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Working Paper 145: Rejecting Knowledge Claims: A Case Study

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Rejecting knowledge claims: a case study

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Abstract

Citizens, policy-makers and scientists all face the problem of assessing maverick scientific claims. Via a case study I show the different resources available to experts and non-experts when they make these judgments and reflect upon what this means for technological decision-making in the public domain.

Keywords

Rejected knowledge, scientific controversy, science and technology policy, gravitational waves, arXiv

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[1] I thank `John Clapham' for sending his paper and the scientists who responded to the questionnaire, many of whom reacted to a first draft with very useful comments. Paul Ginsparg indicated where to look to find out more about arXiv and corrected some important mistakes in an earlier draft. Without the help of Martin Weinel there could have been no discussion of the bearing of the case on Mbeki decision on anti-retrovirals. Collins's continuing project, `To complete the sociological history of gravitational wave detection', is given open-ended support by US National Science Foundation grant PHY-0854812 to Syracuse University "Toward Detection of Gravitational Waves with Enhanced LIGO and Advanced LIGO", P.I.: Peter Saulson.

Rejecting knowledge claims: a case study

Dealing with fringe science

Citizens, policy-makers and scientists all face the problem of dealing with scientific knowledge claims which stand outside the consensus. The difficulty could be said to arise out of what Kuhn (e.g. 1959) called 'the essential tension'. The bulk of scientific activity is 'normal science', which is relatively stable, but science will stagnate unless there are occasional revolutions which involve radical change. Citizens, policy-makers and scientists are all presented with attacks on the scientific consensus any of which might be a scientific breakthrough or at least an important reassessment of the consensus. How are these maverick claims to be treated?

Collins and Evans (2002, 2007) argue that both policy makers and citizens should follow the lead of the expert community when it comes to making strictly scientific or technical assessments – judgements that fall within the 'technical phase' of a policy decision (Collins, Weinel and Evans, 2010).[2] This is an element of what Collins and Evans call 'The Third Wave' of science studies. To save misunderstanding, the Third Wave view is that a technological policy decision in the public domain may go against the consensus arising from within the technical phase if it *overrides* it with considerations drawn from the 'political phase' – this is not the same as rebutting a technical consensus. For example, in 1999 South African President, Thabo Mbeki, was confronted with public demands for the free provision of antiretroviral drugs to reduce the risk of mother-to-child transmission of HIV. Such treatment was being widely used in other countries.

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[2] Here the term `expert' includes experience-based experts such as the expert sheep-farmers discussed by Brian Wynne 1996 (and infelicitously referred to as '*lay* experts') and the farm-workers who were experts in the use of the chemical 245T and discussed by Irwin 1995.

Mbeki, however, decided not to distribute the drugs and made the following statement to his parliament:

> There … exists a large volume of scientific literature alleging that, among other things, the toxicity of this drug [the anti-retroviral AZT] is such that it is in fact a danger to health. … 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)

According to the Third Wave position he should not have made this statement. Should it be that Mbeki had political reasons for rejecting Western pharmaceuticals, such as their cost, or the danger that the South African State would fall under the thrall of Western companies, and should these considerations have been taken to override the scientific consensus over the safety and efficacy of anti-retrovirals, then, under the Third Wave model, that is what he should have said. But neither Mbeki nor his parliament was in a position to challenge the technical consensus. To disguise political judgements as scientific judgements disempowers the political process.

Whether or not one agrees with this Third Wave view it is still useful to understand the difference between the knowledge of experts when they make such assessments and the knowledge of non-experts. One of the reasons that non-experts might, in good faith, make technical judgements that differ from those of experts is because the scientific literature, including that version of it that can be readily found on the internet, gives the impression of being technically empowering. Scientific journals operate with a convincing literary technology– the impersonal passive voice is used to convey objectivity and the reader is given the impression that they are a 'virtual witness' of the experiments described (Shapin, 1984). The internet broadcasts the picture more widely. In the Mbeki instance, such material gave the impression that there was a serious technical controversy going on over the safety of anti-retrovirals. In this case, however, it was deeply misleading. In fact, by the end of the 1990s, when Mbeki made his speech,

there was virtually no disagreement to be found among the mainstream scientific community about the use anti-retrovirals as short term treatment for pregnant mothers and the only controversy there had ever been concerned long term treatment (Weinel, 2010; Chigwedere at al, 2008).

