific or technical claims become relevant for policy-making and science policy-makers are confronted with expert disagreement, they *should* first assess whether the degree of publicly visible disagreement about some scientific fact accurately represents the uncertainty in the expert community, or whether the controversy has been artificially created to serve other ends. Distinguishing between the two in science policy contexts is important: if there is real scientific controversy, then science policy makers have to be careful how the science enters a policy decision; if, however, the controversy is 'counterfeit' or 'manufactured' then it should be ignored.

The question is: can STS offer anything to policy makers as they struggle to make these choices? In other words, can STS help policy makers to make judgements about the authenticity of scientific controversies in 'real policy time'?[4] As with any demarcation

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[2] The term 'science policy' (or 'science in policy') is adopted from Harvey Brooks (1964: 76) and refers to "matters that are basically political or administrative but are significantly dependent upon technical factors – such as the nuclear test ban, disarmament policy, or the use of science in international relations".

[3] This seems to be a major reason for Collingridge's and Reeve's pessimism with regard to the possibility of science informing policy-making in a meaningful way. Even Lindblom and Woodhouse (1993: 22), who seem to be more sympathetic towards a 'rational' and science-informed policy-making process, have to admit that a purely analytic policy-making process is an impossibility, partly because it "cannot wholly resolve conflicts of value and interest". Lehrer (1977, see also Lehrer and Wagner 1981) claims that "rational empiricism" offers a rational way to decide which experts to believe. Expert opinion is regarded as empirical information and by "aggregating expertise" it should be possible to find out which experts form the dominant group; that is the group of experts whose opinions one can believe rationally. Lehrer's model is, however, not only based on various dubious assumptions, but it also seems to be completely impractical (see Goldman 2001 for a critique).

[4] The problem for science policy-makers to choose between conflicting expert opinions is specific instance of the problem that Goldman (2001: 90) calls "novice/2-expert problem". In his interesting discussion of this problem, Goldman (p. 93) suggests five sources of evidence that might help novices or lay persons to find out which of the disagreeing expert opinions to believe: (1) arguments presented by the contending

exercise there will be 'borderline cases' where the judgement is difficult or impossible. But there are other cases, further from the borderline, where the analyst can make a decision about a controversy being genuine or counterfeit even when some actors believe the opposite. Another problem with this kind of criteria-based demarcation exercise is that the criteria, just like any other rules, do not contain the rules for their application. There is a tendency in STS to assume that, because rules do not contain the rules for their own application and because there are always exceptions, formulating rules is a deeply flawed activity. But everyone follows rules all the time, which is why we can tell when mistakes are made, and all social institutions rely on rules. The challenge, therefore, is to help develop rules that go wrong less often and in less damaging ways.[5] Progress will have been made if the method of making such decisions is worked out for a few extreme cases even if it will not yet work for every case. In sum, this paper is looking for practical ways to do demarcation work in an explicit manner by identifying possible criteria, which can structure the demarcation judgement.
The paper is organised in the following way: in the next section, I will introduce an imagined scientific controversy to tease out possible sociological criteria that can be used to make credibility judgements. In the remainder of the paper, the criteria are tested through their application to case studies of controversies.

The case of kiwi fruits and lung cancer

The importance of being able to separate genuine from counterfeit controversies can be illustrated with a simple thought experiment. Imagine that I wake up one morning believing that eating Kiwi fruits causes lung cancer – I might say it is something to do with inhaling the fibres from the skin.[6] I send out a press release which is picked up by some newspapers and news programmes. Widely reported anecdotal evidence from Kiwi consumers confirms many cases where lung cancer has been diagnosed among heavy consumers of the fruit and concerned citizens form pressure groups to campaign for a ban. The claims are contested by scientific institutions and individual scientists with expertise on the causes of lung cancer but the controversy continues to rage.
Does this form of disagreement represent a genuine scientific controversy that should be taken seriously by policy makers? That is, should policy makers consider a ban on kiwi imports or at least consider appointing a resource intensive expert commission to investigate a possible causal link between the fruit and lung cancer on such a flimsy foundation? The answer must surely be 'No.' If the kiwi-lung cancer claims were to be taken seriously by

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experts to support their own views and critique their rivals' views; (2) agreement from additional putative experts on one side or other of the subject in question; (3) appraisals by 'meta-experts' of the experts' expertise; (4) evidence of the experts' interests and biases vis-a-vis the question at issue; and (5) evidence of the experts' past 'track-record'. Goldman's assessment of the usefulness of these sources to lay persons is not gloomy but also not overly optimistic. Part of the problem is that Goldman seems to be too ambitious. In contrast, I limit the task for lay persons to assess whether a scientific controversy they are presented with is 'genuine' or 'counterfeit'. I do not claim that the criteria I'm going to propose can possibly tell a lay person which side of an argument to believe when they are confronted with a 'genuine scientific controversy'.

