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Rejecting knowledge claims inside and outside science

Harry Collins
Cardiff School of Social Sciences, Cardiff University, UK

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 judgements and reflect upon what this means for technological decision-making in the public domain.

Keywords

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

Dealing with fringe science

Citizens, policy-makers and scientists all face the problem of dealing with scientific knowledge claims that 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. Citizens, policy-makers and scientists are all presented with attacks on the scientific consensus, any of which attacks might be a scientific breakthrough or at least an important reassessment of the consensus.

The approach of the sociology of scientific knowledge to the problem as faced by scientists is to point to the 'constructed' nature of the boundaries of scientific knowledge.[1] Here, I undertake another small case study of how mainstream scientists deal with a heterodox claim. My main focus, though, is on the public and other policy actors and how the material to which they have access in making such a claim differs from that available to the core-set of scientists.

Corresponding author:
Harry Collins, Cardiff School of Social Sciences, Cardiff University, Cardiff, CF10 3WT, UK.
Email: CollinsHM@Cardiff.ac.uk

The implicit judgement that underlies the paper, as argued elsewhere under the heading 'Third Wave of Science Studies', is that, in spite of the constructed nature of scientific knowledge, when it comes to the strictly technical aspects of a scientific claim, policy-makers and the public ought to take their lead from the core-set, along with other experience-based elites.[2] To save misunderstanding: the view put forward is that a technological policy decision in the public domain may go against the consensus arising from within the technical phase if it *overrides* the consensus with considerations drawn from the 'political phase', but this is not the same as rebutting a technical consensus.

Given this approach, it is important to understand the difference between the knowledge of experts when they make assessments of technical value and the knowledge of non-experts; the position being advanced here is that technical judgements are the prerogative of experts and it is only the consequences of those judgements that are the prerogative of non-experts. But it is easy for the two kinds of judgement to become mixed up, with non-experts believing that it is possible for them to make technical judgements. One of the reasons that non-experts might, in good faith, think they can make technical judgements that differ from those of experts is that 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 of being a 'virtual witness' of the experiments described (Shapin, 1984). The Internet broadcasts the picture more widely. But the sense of empowerment provided by being a virtual witness, or something similar, is misleading.

This article explores the difference between the specialist oral culture, with its access to the tacit knowledge of the expert community, and more widely accessible written sources; it looks at the difference that 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 judged and acted upon one maverick claim, 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 (Figure 1), 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 to this case and, by implication, to many others.

Scientists' rejection of maverick knowledge claims

There has been little systematic work on how experts reject what they consider to be maverick claims. We do not know how many of such claims there are, though the electronic preprint server arXiv, which began with the intention of allowing anyone who could format their paper in the appropriate technical form to publish, has had to introduce more and more special measures to reduce the impact of the maverick material (see below). Moreover, we do not have a full picture of who creates the maverick work and we do not have any systematic information about how maverick claims are treated by different groups of scientists. We do, however, have anecdotes and experience. For example, 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

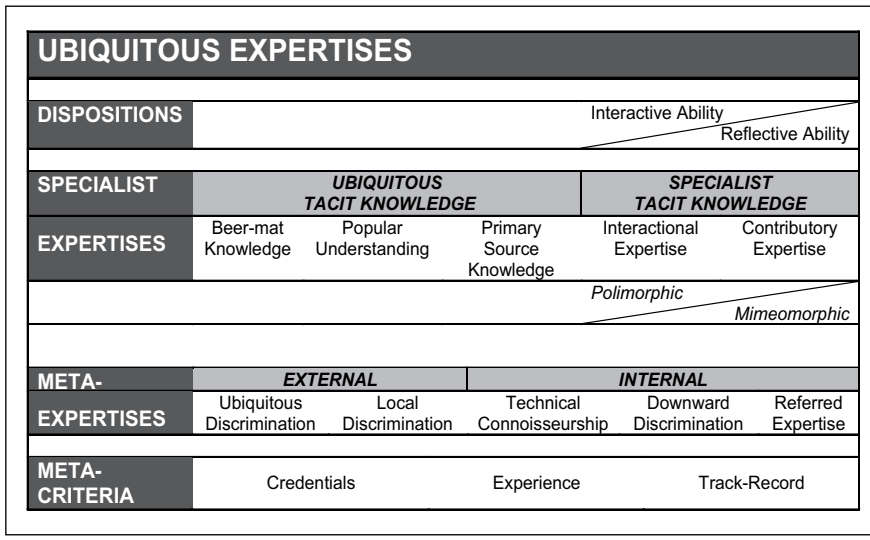

Figure 1. The Periodic Table of Expertises (from Collins and Evans, 2007: 14).

by many high-profile scientists, the so-called green-ink letters – even the author of this article receives them from time to time. At the other end of the spectrum, and much more challenging, there are heterodox published papers that, in terms of their authorship by university scientists, technical content and style are not readily distinguishable from the mainstream scientific literature.[3] And, of course, the historical archive contains cases, some famous, where strange claims were initially dismissed and later accepted; meanwhile, the sociology of scientific knowledge 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 the author has made a special study, an 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 is 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: 312). To illustrate the size of the problem, one scientist wrote to me as follows in response to my 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.