In terms of the Periodic Table of Expertises (Figure 1), in the anti-retroviral drugs case, contributory and interactional experts agreed there was little or no controversy about the safety of AZT when used to reduce the risk of mother to child transmission while those who had access to no more than the Primary Source Knowledge found on the internet could gain the impression that there was a lively controversy. These difference arise because understanding how seriously an apparent disagreement is treated within the expert community requires more than exposure to the literary technology, it requires access to the oral culture and the tacit knowledge of that community. [3]

This paper explores the difference between the oral culture, with its access to the tacit knowledge of the expert community, and more widely accessible written sources; it looks at the difference these two kinds of resource make to the assessment of maverick claims. Via a case study, more details are provided of the way members of the expert community judge maverick claims and some of the content of their tacit knowledge is described. It will be shown why, in the third line of the Periodic Table of Expertises, there is a gulf between the categories to the left and the categories to the right of the division between Primary Source Knowledge and Interactional Expertise and why this division is so important.

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[3] To believe that one can learn the state of science from published papers alone is the view that aligns with what Collins and Evans (2002, 2007) call Wave 1 of science studies.

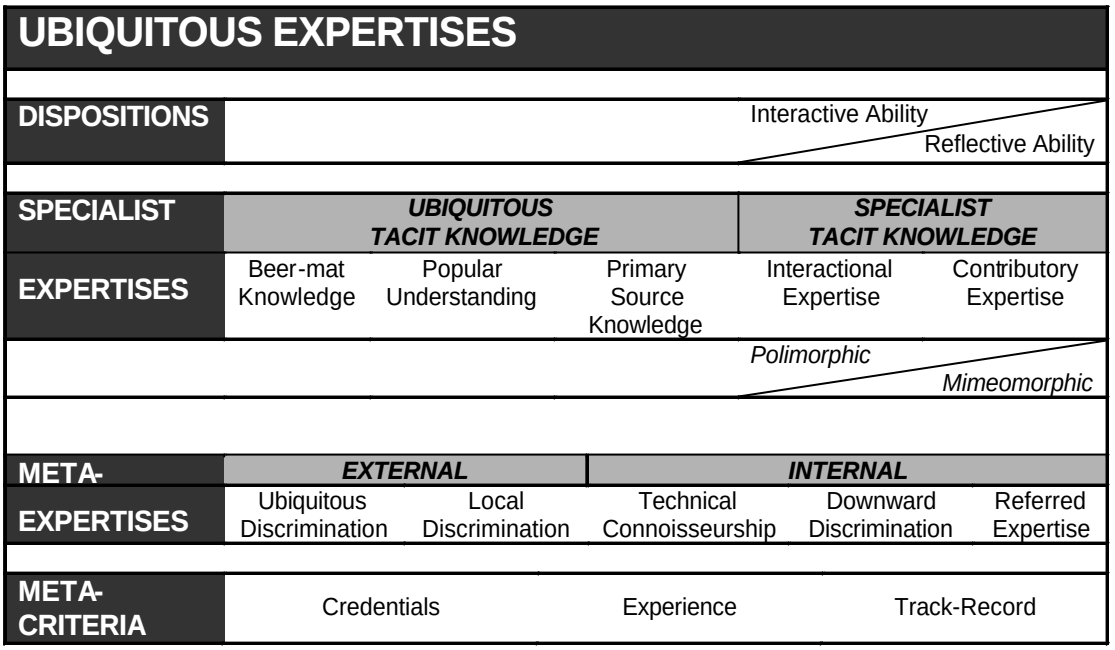

Figure 1: The Periodic Table of Expertises (Collins and Evans 2007, p 14)