[5] I'm grateful to Rob Evans for pointing this out to me. The result of this exercise will be, at best, insecure. It is much easier to be certain about sceptical claims than positive claims (*e.g.* Nagel 1986). Positive claims such as 'this is a genuine scientific controversy' or 'this is a counterfeit scientific controversy', like any other scientific claim, may just turn out to be wrong.

[6] Collins (2008) tries a similar thought experiment involving a link between coffee and cancer. Unfortunately for Collins's example, there are many who believe this to be a genuine effect so his example does not work quite as he intends.

policy makers, it would imply that anyone could start a scientific controversy and, in some cases, vast amounts of public resources would have to be spent following them up.[7] Commonsense, then, suggests that this is the kind of controversy that policy-makers should not take seriously. It is, in the terms of this paper, a counterfeit scientific controversy. This, of course is an extreme case, but it clearly indicates there is a need to find criteria which can demarcate genuine from counterfeit scientific controversies.

First Criterion: conceptual continuity with science

In fact, the Kiwi fruit example suggests four separate sociological criteria for indicating that a scientific controversy might be a counterfeit. This first tries to establish whether the claim lies within the realm of science or not. If a claim, however controversial, is outside of science it cannot be part of a genuine scientific controversy. I call this criterion *conceptual continuity with science* and its absence is characterised by what Collins and Evans (2007:126) call a "lack of intention to make a body of work fit with the existing body of science".
Note that this formulation does not imply that the claim has to be accepted by mainstream science. In other words, it allows *Denkstil (*style-of-thought*)* changes in a Fleckian sense to occur (Fleck 1979 [1935]).[8] Thus, while a body of work might be 'revolutionary', it retains 'conceptual continuity' so long as the intention is to keep as much as possible of existing science in place making revolutionary changes only as necessary.[9] As long as this is the case, a body of work or a particular claim can be said to be part of science. But if a body of work or a claim is too far removed from science so that there appears to be no intent to make it part of science, it cannot be said to be part of a *scientific* controversy. Lack of conceptual continuity may have to do with content – let us imagine it has to do with magic – or method – let us say the claim is based on the discovery of some ancient manuscript or on divination — or both.
This criterion is not as fanciful as might appear. In September 2000 the media reported that the South African Minister of Health received a letter containing a book chapter written by William Cooper (*e.g.* MacGregor 2000). In the book chapter, Cooper explains that HIV was developed by a coalition of *Illuminati*, aliens, the Central Intelligence Agency (CIA) and a few other organisations. HIV was then introduced to Africa through small pox vaccines in the 1970s with the aim of reducing the world population. This account differs markedly from contemporary text-book accounts on the origins of HIV, which describe HIV as originating from a virus found in chimpanzees (*e.g.* Whiteside 2008). Apart from the Minister of Health, who sent copies of the book chapter to all provincial Departments of Health, no one in South Africa and probably not in the rest of the world – with the exception of a determined community of conspiracy theorists – took this piece seriously as a scientific contribution to origin of HIV. Neither *Illuminati* nor aliens nor any of the other leading characters of the conspiracy narrative play roles in scientific theories nor are they likely to do so in the

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[7] While the government, in the imagined case, might decide to ban kiwi fruit imports, it might also decide to spend taxpayers' money on research in order to clarify whether kiwi fruits are indeed carcinogenic. There is, however, an infinite number of imaginable risks and following them all up would be unaffordable for any national budget.

[8] Fleck's early ideas about styles of thoughts have been later popularised by Kuhn (1962) under the heading 'paradigm shifts'. I prefer to refer to Fleck rather than to Kuhn, since Fleck's ideas preceded those of Kuhn by about 30 years.

[9] Collins and Evans (2007: 128) put this in the following rule, which they call the "family resemblance rule": "Except were specific new findings demand a break, the intentional stance of a science must be to maintain continuity as far as possible with the existing science".

foreseeable future. In sum, this explanation is so far removed from accepted scientific explanations that it should not count as an element in any scientific controversy about the origins of HIV/AIDS in Africa.

Second Criterion: expertise of the originator

The second criterion refers to the originator of the controversy. In the thought experiment I started the scare. Here the demarcation is based on the expertise of the person making the claim. The suitability of a potential controversy originator can be discussed in the light of the Periodic Table of Expertises proposed by Collins and Evans (2007: 14). The table separates specialist expertise into the kind that can be gained using only ubiquitous tacit knowledge - such as natural language-speaking, which all of us need to live in society - and the kind that involves specialist tacit knowledge - which can only be acquired through socialisation in the company of experts. In this case, the criteria can be operationalised by examining the social networks in which the person making the claims is embedded. To the extent that these include networks of relevant experts, then there are grounds for believing the person making the claim knows what she or he is talking about. If, however, there is no evidence of such interaction, then it is hard to see why the person should be trusted as an expert (*e.g.* Goldman 2001; Hardwig 1985). But, as the case of the Cumbrian sheep farmers described by Wynne (1989) suggests specialist expertise can also be acquired through long-lasting practice outside of networks of formal experts.