Weber's early 1970s' claims were taken seriously only after he made a series of innovations that increased their credibility. By 1975, however, he was once more finding it difficult, but his reputation ensured that he had a chance. In fact, he managed to get some recognition for unorthodox results on two further occasions, though he failed on a fourth occasion (see below). On the two successful occasions he elicited formal refutations to certain of his papers that would otherwise have been ignored. The first was when his claims about the sensitivity of detectors put at risk the funding of the nascent Laser Interferometer Gravitational-Wave Observatory (LIGO) (Collins, 2004: 380–386). The second was when neutrino scientists were willing to give Weber's novel ideas, about the enhanced cross-section of his detectors, a 'run for their money' before rejecting them (Collins, 2004: 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 thesis and/or from the experimenter's regress and similar analyses, that a scientist can always find grounds for refusing to accept a rejection. As far as the mainstream is concerned, there comes a point when controversies have 'passed their sell-by date', but groups of outsiders can still find grounds for cleaving to rejected claims; their papers can no longer find an outlet in the mainstream journals but they still find outlets 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

Towards the end of 2010, a physicist, whom 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 *Gravity's Ghost* (2011a) 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 such as air or water. If Clapham was right, the international, billion-dollar effort to make a direct detection of gravitational waves using evacuated interferometers had been doomed to failure 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.[4]

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.[5] The paper by Clapham, along with the trust engendered by my long-term engagement with the gravitational wave detection community, offered the rare opportunity to explore in more detail the way in which heterodox published work is rejected by a core-set.

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 10 responses came from very senior scientists working in the field who were now holding or had 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.[6] Box 1 shows the important elements of my email.

Box 1. Crux of email sent to respondents with respect to the paper by 'John Clapham'.

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 paper s 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?

Table 1. Tabulated responses to the emailed questionnaire.[a]

Responses out of 10	Yes	No
1. Heard of the journal	2	8
3. Heard of the author	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	0	10
9e Will consult anyone else for an opinion	2	8

[a]A couple of respondents said they could not remember whether they had heard of journal and author. Since I was looking for the salience of the items in their working lives, I included these answers in the 'hadn't heard of' category. One said he would study the paper 'if I have time'. I prompted this respondent and he said the likelihood of this happening was 'between 0.1% and 1%'. Another respondent said that they might study it but with 'low probability'. I counted these answers as 'will not consult anyone else'. Of the two remaining, one said they might pass it on (I did not prompt further) and one said they might pass the paper to their graduate students as an exercise.

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

It seems almost certain from these responses that had I not intervened, the paper would have been ignored. 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 that they *had* heard of the journal answered the open-ended question as follows:

1. It traditionally 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.
2. It's a few years (5 or 10) 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. Although its initial policy was to allow recognised researchers to freely post their work, it now uses moderators (http://arxiv.org/help/moderation) to reject certain classes of papers and to direct others to the special category of 'general physics'.[7] 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.[8] 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 web site, *Progress in Physics* is, indeed, no ordinary journal; it has a special concern with rejection by orthodoxy.[9] 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 organisational constraints. An extract bearing on publication is reproduced in Box 2.

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

> 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.

The author. Only one respondent was certain that he 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 remarked that in cases like this they check out the author on the web and in this case they were unable to discover sufficient publications outside fringe journals to persuade them to spend more time on the work.

Reasons for not spending more time on the paper. It is the 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

web site 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*.[10] 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' [i.e. 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 experiments 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, given anti-relativity claims a very long run for their money, while most scientists would have been ignoring them for decades.[11]

Discussion

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 journal 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 from 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.[12]

As intimated at the outset, this paper is neither for nor against any of these perspectives but is trying to do something different: it aims to compare experts to non-experts when they are faced with heterodox claims. Here, science is discussed but no scientific judgements are made; the concern is with scientific and technological inputs to policy as might be made by experts and non-experts. The key aim of this paper is to show that the tacit knowledge of the expert community is unavailable to non-experts and also to show what this means for the way judgements are made.

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 above 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

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

	Components of specialist meta-expertise used in judging papers
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

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.

