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## **Cultures of incomprehension?: the legacy of the Two Cultures debate at the end of the twentieth century.**

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### **Abstract**

This paper considers the impact of Snow's Two Culture's thesis on debates about the place of science and scientists in society in the latter part of the twentieth century. Debates concerned with the public understanding of science and the 'science wars', both of which relied to some extent on the dividing of society into 'two cultures', are contextualised within longer efforts by scientists to popularise definitions of science and society and their relationship with other epistemic communities. This paper argues that we should think about all these episodes as part of ongoing rhetorical boundary work, reflective of strains and stressors on science as an institution. The two cultures debate has provided one powerful rhetorical device, amongst many, for ongoing boundary work to establish or question science as the dominant form of knowledge in society and delineate who is allowed to speak for it, and wield its power.

### **Introduction**

In this paper I want to consider the impact of Snow's Two Culture's thesis on debates about the place of science and scientists in society in the latter part of the twentieth century. In particular the way in which the idea of 'two cultures', with science on one side and other forms of knowledge on the other, has provided a powerful rhetorical device for ongoing boundary work between science and other cultural territories. Snow's concern as a scientist and policy maker was on the gulf of understanding between those with a literary intellectual or humanities background and those

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trained to think scientifically. This paper will document how in the 1990s his rhetoric both continued to be mobilised by scientist and literary intellectuals, but also underpinned the surrounding UK context: debates concerned with the public understanding of science and skirmishes between social scientists (Snow's proposed bridging 'third culture' and scientists in what later became called the 'science wars').

### **Science and its epistemic and cultural boundaries**

The work of sociologist Thomas Gieryn (1983, 1995, 1999), argues that we should view debates and statements made by scientists about science as rhetorical boundary work. Boundary work, according to Gieryn, 'occurs as people contend for, legitimate, or challenge the cognitive authority of science – and the credibility, prestige, power, and material resources that attend such a privileged position' (p. 405). Demarcations of science from non-science are driven by a practical 'social interest in claiming, expanding, protecting, monopolising, usurping, denying, or restricting the cognitive authority of science' (Gieryn 1983, 405). Gieryn's early formulation of the concept of boundary work focused on the rhetorical style with which scientists (and non-scientists) describe science and its surrounding epistemic terrains for the public and its political authorities, often hoping to enlarge the material and symbolic resources of scientists or to defend professional autonomy. The two cultures debate between Snow and Leavis can, and should, be viewed in this manner. The truth or falsity of their claims are of less interest to us as are the claims being mobilised and the ways in which they attempt to construct and deconstruct the professional and cultural authority of each 'side'.

Further, ideas concerning popularisation and communication were central to Snow's concerns. Indeed, as media scholar van Dijck has argued, 'implied in Snow's argument of "bridging the gap" between arts and sciences was the dire need to translate between expert and lay communities.' (Van Dijck 2003, 181) Though all acts of popularisation are concerned with the transfer information from expert to non-expert, they are also in Gieryn's formulation also to do with identity

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construction of epistemic domains and professional identities in public sphere. Thus, as I have argued elsewhere, debates about the public understanding of science in the 1980s were not new so much as they repeated older institutional responses to concerns about the relationship between science and the public (Lock, 2009).

Indeed, Gieryn himself has argued that John Tyndall used his prominent position as Superintendent at the Royal Institution to popularise a variety of ideological arguments to justify scientists' requests for greater public support. Tyndall's Presidential address to the British Association in 1874, was boundary work: an attempt to deny the authority of religious beliefs over natural, scientifically derived ones, and by extension fighting for cultural authority and resources for science (Barton, 1987).

One can equally consider the founding of the British Association for the Advancement of Science (BAAS) in 1831 and find reformers in the Royal Society who believed the solution to greater professional identity and more importantly greater resources for science, was to establish an association 'for reviving science in England' (David Brewster to Charles Babbage, in Morell and Thackray 1981, 50). Thus a deficit of understanding or attitude was perceived and also implicitly a link between public feeling and support for science and government support was established.

Popularisation of science was seen as the way to manage the relationship, or boundary, between these two spheres: public and science.