In the kiwi example the application of the rule is clear: I started the imagined controversy without any specialist expertise at all and I did not seek out the company of relevant experts before making my claim public. As such, there is no reason to say that I have special experience beyond that of any other fruit eater that would turn me into an experience-based expert. This is a sociological criterion leading to a policy conclusion in respect of a scientific issue. In contrast, a researcher with a sustained track record in lung cancer research or a person who knew many people who worked in the kiwi industry and has seen many of them die of lung cancer would be a more credible originator of the controversy.

Third Criterion: constitutive work

Distance from relevant expert networks also underpins the third criterion, namely that I have not done any research or similar work that would put me into a legitimate position to make the claim that kiwi consumption causes lung cancer. Since I just announced the causal link after 'waking up one morning' I have done nothing that qualifies as what I will call 'constitutive work.'

The term 'constitutive work' draws on the idea of a 'constitutive forum', a concept that has been developed by Collins and Pinch (1979). Collins and Pinch (1979: 239-240) define the *constitutive forum* as an abstract 'space' which comprises "scientific theorising, and experiment … [with or without] corresponding publication and criticism in the learned journals and, perhaps, in the formal conference setting." The abstract space of the constitutive forum is bounded by what are generally considered to be the set of activities that constitute science as a distinctive form-of-life. This contrasts with the 'contingent forum' which is the abstract space of activities carried out by scientists which are not generally considered legitimate contributions to scientific knowledge building.

Sociologists of scientific knowledge have shown that there is no epistemological distinction between the two *fora* but the sociological distinction remains clear and must remain clear if we want to continue to demarcate science as a distinctive activity. If, in the course of my kiwi-related activity, I had done something that might look like an experiment or a theory-

based publication, or a systematic account of extensive and controlled observations, it would be more difficult to dismiss the kiwi case as a counterfeit controversy. It is important to bear in mind the constitutive work is operationalised in practice by evidence of systematic data collection, observation and reflection. Whilst the traditional format is the peer reviewed paper, a broader definition of expertise allows that constitutive work can also be done by the scientifically unqualified outside of official scientific institutions.

Fourth Criterion: explicit argument

Finally, the fourth criterion examines whether the constitutive work is still the subject of an *explicit argument* within the relevant expert communities. Imagine for a moment that I was a real expert on lung cancer and had done some research on the link between kiwi consumption and cancer. Given that my claims run counter to established wisdom on the causes of lung cancer, would that represent a genuine scientific controversy? Whilst the intuitive answer appears to be 'yes', in practice, things are not so simple. On the one hand, it might be that the claim had been made previously in the peer reviewed literature but had been dismissed by later research. As such, although the claim exists in the constitutive forum, it is not a controversy any more. The criteria is thus operationalised by a wider overview of the literature, of the kind an expert familiar with the field would be able to provide or which might be provided within a systematic review.

A more subtle variant of the same idea is also possible in which ideas are published but never explicitly rejected because no-one in the relevant expert community took them seriously in the first place. In other words, they are marginal or maverick ideas that exist on the periphery of the expert debate but which are, to the majority of experts, simply irrelevant and are therefore implicitly rejected.[10]

In fact, under the initial thought experiment, the Kiwi fruit example is most likely to be rejected on the grounds of 'conceptual continuity' – the claim is just too outlandish to merit explicit argument. But the general point can be made here nonetheless. If an argument has been already resolved *via* explicit argument and the rejected claim in this argument has reached the stage of implicit rejection, it should be considered 'closed' in political contexts unless a new explicit argument – maybe caused by some new discoveries or re-interpretations of old evidence – has broken out.

To summarise, four sociological criteria can be extracted and systematised from the foregoing discussion and used to establish whether a scientific-looking argument should be classified as constituting a 'real' or a 'counterfeit scientific controversy'. The criteria are: *conceptual continuity*, *expertise*, *constitutive work*, and *explicit argument*.[11]

1. The first criterion enables the analyst to check whether a claim exhibits *conceptual continuity* with science.

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[10] Collins and Pinch (1979: 239) distinguish between "implicit and explicit rejection" of knowledge claims: "Implicit rejection operates when rival knowledge claims are ignored by orthodoxy, whilst explicit rejection is characterised by controversy where the objects of dispute are articulated by individual scientists or opposed groups of scientists." In accordance with the theoretical framework provided by Collins and Evans (2007) the word "scientists" should be replaced by the word "experts". Note also that the original "explicit rejection" is here substituted by "explicit argument" as a scientific controversy might entail more than outright rejection.

[11] That does not mean that, for example, my claim about the link between kiwi consumption and lung cancer is untrue or wrong. Future research might show that there is indeed some link. However, it has to be stressed that for contemporary policy purposes there simply is no explicit argument about the alleged link. Thus, policy-makers should not ban kiwi imports with reference to my claim.