The author of this article stands at some point between the expert and the non-expert (by having 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, so I will start by using myself as the judge of the accessibility of the relevant tacit knowledge.

Working down the table, I could not use the subtle clues in the first two rows of the table; to use these one must have been socialised into the community of specialists – one must be part of the oral community. 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 judgement; to know what it would be normal to have heard of one would, once more, have to be a member of the oral community. Knowledge of 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 and these standards are acquired through specialist socialisation. 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 the paper you are currently reading is not dissimilar to that of Clapham's.[13] 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, in consequence of his one-time interactional expertise in gravitational wave physics, was expert enough to recognise it and guess what it would mean for the Clapham paper's reception 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 text of 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 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:

> 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 judgements from the outside that likely correspond with 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 judgements about physics 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', however, this paper is 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 judgements made by the respondents to the survey, should be accounted insufficient to change the direction of interferometric gravitational wave detection research.[14] Such a policy judgement 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 judgements that are described here can only be acquired through social contact with the expert

oral 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 judgements 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 (e.g. 1996), Irwin (e.g. 1995) and Epstein (e.g. 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 in such a position.[15] 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 enable those who do not have access to the tacit knowledge of the expert community to make such technical contributions.

Acknowledgements

I thank 'John Clapham' for sending his paper and for reading and commenting on the draft, and also for sending me additional material and details of his career. I thank the scientists who responded to my questionnaire, many of whom reacted to a first draft of this article 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.

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Notes

1. In a series of studies, Collins and Pinch showed how paranormal findings are treated, later extending the analysis to other disputes (e.g. Collins and Pinch, 1979, 1993; Pinch and Collins, 1984). Gieryn (1983, 1999) produced a general description of science as boundary maintenance.
2. See Collins and Evans (2002, 2007) and Collins et al. (2010). 'Experience-based elites' include the sheep-farmers discussed by Brian Wynne (1996) and the farm-workers who were experts in the use of the chemical 245T and discussed by Irwin (1995).
3. 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.
4. After seeing my own article in draft, Clapham explained to me,

I have a BSc (Hon1) and PhD (Theoretical Physics, 1970) from the University of New South Wales (located in Sydney). I have in excess of 100 publications: early ones in low energy nuclear physics, then many in particle theory (quark – gluon physics), and now relativity theory and related experiments.

5. For the reception of the 1996 Weber paper (which was co-authored with B Radak), see Collins (2004: 366–368).
6. 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 'probed'. To put this another way, every member of the community is a representative of a 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 slightly 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 the richer descriptive detail needed to complete Table 2.
7. A recent innovation is that potential arXiv authors must have a 'sponsor' from among those already published in the section in which they want to publish. This has raised the barrier further beyond open posting.
8. Thus, a scientist complains, '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)' (from http://archivefreedom.org/freedom/Cyberia.html (accessed 24 December 2010)). The same post goes on to complain that there is no cross-listing from there to other categories.
9. One or two of the respondents also reported the results of a web search on the journal or the author.
10. There are two journals with this name; the one in question is an online physics journal.
11. 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.
12. However, in many such public controversies, it is often hard to say who is powerful and who is not. Thus, consider the decision by the South African President not to distribute anti-retroviral drugs to pregnant women. Mbeki and his ministers were powerful while the pregnant mothers who were 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 (MMR) vaccine revolt in the United Kingdom (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 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.
13. 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, first, from the fact that it turns on a group of physicists whom I have been studying since 1972 and, second, because the initial paper setting out the

general approach (Collins and Evans, 2002) was initially treated as somewhat heterodox. It has become 'well coupled to the rest of reality' only fairly recently. 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 et al., 2007), political philosophy (Durant, 2010) and a growing number of other disciplines including education and management. The work has also recently gained institutional recognition with an award of a €2.26 million Advanced Grant from the European Research Council, a Proof of Concept grant from the same source and grants from the British Academy and the UK Economic and Social Research Council (ESRC). Nevertheless, in spite of the fact that citations to the original publications are legion and growing, there is still little work in the mainstream of science and technology studies that bears on what is being argued here and therefore few outward-looking citations.

14. 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.
15. For an accessible analysis of the relationship between lay persons and experts, see Collins, 2014

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Author biography

Harry Collins is Distinguished Research Professor in the School of Social Sciences at Cardiff University and Director of the Centre for the Study of Knowledge, Expertise and Science (KES). He is a Fellow of the British Academy, winner of the JD Bernal Prize of the Society for Social Studies of Science and the Robert Merton book prize of the American Sociological Association. He has authored over 150 papers and 18 books in areas related to the nature of knowledge.