Almost sixty years later, we find similar concerns about the lack of public recognition of science as the President of the BAAS, John Burdon-Sanderson, at its Annual Meeting in Nottingham in 1893, argued:

"It is not the fault of governments, but of the nation, that the claims of science are not recognised. We have against us an overwhelming majority of the community, not merely of the ignorant, but of those who regard themselves as educated, who value science only in so far as it can be turned into money ..."  
" (Burdon-Sanderson 1893)

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In the twentieth century we can find again similar sentiments being made by scientists in the 1930s (Collins 1981), and in the following decade the first use of the term 'public understanding' as the British Association's conference, 'Science and the Citizen: The Public Understanding of Science' in 1943, held at the Royal Institution, where J.G. Crowther argued that "a hundred years ago, it was desirable that the people should know about science: to-day it is necessary for survival". Calling for a "more professional and systematic" approach to the "explanation and appreciation of science", he argued that scientists could no longer leave this task to "occasional and amateur activity" if we were to "look forward to a scientific civilisation supported by democratic approval, understanding and participation" (Advancement of Science 1943). These are but a few of many similar examples whereby scientists' calls for greater popularisation of science and a greater appreciation and understanding of science, can be seen as attempts to carve out and expand a role for science as a way of knowing and as having a more prominent role in cultural and political life.

Similarly, Snow's argument about the gulf between scientists and literary intellectuals was not new in 1959. American literary critic Lionel Trilling, for example, has argued that the debate between Snow and Leavis in the mid-twentieth century should be seen as a 're-run' of the debate at end of nineteenth century between T. H. Huxley and Matthew Arnold (Trilling, 1967). Huxley had argued that "the great majority of educated Englishmen" believed that "the man who has learned Latin and Greek, however, little, is educated; while he who is versed in other branches of knowledge, however deeply, is a more or less respectable specialist, not admissible into the cultured caste" (Huxley 1880, 141-2). The two cultures debate therefore forms part of such wider and ongoing boundary work concerned with expanding the cultural authority of scientists.

## **Two Cultures revisited? The public understanding of science**

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It is probably no coincidence that at the same time as Snow's lecture in the UK the term 'scientific literacy' was coined in the United States. Similar contemporaneous arguments put forward by anthropologist Margaret Mead show that the 'Two Cultures' debate was not simply a British preoccupation. Where Snow had concerned himself largely with literary intellectuals in positions of, or who had influence over people with authority, Mead was concerned with what she perceived as a growing alienation of lay people from the worlds of science and technology. She suggested that a 'schismogenic process' was taking place in Western culture, which could only be stopped by the discovery of 'new educational and communication devices' that would be able to bridge the gulf between 'the specialized practitioners of a scientific or humane discipline and those who are laymen in each particular field' (Mead, 1959).

Improving scientific literacy - ensuring that American children were equipped to cope with a society of increasing scientific and technological sophistication - was viewed as one way of addressing such concerns. Interest in the concept was fuelled by concerns among the American science community over a lack of public support for science following the launch of the Soviet satellite Sputnik (Luagksch 2000). As Alan Waterman, Director of the National Science Foundation noted, in a review of the organisation in 1960, progress in science depended to a 'considerable extent on public understanding and support of a sustained programme of science education and research' (Waterman 1960).

Ortolano, has argued that the debate between Snow and Leavis, was far more than a "disciplinary dispute about the arts and sciences," and should be viewed as "an ideological conflict between competing views of Britain's past, present and future" (Ortolano 2009, p. 1). Labelling it a clash between the "technocratic liberalism" of Snow and a "radical liberalism" of Leavis, these ideas were picked up and shaped, British political rhetoric. As he documents, the most notable manifestation of this permeation into politics is perhaps Harold Wilson's first speech as Labour party

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leader in 1964, in which he alluded to the importance of public understanding of science in driving us toward a technological future:

“In all our plans for the future we are redefining, and we are restating, our socialism, in terms of the scientific revolution, but that revolution cannot become a reality unless we are prepared to make far reaching changes in economic and social attitudes which permeate our whole system of society.”<sup>1</sup>

Continuing into the 1970s a few academic programmes and courses were established to address the best means of achieving better scientific literacy and greater public understanding of science (Open University 1971, McCarty 1974, Perlman 1974, Rose 2003).