2. The second criterion checks whether an actor has the appropriate *expertise* to make a particular claim or answer a propositional question.
3. The third criterion allows checking whether the claim is supported by some kind *constitutive work.*
4. The fourth checks whether a claim is still subject of an *explicit argument* or whether it has yet reached the stage of implicit rejection.

I make the strong claim that if a single criterion is not met then a controversy should be classified as 'counterfeit'. The four criteria can be read as a 'decision tree': the 'scientificness' of a controversy increases from stage to stage, but only if a controversy satisfies all four criteria can it be judged to be a genuine scientific controversy.

Case studies: The South African AZT controversy, the MMR saga and an example from the 'Bar Wars'

I now show how these criteria work in the case of three examples, which correspond to the criteria 2 to 4, although they will be set out in reverse order.[12] Accordingly, I will start with an example that fails the fourth criterion. Inevitably, these examples have reached closure but the point is to show that the criteria could have worked if they had been applied when the controversies first broke; thus we look forward to possible future application of the same kind of analysis to controversies that are still ongoing.

- First, the controversy about the sensitivity of room-temperature bar detectors – devices to detect gravitational waves - is analysed.[13] While to outsiders it looked as if an explicit argument was going on in the early 1990s, claims that bar-detectors were sensitive enough to be able to detect gravitational waves had already been thoroughly rejected within the community of gravitational physicists.
- Second, the UK specific MMR vaccine debate will be looked at. Although the main protagonist of the campaign against the triple-jab MMR vaccine, Dr. Andrew Wakefield, might have had some expertise on vaccines and its effects on the human body, Wakefield failed to back up his claims with constitutive work.
- Third, the case of Thabo Mbeki's decision to prohibit the distribution of

[12] All controversies under investigation pass the first criterion; they are considered to have a conceptual overlap with science.

[13] This should not be confused with the original controversy about whether Weber had detected gravitational waves in the 1960s and 1970s.

AZT to reduce the risk of mother-to-child transmission of HIV (MTCT) in South Africa on scientific grounds will be analysed. It is classified as a counterfeit scientific controversy because Thabo Mbeki lacked the appropriate expertise to make technical claims about the toxicity of AZT and thereby start a scientific controversy.

Bar Wars[14]

The 'counterfeit controversy' associated with gravitational wave detection took place in the early 1990s when Joseph Weber, one of the pioneers of gravitational wave detection, started to contact members of the US American Congress to lobby against government funding for LIGO (Laser Interferometer Gravitational-wave Observatory). The consensus within the community of gravitational wave physicists was that laser interferometers, which would cost hundreds of millions of dollars of taxpayers' money, were the way to detect gravitational waves. It was also widely believed within the scientific community that older detection devices, either room-temperature bar detectors or cryogenic bar detectors, were simply not sensitive enough to be able to detect the waves. Weber's lobbying against the funding of laser interferometers challenged this firmly established consensus.

Weber claimed that there was no need to spend millions of dollars on interferometers, because room-temperature bar detectors – invented by Weber in the 1960s – were much more sensitive than previously thought and were, in fact, as sensitive the largest planned laser interferometers. Weber was able to back up his claims by pointing to several of his publications. In a paper published in *Foundations of Physics* in 1984, Weber presented new calculations that showed that the sensitivity of room-temperature bar detectors was much higher than previously believed (Weber 1984). Retrospectively the claims in the 1984 paper can be read as a prediction that room-temperature bar detectors would be sensitive enough to pick up gravitational waves emitted by a supernova. As it happens, a supernova, SN1987A, was observed in 1987 and worldwide only two detectors were running at the time and both were room-temperature bar detectors: one was in Weber's laboratory in Maryland and the other one was located in Italy. After comparing their data, the two research groups found 12 coincidences between their detectors and the emissions from the supernova. While a paper reporting these findings was rejected by the esteemed *Physical Review*, it was subsequently published in an Italian journal, *Il Nuevo Cimento* (Aglietta *et al*. 1989).

In 1992, therefore, as Weber tried to convince members of Congress not to fund interferometers, he could point to at least two papers that backed up his claims about the sensitivity of the bar detectors – one making a prediction and the other confirming the prediction. Mainstream gravitational physicists who were not sympathetic to Weber's lobbying of Congress, on the other hand, could point to a refutation of Weber's claims written by Leonid Grischuk and published in *Physical Review D* in 1992.