Scientists' rejection of maverick knowledge claims

There has been little systematic work on how experts reject what they consider to be maverick claims. We don't know how many of such claims there are though the electronic preprint server arXiv, which began with an open access philosophy, has had to introduce more and more special measures to reduce the impact of the maverick material (see below); we don't have a full picture of who creates the maverick work and we don't have any systematic information about how maverick claims are treated by different groups of scientists. We do, however, have anecdotes and experience. For example, we know that some maverick claims are easy to dismiss out-of-hand because they indicate a high degree of eccentricity: such are the personal letters with unusual typographical conventions received by many high-profile scientists– so called 'green-ink letters'.[4] We

[4] The author of this paper receives them from time-to-time.

know that at the other end of the spectrum, and much more challenging, there are heterodox papers published by scientists working in universities which, in terms of technical content and style are not readily distinguishable from the mainstream scientific literature.[5] And, of course, we know that the historical archive contains cases, some famous, where strange claims were initially dismissed and later accepted while the sociology of scientific knowledge (and Wave 2 of science studies as a whole – Collins and Evans, 2002, 2007) has shown why it is so hard to refute a serious knowledge claim with absolute certainty or to assess its value with complete confidence.

To give examples from the field of gravitational wave physics, of which Collins has made a special study, that international, billion-dollar, detection programme arose out of Joseph Weber's 'impossible' and eventually rejected claims to have seen gravitational waves with a relatively cheap and simple apparatus (Collins, 1975, 2004). It is, however, only under unusual circumstances that even a well-crafted maverick claim be given extended examination. To extinguish every possible doubt about the state of the world would be a task like the `Trials of Tantalus' (Collins, 1999; 2004 p 312).[6] In fact, Weber, because of his established reputation, managed to get some recognition for unorthodox results on two further occasions though he failed on the fourth occasion (see below). On the two successful occasions he elicited formal refutations to certain of his papers which would otherwise have been ignored. The first was when his claims about the sensitivity of

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[5] I am grateful to Luis Galindo for referring me to Baez (1998), Siegel (2010) and 't Hooft (2010) who, in schoolboy humour manner, attempt to characterise scientific cranks or quacks. Langmuir (1953) is a more serious effort.

[6] One scientist wrote to me as follows in response to the survey (see below): `I receive a *lot* of good papers every day that I would read if I only had the time. I'm serious when I say a lot; I keep a folder on my desktop called "arXiv new" with papers I've downloaded but not gotten around to reading. There are currently about 800 papers in that folder. So, I have to be judicious in my choices of what to read, concentrating on what is really vital. Very good papers that are slightly outside my direct day-to-day work don't get looked at. So, papers that look incorrect certainly don't make the cut.'

detectors put at risk the funding of the nascent Laser Interferometer Gravitational-Wave Observatory (Collins, 2004, pp 380-386). The second was when neutrino scientists were willing to give Weber's novel ideas to do with the enhanced cross-section of his detectors a 'run for their money' before rejecting them (Collins, 2004 pp 334-336).

The second case is important: ideas can be given a run for their money but then count as rejected even if they are not completely extinguished. It follows from the Duhem-Quine model of science and/or from the experimenter's regress (and the whole of Wave 2 of science studies), that a scientist can always find grounds for refusing to accept a rejection. That is one reason why it could seem that there was an argument going on over anti-retroviral drugs as late as the 1990s. As far as the mainstream was concerned any such debate as there had been had 'passed its sell-by date' but a small group of outsiders could still find grounds for cleaving to the rejected claims; their papers could no longer find an outlet in the mainstream journals but they were still finding an outlet in fringe journals (Weinel, 2010). To the outside world, seeing the debate though the prism of the internet, it would not be obvious that the mavericks were doing anything out of the ordinary in respect of their science.