It wasn't, however, until the 1980's that concerns over the relationship between the public and science rose to prominence again within the UK scientific community. The Conservative Government at this time had decided it had little need of scientific advisors and without the 'special status' that had been accorded to prominent scientists in policy-making, particularly those in the Royal Society, their influence declined (Yearly 2000). The Conservative administration had turned away from academic science as a guiding influence on public spending, and believed instead that industry should know what they needed in the way of research and development, and should pay for it themselves. Members of the scientific community were faced with a lack of support from government, as well as declining public funds, falling numbers of students studying science, and a drain of talented scientists to the United States. The Royal Society sent regular delegations to meet with the Secretary for Science and Education, who were told that the Government could not afford to spend more on science (Bodmer 2004). The scientific community also perceived itself to be faced with a lack of support from the public who, scientists felt, at best showed indifference to science, and at worst were likely to question its uses and practitioners.<sup>2</sup> As John Ziman, a leading physicist, and Fellow of the Royal Society, later reflected there had been a concern within the science community that the efforts of those scientists over the past hundred years, who in his words 'had made it their business to explain in simple terms what the scientists were doing'

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(Ziman 1991, 99), had not had much effect. This perceived lack of public understanding of science by some in the scientific community was baffling to them, as he recalled:

It seemed a paradox that so many people should have so little understanding of the science that dominates their culture. They might find themselves quite unable to make sense of important practical questions affecting their lives – small questions, such as the goodness of eggs; large questions, such as the safety of nuclear power. In fearful ignorance, they might even take against science altogether, heedlessly throwing out the baby with the bath water.  
(ibid)

In 1985 the Royal Society published their report on the public understanding of science (PUS) (The Royal Society 1985). The report problematised the public's levels of knowledge of science, and gave rise to a variety of science communication activities which attempted to correct a perceived deficit of scientific knowledge on the part of the public. How much decision-makers and their advisers, and the public to whom they are ultimately responsible, understood the scientific and technological aspects of each issue, and more generally, the scope and limitations of the scientific method were seen as key. Thus the 'Bodmer' report argued that scientists had to come out of their laboratories and start communicating with the public directly. It has been argued that this was a break with the status quo of the previous decade or so, where, with the exception of a few 'visible scientists' most communication of science had been left to media professionals, and scientists were encouraged to focus on their research careers (Gregory and Miller, 1998, Lock, 2011). Goodell, for example, has suggested that a reluctance to engage in public communication had been instilled in many scientists as a tacit part of their socialisation into the profession (Goodell, 1977). The major outcomes of the Bodmer report was the formation of the Committee on the Public Understanding of Science (COPUS). Established in 1986 as a joint committee of the Royal Society, the Royal Institution and the British Association for the Advancement of Science, it was given the general aim of raising the profile and number of public understanding of science activities in the UK, particularly among scientists. Key activities included setting up a grants scheme to fund those scientists who wished to stage science communication

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activities, a Media Fellowship scheme, the establishment of the Faraday Award, awarded to a scientist or engineer deemed by COPUS to have contributed significantly to the public understanding of science and a National Science Week. Also central to the PUS movement in the 1980s was the fostering of high quality popularisations and one mechanism to facilitate this, also set up by the COPUS committee, was the science books prize. As Elizabeth Leane has documented in some detail, the UK experienced a boom in popular science writing in the early-1990s (Leane 2007). Stephen Hawking's book, *A Brief History of Time*, and others by Richard Feynman, Stephen Jay Gould, and Richard Dawkins were published and widely available. Leane documents how many articles in the late 1980s and early 1990s concerned with popular science and popular science books demonstrate that Snow's two cultures 'barrier' was still viewed as a primary obstacle to the popularisation and public understanding of science.

For example, in a 1986 article, "In Praise of Science Writers" biologist, Colin Tudge argued that anyone knowledgeable about science was subject to a form of prejudice he termed "scientism" (Tudge 1986). This prejudice was evident in the media bias towards the arts:

Thus in the heavy Sunday newspapers, which focus the proper concerns of educated people, you'll find pages and pages on the posthumous letters of Vita Sackville West, or the annotated laundry lists of D.H. Lawrence; but you'll find precious little science, past or present.... . Indeed it remains the case, just as in Snow's day, that to be interested in science is still somewhat infra dig. (ibid., 44)

In his concluding paragraph Tudge argued that any "second Renaissance" of science in society would require, among other things "a little less philistinism among the arts-trained moguls of "the meejah" (ibid., 48), a sentiment also felt by members of the COPUS committee and the impetus behind their establishment of a media fellowship scheme to try and improve journalists' understanding of science.