While it might appear that there was a genuine scientific controversy in 1992 with regard to Weber's claims, this was not the case. Applying the criteria proposed in this paper, the dispute between Weber and Grischuk should be classified as a counterfeit scientific controversy. While Weber was undoubtedly an expert on matters to do with bar detectors

14 This section is based on "Gravity's Shadow: The Search for Gravitational Waves" by Harry Collins (2004).

and while he had done new calculations and observations and even published them in peer reviewed journals, the explicit argument about his claims actually took place in the 1980s. By 1992 Weber's claims had reached the stage of implicit rejection amongst his peers. The explicit argument between Weber and the rest of the scientific community was played out orally during conferences and did not leave traces in print. This was because almost everyone working in the field believed that Weber was wrong. Oral refutation of Weber was considered to be enough since his claims had no practical consequences throughout the 1980s and early 1990s. But once Weber started to lobby against the funding of laser interferometers consequences such as non-funding of the interferometer project became a real possibility and so he had to be taken seriously. Grischuk's 1992 publication has to be seen in this context. While ostensively refuting Weber's claims, it was not actually directed at the scientific community since everybody knew that Weber was wrong. Rather, the real purpose of Grischuk's publication was to minimise the chances that Weber would succeed in his attempt to persuade members of Congress that funding laser interferometers was a waste of money. By pointing to Grischuk's paper supporters of LIGO could say that Weber had no case and that LIGO was the only option for gravitational wave detection.[15] Since Weber's arguments had already been rejected during the 1980s, his claims with respect to the sensitivity of bar detectors fail at the fourth criterion and should not be mistaken as part of a genuine scientific controversy.

The UK MMR vaccine controversy

Another case of a counterfeit scientific controversy is the debate about a causal link between measles, mumps and rubella triple vaccine (MMR) and autism - a controversy that occurred almost exclusively in the UK. By 1998 the MMR vaccine has been approved and used in over 90 countries, among them the United Kingdom, where it had been introduced in 1988 (Boyce 2007: 2).
In 1998, Dr. Andrew Wakefield, an adult gastroenterologist at London's Royal Free Hospital, reached a large public audience when he claimed that the use of MMR is risky since it might possibly cause autism in children.[16] In addition, Wakefield suggested that three separate doses of vaccine would be safer than the combined MMR vaccine. He made these claims during a press conference intended to publicise research done by a team of scientists at London's Royal Free Hospital and during a video news release authorised by the Hospital (Boyce 2007:4). Despite acknowledging that no causal link has been proven – a statement that has been explicitly made in the published research – and that his co-researchers are likely to disagree with him, Wakefield raised doubts about the safety of the MMR vaccine on three separate occasions during the interview for the video news release (Boyce 2007: 4-5).
While parts of the British media took the alleged MMR-autism link seriously, scientists and scientific institutions disputed Wakefield's claims. The latter believed that there was simply no evidence to back up Wakefield's allegations. This raises the question of whether or not the UK government did the right thing when it refused to change vaccination procedures as a result of this dispute.

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[15] Collins calls Grischuk's paper a 'political marker': "A political marker we can define as a paper that may not be read carefully – except by those it criticizes – but that can serve a political purpose by being spoken about" (Collins 2004: 385).

[16] Public attention to the MMR related claims by Wakefield increased even more in 2002, but the focus is here on his original 1998 claims.

First, it might be debatable whether Wakefield can be regarded to be an expert in paediatric gastroenterology given that his formal qualification is in adult gastroenterology. While there is overlap between adult and paediatric gastroenterology, there are some significant differences. It is, however, fair to assume that Wakefield has at least developed interactional expertise on paediatric aspects, since the research published in *The Lancet* focussed on children and some of the co-authors are paediatric gastroenterologists. With regard to expertise, Wakefield can be regarded as to being in a position 'to know what he is talking about.' He thus passes the second criterion.

The research on which Wakefield apparently based his allegations and which was published in *The Lancet* did not, however, support the claims that he made during the video news release and during the press conference (Wakefield *et al*. 1998). The article in *The Lancet* proposed the possible discovery of a new syndrome which involved chronic enterocolitis and regressive developmental disorder, whereby autism was one expression of the latter. The main purpose of the article was to describe cases – 12 children were examined – and to start investigating possible causes of this supposedly new syndrome.

Since the parents of 8 out the 12 children reported an onset of behavioural changes in their children – which were mostly diagnosed as autism – within 2 weeks after the children were inoculated with MMR vaccine, the authors specifically focussed on the MMR vaccine as an "environmental trigger" (Wakefield *et al*.1998: 637). In the discussion section of the paper, however, the authors clearly state that no causal link between MMR vaccine and the proposed new syndrome has been proven:

> We did not prove an association between measles, mumps and rubella vaccine and the syndrome described. Virological studies are underway that may help to resolve his issue. (Wakefield *et al*. 1998: 641)

This statement in the peer reviewed paper exposes Wakefield's claims that MMR vaccine might be the cause of autism in children as mere speculation. While the MMR vaccine might indeed be causally implicated in the syndrome described by Wakefield *et al*. this statement is not supported by *constitutive work* done by the authors on the issue. The authors themselves, in the second sentence of the quote, admit that only further research might resolve the matter.[17]

In sum, while Wakefield might possess the relevant contributory, or at least interactional, expertise to make claims about effects of the MMR vaccine on bodies of children, the MMR – autism debate fails the sociological test of constitutive work and should therefore be considered a counterfeit scientific controversy. Had the *Lancet* paper demonstrated a link, then the situation might be different but, with no acceptable scientific evidence to back up his claims, Wakefield's allegations against the MMR vaccine remain mere speculation. The UK government was right not to take his claims seriously, because the Wakefield case fails at the third criterion.