The Case Study

Toward the end of 2010 a physicist, who I will refer to as 'John Clapham' sent me a paper he had recently published in the journal *Progress in Physics*. Clapham explained that my understanding of gravitational wave physics (my, 2011a, *Gravity's Ghost*, had just been published), along with that of the rest of the gravitational wave detection community was incorrect. His paper argued that LIGO, and other interferometric gravitational wave detectors, cannot work because their light path is a vacuum and the effects sought would be seen only if the medium was a dielectric. If Clapham was right, the international billion-dollar, effort to make a direct detection of gravitational waves using evacuated interferometers had been doomed to fail from the outset.

Clapham wrote from the physics department of an established university, his paper was published in a physics journal, and, as far as I could judge, it had all the hallmarks of serious technical accomplishment in physics, with the usual equations and so forth. My view was backed up by a working physicist from the gravitational wave field who said of it:

> It's professionally done. ... The text is pretty good, the equations are mostly explained and the figures are clear. This man knows how to write a scientific paper.

I guessed, nevertheless, that in spite of its professional appearance, the reaction of the gravitational wave community to Clapham's paper would be broadly similar to their reaction to a paper published in 1996 by Joseph Weber (the fourth case), which was *not* given a run for its money but was simply ignored[7] The paper by John Clapham offered the opportunity to explore in more detail the way in which heterodox published work is rejected by the expert community.

I therefore emailed a questionnaire to a dozen scientists working in the field of gravitational wave detection, attaching a copy of the Clapham paper. After a few days and minimal prompting I received 10 responses. Five of the ten responses came from very senior scientists working in the field who were now holding, or once held, prominent institutional positions in the area; three were from leading theorists in the field; and two were from more junior but nevertheless very well established analysts.[8] Box 1 shows the important elements of the email:

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[7] For the reception of the 1996 Weber paper (which was co-authored with B. Radak), see Collins, 2004, pps 366-68.

[8] In a field tightly held together by a common language there is no need to construct samples with great care – nothing is being averaged but, rather, a common view is being tapped. To put this another way, every member of the community is a representative of a

Could you please take a little while to glance at the paper I attach to this mailing and answer my questions. The paper is by [John Clapham], in the journal, *Progress in Physics* It argues that it is impossible for the current generation of interferometric devices to detect gravitational waves because the interferometer light travels in a vacuum and there can be no effect unless the medium is a dielectric. I am not interested in the validity of this argument I am interested only in what you do when you come across papers like this. … I can't explain much more without prejudicing your answers but please don't try to `second guess' what I am going to do with the results … Please do everything you can to treat the paper just as you would if it had been sent to you by some physicist colleague as part of everyday email chit-chat and you were responding to him or her. …

Q1) Had you heard of the journal *Progress in Physics* before I sent this email?
Q2) If `yes', please tell me what you know about it.
Q3) Had you heard of the author, [John Clapham], before I sent this email?
Q4) If `yes', please tell me what you know about him
Q5) Had you heard about the paper before I sent it?
Q6) If `yes' please tell me what you knew about it
Q7) Had you heard of any related papers by [John Clapham] that claim LIGO cannot detect GW?
Q8) If `yes' what was your view of them?
Q9) Please give me your immediate view of the paper using the following questions as a guide:
(a) Now that you have a sense of what is in the paper, are you going to study it further?
(b) If you are not going to study it further, could you explain why?
(c) Do you have a technical reason to think it is flawed and if so, can you indicate what it is?
(d) If you are going to spend more time on it, how long do you think that might be?
(e) Are you going to ask anyone else's opinion of the paper?
(f) If `yes' – who, or what sort of person, is it likely to be?
(g) Any other comment on the paper?

Box 1: Important elements of email sent to respondents in respect of paper by 'John Clapham'

The distribution of answers to questions asking for a yes/no response is shown in Table 1:

collectivity that shares a practice language (Collins, 2011b). The common and expected `timbre' of the responses was, indeed, clear after the first couple of replies. With a larger number of responses, however, it is possible to recognise any eccentric replies, to gain a sense of the extent to which the journal/author/paper is known, and to collect richer descriptive detail.