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Michael Kenward, a prior editor of *New Scientist*, put forward a similar argument two years later:

The disregard for books about science, not to be confused with scientific books, in newspapers and magazines is scandalous. The “literary pages” happily publish interminable reviews of the laundry lists of members of the Bloomsbury set: no massive biography (auto or otherwise) of lesser figures escapes attention, nor do the trivial confessions of media “personalities”. If you are a scientist, however, the literary supplements will not even consider your book unless you have a Nobel Prize or some new theory about the origins of life, the Universe and everything. (Kenward 1988)

Leane has suggested that for both of these commentators, the “two cultures’ divide was therefore current and manifested by the literary community’s disregard for popular science books. However, one can look further afield than the still relatively small domain of popular science books publishing and find scientists rehearsing or mobilising versions of Snow’s original arguments. The public sphere was thus a key battle ground for staking out and defending the authority of these cultural territories. Likewise, the importance of scientific thinking, who is allowed to speak for, and define, science and who should have a say on what it does were key concerns of the institutional public understanding of science ‘agenda’. Scientists felt therefore empowered, and were encouraged to speak out on these matters at this time.

The rhetoric was mobilised in both directions. In 1991, feminist author, Fay Weldon, penned a parody of a debate between a member of the public and a scientist in the *Daily Telegraph* which portrayed the scientist as in need of love and attention from the public and combating an inferiority complex when it came to artists. Her member of the public, in turn accuses science of failing to answer the important questions about life that the public really wanted answers to, leaving us with a cold, technologically advanced but immoral world. Referencing Snow, explicitly they state:

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I am glad that C P Snow, who back in the Fifties used to lament the Great Divide between the Arts and Sciences, is safely in his grave. It's getting really bad. I called a professor of English Literature ...I said 'tell me what you think of scientists.'

"They have leaky pens, though sometimes pharmaceutical companies give them plastic liners for their pockets. They wear anoraks in all weathers and the women wear cardigans. They have an eerie mortality...." (Weldon 1991)

Professor Lewis Wolpert, an embryologist at University College London responded the following week. As one of a few 'visible scientists' in the UK, Wolpert had been active in popular lecturing, writing, and television and radio broadcasting for many years. In his letter he explained that he had great difficulty understanding why Weldon and others like her, 'gifted in the arts, could be so misguided, even silly, when it comes to science' (Wolpert 1991). The tradition was, he argued however, a long one. He equally drew similarly sharp and hostile lines between science and literary intellectuals in his response to her depiction of scientists:

It is as discordant as image as mine of feminist novelists: woolly skirted but smart, publicly oriented, quick to take up the latest politically correct attitude, pious, overconfident on all matters, full of feeling, but low on thought. (On the other hand, my image seems to be not so inaccurate). (ibid)

At fault, to Wolpert, was the ways in which scientists were 'almost entirely excluded from anything that smacks of general culture, high or low'. As he continued:

whether in the literary pages of journals or on radio and television chat shows, scientists are not there, and to judge from the social and obituary pages of this and other newspapers, they rarely have birthdays or die.... It is you and your "friends" who dominate the media... What you do not seem to recognise is

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that you are part of a rolling anti-science bandwagon on which so many in the humanities jump.’ (ibid)

An address by Neil Cossons, Director of the Science Museum, published in *The Guardian* the following year, echoed some of Weldon’s characterisations of the public feelings towards science, focusing on what he perceived as negative cultural attitudes towards science and, one assumes, equating ‘fashionable people’ as being those without a science education, yet in jobs of high political influence:

Science is perceived as part of something bigger, more insidious, threatening, inaccessible and sinister ... fashionable people want little of science. And people who want to be fashionable people will distance themselves from science in case they become tainted. (Cossons 1992)

In a documentary for Channel 4 in 1996, Professor of Public Understanding of Science, Richard Dawkins, similarly expressed his worry over the lack of scientific understanding and toleration of such ignorance. To blame was what he called a “double standard in or society with respect to science.” As he continued:

Earlier this year I was on a late night television talk show and I mentioned the names of Watson and Crick and the chairman promptly stopped me and said “for the benefit of viewers who are Watson and Crick?”. Now if I’d said I’d just been to the Cezanne exhibition, she wouldn’t have dreamed of saying, “for the benefit of viewers who was Cezanne?”. And that double standard matters, not that we should value Cezanne less, but we should seek to value science more. (Dawkins 1996)

### **Two tribes go to war: the UK ‘science wars’**

The public understanding of science movement in the 1980s was as much to do with scientists’ controlling the public narrative, and definition of, science, in particular against what many saw as a rise of pseudoscientific knowledge in the public sphere in the early 1990s (Holton 1992). Concerns over the potentially subversive impact on

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the public of definitions of science that were not from the scientific community were not solely directed at new age practitioners, however, but were also at the core of the conflict between many scientists and social scientists during this period. Snow, in his 'Second Look' at the two cultures, had hoped that the rise of social sciences might develop into a 'third culture', which would help to bridge the gulf between natural sciences and literary culture. Yet the so called 'science wars' of the 1990s hinged around conflict between certain social science and humanities scholars and natural scientists. The rise of social constructionism and postmodernism within social sciences from the 1960s onwards had not gone unnoticed within the scientific community. Though more of a phenomenon in the United States, the 'science wars' brought the field of science studies (or science and technology studies in the UK) and related disciplines under scrutiny and gave it media attention. Here now, was a professional and accredited body of scholars with the explicit goal of understanding and defining science. As will be documented below, in the UK, many of those actors involved in public understanding of science efforts, both in an institutional, and academic, context clashed over differing definitions of science . It is easy to characterize these debates both in the UK and abroad as an esoteric set of debates about the inner workings of science, played out largely in an academic sphere. The stakes, however, were high: the legitimacy to define and promote a definition of science in public and subsequently impact on the public and government's understanding and attitudes towards it.

A few specific events can be identified as mobilising and sustaining the rather larger conflict in the US. First, certain scientists, such as Gerald Holton, Professor of Physics and History of Science at Harvard, had expressed concern at the Smithsonian Institution's 1994 exhibition *Science in American Life*, and branded the exhibition as anti-scientific, claiming it would engender anti-scientific attitudes in its visitors (Gieryn 1999). The exhibition, which showed science in its social and historical context, had included images of atomic destruction and chemical pollution. It was too negative for its financial backers, the American Chemical Society, and they disowned the exhibition's representation of science a year later, blaming post-

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modernist sentiments among some of the curators and members of the advisory board. Picking up on Gerald Holton had left off, mathematician Norman Levitt and Paul Gross, former director of the Woods Hole Marine Biology research station, published *Higher Superstition: the academic left and its quarrels with science* (Gross and Levitt 1994). In the book the authors defended science against what they perceived as the 'anti-scientific' attitudes proffered by what they called an 'academic left' of social constructivists, cultural theorists, feminists, multiculturalists and some extreme environmentalists. The book was followed by a conference hosted by the New York Academy of Sciences in the following year entitled 'The Flight from Science and Reason' which rallied many scientists together to decry these same academics whom they perceived to be taking up 'cudgels against science'. Later, in 1996, Alan Sokal, a professor of physics at New York University, submitted a paper of what he called 'outright nonsense' for publication in cultural studies journal *Social Text*, as an experiment to see if a journal in that field would, 'publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors' ideological preconceptions' (Sokal 1996). The ensuing debate centred on both the intellectual rigour (or lack thereof) of the social sciences, and the ethics of Sokal's attempt to defraud a journal.