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[17] Some people argue that Wakefield's speculations should have been taken seriously since this would be in line with the 'precautionary principle.' It is, however, not so clear which course of action represents the most 'cautious' approach. The British National Health Service (NHS), for example states that administering three separate jabs to protect children against measles, mumps and rubella increases the risk of "catching the diseases while they are waiting for full immunisation cover" (National Health Service 2004). It is therefore at least debatable whether the discontinuation of the triple MMR jab would have really represented a cautious approach.

The South African AZT controversy[18]

In 1994, the outcomes of clinical trial ACTG076 were published (Connor *et al.* 1999). The results showed that administering AZT (azidothymine) reduced the risk of mother-to-child transmission (MTCT) of HIV during pregnancy and childbirth by about 50%. In the following years, other trials confirmed the effectiveness of AZT in the prevention of mother-to-child transmission (PMTCT) (Dabis *et al.* 1999, Shaffer *et al.* 1999, Wiktor *et al.* 1999). The US American Food and Drug Agency (FDA) licensed AZT for PMTCT in 1994 and the drug has been approved for this use by regulatory bodies around the world, including the South African Medicines Control Council (MCC). As a result, AZT is used in numerous countries for PMTCT.

In South Africa, various institutions and actors, including the ANC-led government, have been considering how to make AZT widely available throughout the public health sector since the mid-1990s. Plans to introduce pilot-sites in every province were about to be implemented in late 1998 when the government decided shelve those plans on financial grounds. The government argued that scarce resources would be better spent on programmes that focus on education and information that would prevent people from becoming infected with HIV in the first place. One year later, however, President Thabo Mbeki said that the government could not provide AZT for PMTCT because the drug is far too toxic.[19] He made the following comment in this respect at the end of his inaugural speech to the National Council of Provinces, the second chamber of parliament on 28 October 1999:

> There … exists a large volume of scientific literature alleging that, among other things, the toxicity of this drug [AZT] is such that it is in fact a danger to health. These are matters of great concern to the Government as it would be irresponsible for us not to head [sic] the dire warnings which medical researchers have been making. I have therefore asked the Minister of Health, as a matter of urgency, to go into all these matters so that, to the extent that is possible, we ourselves, including our country's medical authorities, are certain of where the truth lies. To understand this matter better, I would urge the Honourable Members of the National Council to access the huge volume of literature on this matter available on the Internet, so that all of us can approach this issue from the same base of information. (Mbeki 1999)

The speech marked the beginning of a heated controversy in South Africa about the safety of AZT that lasted until 2003, when the government was forced by courts to introduce AZT for PMTCT. But was it a genuine scientific controversy?

Thabo Mbeki was not an appropriate originator of the controversy, since he lacked the necessary expertise to make technical claims about AZT. Mbeki never received any formal training in pharmacology, chemistry or bio-chemistry or any remotely relevant discipline to be considered a certified expert on AZT. But formal training is not the only route to acquire specialist expertise, be it interactional or contributory. Did Mbeki acquire expertise through informal means?

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[18] This chapter is based on my long-term research on issues around HIV/AIDS in South Africa and fieldwork done in the country in 2008. For ethical reasons, some interviewees have to remain anonymous.

[19] No attempt is made to explain as to why Thabo Mbeki questioned the safety of AZT. I consider this question to be beyond the reach of sociological investigation as a considerable 'forensic effort' would be needed to find out what was going on in Thabo Mbeki's head. Some researchers have, however, tried to tackle the question. For me, the best explanation to date is provided by James Myburgh (2007), who shows in detail how the AZT controversy was closely linked to what is known as the 'Virodene affair'. Other explanations are summarised in the works of Nattrass (2007), Coovadia and Coovadia (2008) and Whiteside (2008).

The most remarkable and telling statement is conveyed in the last sentence of the above quote, when Mbeki urges the Parliamentarians to read the "huge volume of scientific literature on this matter [AZT's toxicity] available on the Internet" for themselves. It is worth quoting the last sentence of his speech again:

> To understand this matter better, I would urge the Honourable Members of the National Council to access the huge volume of literature on this matter available on the Internet, so that all of us can approach this issue from the same base of information. (Mbeki 1999)