Reponses out of 10	Yes	No
1. Heard of the journal	2	8
3. Heard of the author[1]	1	9
5. Heard of the paper	0	10
7. Heard of any other related papers by this author	1	9
9a Going to study the paper further[1]	0	10
9e Will consult anyone else for an opinion[1]	2	8

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It is seems almost certain from these responses that had I not intervened the paper would, have been ignored like the 1996 Weber paper. Indeed, we can get a sense that this is so in that Clapham had already promulgated a number of papers the arguments of which were related to the analysis in this one, and these were largely unknown to the gravitational wave community.

Discursive responses to the questionnaire

The journal: The two respondents who said they *had* heard of the journal, answered the open-ended question as follows:

(i) it traditio nally contains material that can't get past (or even to) the arXiv stage of publication. It has either been rejected by peer review or expects to be

(ii) it's a few years (five or ten) old. Publishes papers that can't make it through conventional refereeing and review.

`arXiv' is the electronic manuscript server which is almost universally used in some areas of physics, including this one, to promulgate findings prior to peer review and

publication. Though its initial policy was to allow recognized researchers to post their work, it now uses moderators [http://arxiv.org/help/moderation] to reject certain classes of paper and to direct others to the special category of `general physics'.[9] General physics appears to be analogous to the "anything goes" General Physics poster session at the American Physical Society meetings, which involves no or minimal refereeing. The arXiv general physics section is widely recognised, including by those who find their work directed to it, as counting less than other physics categories.[10] John Clapham has, in fact, more than 50 posted submissions to arXiv, but the most recent 40 (since May 2002) are all in the general physics category and, as can be seen, my respondents had not read them.

As I subsequently ascertained from the journal's website, *Progress in Physics* is, indeed, no ordinary journal; it has a special concern with rejection by orthodoxy.[11] It was founded in 2005 and the first volume of 2006 has an `Open Letter from the Editor-in-Chief' stressing that it is the work of individual scientists that advances science and that they often work in the face of fierce organizational constraints. An extract bearing on publication is reproduced in Box 2:

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[9] A recent innovation is that potential arXiv authors must have to have a 'sponsor' from among those already published in the section in which they want to publish. This has raised the barrier still further beyond open access.

[10] Thus a scientist complains http://archivefreedom.org/freedom/Cyberia.html [accessed 24 Dec 2010]: `when I tried submitting my most recent paper … to the hep-th (high energy physics theory) category [of arXiv] my paper was removed and displaced to the general physics category (the bottom of the pile in readership and audience).' The same post goes on to complain that there is no cross-listing from there to other categories.

[11] One or two of the respondents also reported the results of a web search on the journal or the author.

Declaration of Academic Freedom (Scientific Human Rights)
[From] **Article 8: Freedom to publish scientific results**
A deplorable censorship of scientific papers has now become the standard practice of the editorial boards of major journals and electronic archives, and their bands of alleged expert referees. The referees are for the most part protected by anonymity so that an author cannot verify their alleged expertise. Papers are now routinely rejected if the author disagrees with or contradicts preferred theory and the mainstream orthodoxy. Many papers are now rejected automatically by virtue of the appearance in the author list of a particular scientist who has not found favour with the editors, the referees, or other expert censors, without any regard whatsoever for the contents of the paper. There is a blacklisting of dissenting scientists and this list is communicated between participating editorial boards.

Box 2: Extract from Progress in Physics editor's Open Letter

The author: Only one respondent was certain that they had heard of John Clapham and that respondent said that he knew of him as:

> one of a cohort of 'special relativity is wrong' people.

Another respondent Googled the author and found:

> what is there at first blush seems legitimate. [His University] … as far as I know it, is of acceptable quality. I couldn't find his credentials, e.g., his education or degrees. However none of his publications in the last decade have been in any journal that I have much confidence in.

Other respondents too remarked that in cases like this they check out the author on the web and this case they were unable to discover sufficient in the way of publication outside fringe journals to persuade them to spend more time on the work.

Reasons for not spending more time on the paper: It is responses to the questions about why scientists would not spend more time on the paper and whether they had technical reasons for rejecting its findings that provide the richest insights into the way physicists think about these things. One robust response was `the markers of "crankness" are all over this paper.' On prompting for these markers they turned out to

refer to things that were mentioned by other respondents too. Thus a total of four respondents refer to the large number of self-citations in the articles and the high number of citations to a limited number of papers in the same journal, or other relatively unknown journals, with comparatively few citations to the wider literature. One respondent skimmed the journal's website and found that most issues of the journal contained papers by the same small number of authors and that the editors of the journal were very often included among the referees. Another found that Clapham's papers seemed to appear only in this journal or another journal with an anti-establishment credo – *Apeiron*.[12] One respondent summed up this objection graphically: `this is clearly a paper from planet [Clapham], only lightly coupled to the rest of reality.'

Another repeated theme was 'sell-by-date'. Fundamental criticisms of relativity are of very long standing. One respondent said: `I receive many communications claiming to disprove relativity or improve on it' [ie, so many that I ignore them all]. Another wrote: `it is the author's challenge to the speed of light which led me to trash it'; another: `Clapham chooses to ignore compelling evidence based on many experiments that [have] shown that c is isotropic'; another `he does not understand the foundational principles of relativity'; another said that there is `very selective quotation of Michelson-Morley experiments'; another that this paper is `jumping straight into crank territory;' and a couple of others pointed out that the effects, if genuine, would have shown up on many other kinds of experiment that have been done over the years. One senior theorist wrote: `I completely ignore articles like the one you sent and have done so since the mid-1990s.' The `since the mid-1990s' is the key phrase: the senior theorist quoted above had, in fact,

[12] There are two journals with this name; the one in question is an online physics journal.

given anti-relativity claims a very long run for their money – right up to the mid-1990s – while most scientists would have been ignoring them for decades.[13]

Discussion

There are several ways to read the outcome of this study depending on one's perspective. John Clapham and the editors and supporters of *Progress in Physics* will justly feel that they have seen once more what is to be, to quote from the editor's `open letter', `routinely rejected if the author disagrees with or contradicts preferred theory and the mainstream orthodoxy'. They can feel that they have, indeed, been rejected `without any regard whatsoever for the contents of the paper'. Those whose perspective is drawn from Wave 2 of science studies have seen it demonstrated, once more, that a scientific argument is much like any other argument so that the epistemological high point on which science once seemed to stand is, indeed, not much different to level ground. Those social scientists who believe the `critical disciplines' should always favour the underdog will have seen an example of the powerful suppressing the powerless.[14]

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[13] Only one respondent – another senior theorist – said he could dismiss the paper immediately on purely technical grounds. He said he had done calculations about the way interferometers interact with gravitational waves some years back and they did not agree with Clapham's conclusions so he did not need to check them. Of course, those calculations were based on premises that Clapham was challenging and, in any case, if a single calculation could always settle an issue there would be no need for peer-review or any of the other communal aspects of science. From our wider perspective, we can interpret this response as indicating that nothing had seriously challenged the calculations over the years.

[14] Though in many such public controversies it is often hard to say who is powerful and who is not. Thus, Mbeki and his ministers were powerful while the pregnant mothers refused anti-retrovirals were powerless – and yet the Western drug companies trying to sell the drugs were powerful. In the case of the Mumps, Measles and Rubella vaccine revolt in the UK (Boyce, 2006, 2007), the government, the medical establishment and the epidemiologists were powerful, but so were the middle-class parents who led the anti-vaccination campaign and could pay for separate injections. The powerless were the bulk

As intimated at the outset, this paper is aligned with none of these perspectives but is aims to compare experts to non-experts when they are faced with heterodox claims. Here, science is discussed but no scientific judgments are made; the concern is with scientific and technological inputs to policy as might be made by experts and non-experts. They key point is that the tacit knowledge of the expert community is unavailable to non-experts.