This clearly indicates that Mbeki believed that reading some literature was sufficient to understand scientific matters well enough to lead to sound policy judgements. It also indicates how Mbeki acquired his knowledge about AZT's alleged toxicity: he just read "literature… on the Internet". Initially, Mbeki's interest in AZT was aroused by draft manuscript that would later be published as *Debating AZT: Mbeki and the AIDS drug controversy* (*e.g.* Gevisser 2007: 729; Gumede 2005: 158; Sparks 2003: 286). It was written by Anthony Brink (2001), a lawyer who classifies himself as an 'autodidact expert' on AZT (personal communication). The book consists mainly of direct quotes from the scientific literature that deals with AZT toxicity on the one hand and polemic and at times sarcastic comments by Brink on the other. According to Brink, it exclusively concentrates on the risks of AZT; no attempt is made to present a balanced picture which would include mentioning benefits of AZT (personal communication). Available evidence suggests that Mbeki visited websites such as www.virusmyth.com, which disseminates material produced by a small and dedicated group of mavericks who are organised in a network called *The Group for the Scientific Reappraisal of the HIV-AIDS Hypothesis* (*e.g.* Gumede 2005; Sparks 2003). They are united in the belief that HIV is not implicated in the aetiology of AIDS and some of them believe that drugs like AZT are the real cause of AIDS. They had, however, no credibility within the scientific community of HIV/AIDS researchers since their hypotheses have long been explicitly rejected and in the late 1990s they had reached the stage of implicit rejection, *i.e.* they were completely ignored. There is evidence that Mbeki started to immerse himself in the wider scientific literature only after his speech in October 1999 (Myburgh 2007).[20] Having no formal expertise in pharmacology or other relevant scientific fields and having only read a very limited amount of literature another possible route for Mbeki to acquire the appropriate specialist expertise on AZT would have been to immerse himself into the community of relevant experts. A South African scientist told me that Mbeki consulted a small group of scientists from the Medical University of South Africa (Medunsa) two or three months before making the speech, but the contact was brief and he only sustained contact with those scientists of the group who supported the claim that AZT is too toxic to be used. The low intensity and short duration of these contacts, however, would not have enabled Mbeki to develop an appropriate level of interactional expertise.

Mbeki's information on AZT's alleged toxicity stem almost exclusively from reading literature. Without meaningful expert consultation, it seems clear that Mbeki could not draw on *specialist tacit knowledge* when deciding that the literature he read was important enough to start some kind of review process of AZT. The missing specialist tacit knowledge makes Mbeki an unsuitable initiator of or contributor to a scientific controversy about the safety of AZT. Reading literature in isolation leaves Mbeki with a type of expertise that is at best what

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[20] Despite intensive research, I was able to find only one paper, published in a peer reviewed journal, which clearly states that the risks of AZT outweigh its benefits and that the drug should therefore not be used in any case. This paper was only published in November 1999 (Papadopoulos-Eleopoulos *et al*. 1999). None of the scientific papers cited by Brink to make a case against the use of AZT for PMTCT concludes that the risks of AZT outweigh its benefits. Brink, however, seems to believe that the authors of these papers do not understand the results of their experiments fully (see for example his discussion of Kuhn *et al*. (2000) in Brink (2001: 40)).

has been referred to as *Primary Source Knowledge* (PSK) (see Collins and Evans 2007; Weinel 2007). PSK is based on reading scientific primary sources, such as journal articles, research reports and so on. Crucially, PSK does not involve any interaction with experts in a domain who possess specialist tacit knowledge. The lack of specialist tacit knowledge makes it difficult, if not entirely impossible, to make informed judgements about the state of knowledge in any particular field because it is impossible to judge the value of the published papers and there are many published papers which are of no real value. The specific difficulty lies with the poverty of the insights gained from reading written sources in isolation (Weinel 2007).[21]

Unfortunately, Mbeki's position in the South African society and his political power made it impossible to ignore his claims about AZT. It is unlikely that the President would have refrained from the course of action he took in 1999 had this paper already been published then. But this does not affect the sociological analysis. The point is that this analysis, using the proposed criteria, concludes that the AZT controversy was a counterfeit scientific controversy, because it fails at the second criterion.

Conclusion

The main aim of this paper has been to start thinking about ways that STS can contribute to policy making. One way to do this is to find criteria that might help science policy-makers to demarcate between genuine and counterfeit scientific controversies when they are confronted with expert disagreement. With reference to the imagined kiwi-lung cancer example the logical possibility of making a distinction between the two types of controversies has been established.

I have gone on to show that it is possible to draw on sociological expertise on scientific controversies to find criteria to guide the analyst in the demarcation process. Four criteria for distinguishing between genuine and counterfeit scientific controversies have been proposed and tested: *conceptual continuity*, *expertise*, *constitutive work* and *implicit rejection.* All the examples used have been 'extreme cases' and have been analysed retrospectively. There is, however, nothing in the nature of the criteria that would prevent them from being applied to ongoing controversies and, all being well, future work will turn to the examination of contemporary cases.

The possible contribution to policy making that follows from the establishment of the four sociological criteria is to reduce the effect of counterfeit scientific controversies on science policy making.[22] As it is the consequences counterfeit controversies can be grave as the

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[21] Another problem lies in the apparent misunderstanding of how science works and of how credibility is attributed in scientific communities. Published articles are part of the constitutive forum of a science. There is, however, also a contingent forum, which can only be accessed through direct interaction with other members of the scientific community of a domain. This latter forum tends to be very influential when it comes to the interpretation of published sources (Collins and Pinch 1979; for an example see Collins 2004: chapter 6**).**

[22] A further effect of the introduction of the four criteria is the reduction of the influence of political preferences held by the analyst on the analysis. Thus, there is no longer need to invoke "politico-moral" arguments in order to state that the tobacco lobby is inventing scientific controversies to further its profits – and these are obvious and easy cases since there seems to be a consensus in the STS community that these particular representatives of capitalism are "bad". But this kind of argument is much harder to extend when it comes to cases like MMR or the AZT controversy. Is Andrew Wakefield fighting against MMR to reduce the risk of autism or were his actions motivated by more pecuniary interests? Is Thabo Mbeki a post-colonial African hero, bravely taking on the might of Big Pharma or is he maybe just an isolated intellectual who got it completely wrong with respect to AZT's toxicity? By sticking to the proposed sociological

example of MMR and AZT show – in the case of MMR it is almost certain that the incidence of measles in the UK has increased as a result of declining vaccination rates and that some of these cases will have been fatal. By delaying policy decisions on the provision of AZT to reduce the risk of MTCT with reference to the alleged ongoing scientific controversy about AZT's safety for more than 3 years it is very likely that tens of thousand of babies have been unnecessarily infected with HIV.[23] Without any chance of getting long-term treatment to slow down the progression to AIDS, survival rates for infected children in South Africa (and elsewhere) are virtually zero after only a few years.

While the intervention in the science policy process proposed in this paper can be described as an exercise in 'policing', such policing is restricted to technical aspects of controversies. Collins and Evans (2002, 2007) introduce a distinction between what they call a 'political' and a 'technical phase' (see Evans and Plows 2007: 833-835 for a more detailed explication). In these terms, the proposed criteria would only impact on the technical phase which deals with narrowly framed propositional questions, for example: Does smoking cause lung cancer? Or does AZT meet certain safety benchmarks and is it therefore safe to be used for particular purposes in a public health sector? The political phase deals with the much wider question of preferences, for example: Do we allow people to make their own decisions when it comes to smoking or do we want to live in a completely smoke-free society? Do we want to spend tax-payers money on drug-based HIV/AIDS prevention programmes or do we want to spend it on something else? All that is argued here is that it should not be possible for politicians like Thabo Mbeki, scientists like Andrew Wakefield and Joseph Weber or capitalist enterprises like the tobacco industry or oil companies, to refer to counterfeit scientific controversies to influence policy making.

The example of the AZT controversy illustrates the point about the relationship between the technical and political phases with clarity. I show that it is possible to criticise Thabo Mbeki's decision not to provide AZT on the ground that he misread the technical literature on AZT's toxicity and apparently based his decision against AZT on this misreading. But with the analytic tools proposed in this paper it would not be possible to criticise Mbeki if, for example, he had said that he did not like the idea of being dependent on foreign pharmaceutical companies or that he preferred to spend tax payers' money elsewhere. These imagined decisions would be open to political debate – which is hopefully open, inclusive and fair. But the critical point is that if the illegitimate framing of the debate as scientific controversy is not challenged then the political process is disempowered as political decisions are taken by default as a result of the apparently unfolding scientific investigation. It is only be recognising the controversy as a fake controversy – something STS with its expertise in the social analysis of science is uniquely well-placed to do – that the proper political process can be allowed to take place.[24]

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criteria, political preferences of the analyst are not completely kept out of the analysis – a point made by previous research in STS – but the effect of extrinsic interests can be reduced – which is all we can hope for.

[23] The rough calculation goes like this: the South African government estimated that in the late 1990s about 70,000 babies were infected through MTCT. It is assumed here that this number remains constant. At this point in time, no drug-based MTCT prevention programme was in place in the public sector. Such a programme, if the results of the drug trials are extrapolated, has the potential to reduce the transmission rate by about 50%. Given all this, over the course of three years 105,000 babies could have been saved from getting infected with HIV, had there been a countrywide PMTCT programme in place from 1999 onwards.

[24] In principle, the four criteria aid a process in technical appraisals that Andy Stirling (2008) has called 'closing down' – that is, they aim to limit debate. The four criteria exclude arguments from the technical phase that are considered to be inappropriate. However, whether subsequent deliberations in a technical phase are done in 'opening-up' or 'closing-down' mode is left open in the normative SEE approach, which is more concerned with actors and not so much with procedures.

Although the title of the paper entails a reference to another restriction with regard to 'policing' and is therefore implicitly acknowledged, it is worth to emphasise this point explicitly. The 'policing' is only directed at the interaction between science and science policy. It *should* therefore have no effect on purely scientific contexts. For example, while I classify Wakefield's speculation about the link between MMR and autism as a counterfeit scientific controversy in science policy contexts, I would say no such thing with regard to scientific contexts. Wakefield *et al*. (1998: 641) state in *The Lancet* paper that more research on the suspected link is needed to establish whether there is anything to it. I would support this, although it is ultimately down to funding institutions to decide whether this is worthwhile or not.

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