First, just being a member of the community directs one to certain literatures and away from others. No physicist has time to read everything that could be construed as physics, not even all the papers that pertain `directly' to their specialty; as the physicist quoted in footnote 6 remarks: `I have to be judicious in my choices of what to read, concentrating on what is really vital'. Those choices are going to be different for each physicist but they are likely to be heavily constrained by the way reading habits are developed during the course of socialisation into the profession – certain groups of journals and authors will be read and certain groups ignored. Expert reading habits are part of the `collective tacit knowledge' (Collins, 2010) of the domain. There will be other aspects that are too subtle to write down – something about the `flavour' of a paper that will provide a sense of whether the paper is to be read (or not read). One respondent did say that `the abstract is completely nutty in terms of style: "protesting too much"'. Table 2 lists these and other aspects of the meta-expertise. We can ask which of these indicators are accessible to groups outside of the expert community.

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of the population whose continued acceptance of MMR maintained a degree of herd immunity for the `free riders', and the children who were too sick to be given any kind of measles vaccine and were at grave risk from the measles epidemic that would be consequent on a successful revolt.

	Components of specialist meta-expertise used in judging paper
1	Attention directed one way rather than another by socialisation
2	Tacit aspects of style
3	Never heard of the journal
4	Never heard of the author
5	Never come across this article or similar by this author
6	Author has little record of scientific accomplishment
7	Journal and paper are incestuous in terms of author list and citation pattern
8	Typical cranky anti-relativity paper; anti-relativity is past its sell-by date

Table 2: Specialists' and lay persons' deployment of meta-expertise in judging a paper

The author of this paper, in view of his long-running immersion in the gravitational wave physics community stands at some point in-between expert and non-expert (he has some interactional expertise in the narrow field of gravitational wave physics but less in physics as a whole), so I am a useful 'litmus paper'. If I cannot make use of the indicators in Table 2 then they are unlikely to be accessible to others with less experience of physics. Working down the table, I could not use the subtle clues in the first two rows of the table for obvious reason but nor could I use the information in rows 3 to 5. I had never heard of the journal, the author, the paper, or similar papers but I do not know enough about what it would be normal to have heard of to feed this information into a judgment. What 'you ought to have heard of' is tacit knowledge ; without it what one has or has not heard of is not useful information. The same goes for row 6 – one must have tacit standards of accomplishment before one can know what a certain level of accomplishment means.

Row 7 is a little more complicated. It just might be that outsiders could develop sensitivity to the incestuousness of the citation pattern in both the paper and the journal – what has been referred to as the 'planet Clapham' aspect of the work. But, again, it is

hard to turn this into a formal rule. As one of my respondents pointed out, the self citation rate of this paper is not dissimilar to that of Clapham's.[15]

The final row of the table refers to 'sell by date', which is again a vague category though immediately recognisable by a member of the expert community and, as explained, the author of this paper was expert enough to recognise it and guess what it would mean for the Clapham paper's reception of the paper by the expert community. But that was all he had to go on; less expert readers of the paper would not have been able to make use of that indicator either.

An assiduous search by a lay person might uncover the anti-establishment credo expressed in the first issue of 2006 of *Progress in Physics*, or similar things in other journals, but it is not clear what these would be taken to mean. There is nothing wrong, *per se*, with being anti-establishment though, in this case, it is might be more significant when combined with the other indicators. It is however, interesting, that an extremely experienced physicist from another field who read an early draft of this paper wrote to me as follows:

[15] Unfortunately, the politics of academic polarisation means I have to be cautious with this little joke. I must explain, then, my own self-obsession and scholarly deficiencies aside, the high number of self-citations results, firstly, from the fact that it is turns on a group of physicists who I have been studying since 1972 and, secondly, because the initial paper setting out the general approach (Collins and Evans 2002), was, for several years, treated as heretical by the core of the science and technology studies community. Nowadays, however, the approach is well ` coupled to the rest of reality'. Thus, it has given rise to work in many other fields including criminology (Edwards and Sheptycki 2009), journalism studies (Boyce 2006), the study of agriculture (Carolan, 2006), psychology and neuroscience (Gorman 2008; Schilhab, 2007), marine conservation (Jenkins 2007), philosophy (Selinger, Dreyfus and Collins, 2007), political philosophy (Durant, 2010) and growing number of other disciplines including education and management. The work has also recently gained institutional recognition with an award of a €2.26m Advanced Grant from the European Research Council.

> I searched for [John Clapham] [on the internet] and see no obvious red flags among the top results, not only for a lay person but even from the standpoint of say a condensed matter experimentalist to adjudicate, much less a biologist or computer scientist (private communication, 25 December 2010).

Thus, it is hard to make judgments from the outside that are likely to correspond to those made from the inside of the expert community.

To repeat, none of this means that Clapham's physics is wrong – I am not a physicist and am not making a claim belonging to physics. Physicist respondents were also aware of the dangers and difficulties of making physics judgements in this way. One volunteered: `and yes, I know about the risk in missing an unknown Einstein' while another said, `In principle it is possible that [Clapham] has a grand insight into something that no one else does, but it seems unlikely to me'. Echoing the `Trials of Tantalus' point, however, this respondent continued, `and [it] would take an enormous effort to determine. If one has limited time, one has to pick and choose where to invest it.' It is certain that there will be occasions when a group of experts making judgements of the sort we have seen exercised here will turn out to have made the scientifically incorrect decision; this paper is not meant to adjudicate on such matters.

In the spirit of the Third Wave of science studies, this paper is, however, meant to indicate how one might decide on matters of *physics policy* (and, by extension, other areas of science and technology policy that fall into the public domain). We can imagine that if the parties discussed here were making policy choices from behind 'a veil of ignorance', even John Clapham would readily agree that the results found in his paper, given the judgments made by the respondents to the survey, should be accounted insufficient to change the direction of interferometric gravitational wave detection

research.[16] Such a policy judgment could be made even while Clapham's physics was felt to be credible by one or more parties, even while it had not been decisively disproved, and even if, in the long term, it should turn out to be right. The policy problem is resolvable even if the science problem is not.

To introduce a positive note, while it seems that the means to make the expert judgments that are described here can only be acquired through social contact with the expert community, it should be possible to inform non-experts about the criteria used by experts and thus put them in a better position to make more nuanced judgments of material they find on the internet and the like. For example, it might discourage non-experts from believing that symptoms of scientific controversy found on the internet necessarily indicate the existence of a live scientific controversy. None of this determines policy choices, it is merely a discussion of what *should feed into* policy choices.

Finally, the argument presented here is not opposed to the findings of classic studies such as that of Wynne, Irwin and Epstein (e.g. 1996, 1995 and 1996), that demonstrate the immensely valuable contribution that unqualified but experience-based experts can make to even the technical phase of a technological controversy. It is, however, opposed to referring to such people as 'lay-experts' – they are experience-based experts – and it is opposed to assuming such cases to be the 'default position'. The default position, in so far as the argument presented here is sound, is that the unqualified are rarely in a good position to make such technical contributions even though, based on what they can read in the journals or on the internet, it is easy for them to gain the impression that they are. What I have referred to as the 'classic cases' are, therefore, still more interesting and deserve further study so as to understand the special circumstances that do occasionally

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[16] The idea of the veil of ignorance is taken from Rawls (1971). In this case it would mean that Clapham, when asked to make a policy choice, would not know whether he was in Clapham's position or that of the mainstream community.

enable those that do not have access to the tacit knowledge of the expert community to make such technical contributions.

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