In the UK the conflict in the 'science wars' was a smaller affair. One of the chief 'combatants' was no stranger to public debate in such issues, Lewis Wolpert, who had succeeded Walter Bodmer as Chair of the Committee on the Public Understanding of Science (COPUS) at the start of 1994. Crucially, he had also staked out his own claims as to the nature of science, a year earlier, in his book *The Unnatural Nature of Science* (1992), which included a chapter entitled 'Philosophical Doubts, or Relativism Rampant'. In this book, he had argued that science had a problematic relationship with society because it involved an 'unnatural' way of thinking that was in direct conflict with common sense. Therefore, as Wolpert explained in his book, 'the best and probably only way to understand science is to do scientific research' (p. 177). Lay-persons, he argued, lacked any familiarity with scientific thinking and science should therefore be left for scientists. The implications

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of this statement, as Durant outlined in an editorial in an issue of *Public Understanding of Science*, which contained several hostile reviews of Wolpert's book by social scientists and historians, was that the task of popularising science would be very difficult, if not 'downright impossible' (Durant 1993). This was, as discussed above, a period of expansion for popular science publishing and Wolpert's book was only one among many which brought scientists' definitions of science into the public domain many of which took up arms against the social studies of science. Steven Weinberg's book *Dreams of a Final Theory* (1992) had a chapter entitled 'Against Philosophy' in which he attacked constructivism. Dawkins, later also spoke against 'cultural relativism' in *River Out of Eden* (1995) and *Unweaving the Rainbow*, addressed what he called the "hostility from academics sophisticated in fashionable disciplines" (Dawkins 1998. p.20)

This mobilisation is in part down to the fact that scientists were not now the only scholars proposing definitions and explanations of science in the public sphere. Taking a very different stance to Wolpert's book, that same year sociologist Harry Collins, along with long-time colleague Trevor Pinch, published their book *The Golem: What everyone should know about science* (1992). The book had several main messages, the first of which was an attempt to correct a public image of science as something which produced certainty. This image, the authors felt, was promulgated by heroic histories of science and school science, neither of which portrayed the activity of social negotiation that surrounded scientific discovery. The inevitable product of this sort of model of science in public, the authors argued, was that science would be viewed as all good or all bad, which was dangerous and misleading:

The overweening claims to authority of many scientists and technologists are offensive and unjustified but the likely reaction, born of failed promises, might precipitate a still worse anti-scientific movement. Scientists should promise less; they might then be better able to keep their promises. Let us admire them as craft persons: the foremost experts in the ways of the natural world.  
(p. 142)

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Similar to many of the scientists within the public understanding of science movement, Collins and Pinch were concerned about anti-science sentiment, and they admired any attempts at disabusing the public about unsupported claims. Similarly, the public should still be educated, as citizens needed, they argued 'to know enough to come to some decision about whether they prefer more coal mines, or more nuclear power stations' (p. 144). However, they argued that it was ridiculous to expect the public to be educated with scientific facts, and then be able to make better sense out of science at the messy research frontier, where all sides had expertise 'way beyond what can ever be hoped of the person in the street' (p. 144). Thus, what was different in their formulation of PUS, as opposed to the more traditional conceptualisation of COPUS and its followers, was simply a matter of what sort of education was given to the public. Rather than the content of science, it was the relationship of experts to politicians, to the media, and to the rest of us in which the public needed to be better educated. As they argued:

To change the public understanding of the political role of science and technology is the most important part of our book and that is why most of our chapters have revealed the inner workings of science. (p. 145)

The book was controversial to many outside of science and technology studies, indeed *Nature* called it 'perverse but entertaining' (Gratzer 1993: 22), and Lewis Wolpert argued that it presented a 'distorted and highly biased image of science' (Hendry et al 1994). Yet a similar fashion to his American counterparts, Wolpert appeared to have found science studies threatening enough to the public reception of science to devote time and effort to what Gieryn labels, 'protection boundary work'; by trying to deny the authority or legitimacy of science and technology studies' academics in proposing their own definitions of science. The fact that Collins and Pinch had presented *The Golem* as a contribution to the public understanding of science (indeed the book's subtitle was *What Everyone Should Know About Science*) also posed an implicit challenge to the dominant role that scientists and institutions such as COPUS had, up until this point, enjoyed in

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conducting activity in this area. A line was also being drawn between differing approaches to PUS, and Wolpert, as the Chair of COPUS at this time, was almost duty bound to respond to such challenge.

It was in this already heated and emotive context that the stage was set for Collins and Wolpert to meet at the British Association's Loughborough meeting. The interactions at the meeting are good examples of the boundary work being deployed at this time by scientists and social scientists to protect or expand their cultural authority. It is interesting also to note the similar arguments and tactics on display throughout this exchange to those seen between literary intellectuals and scientists during prior cultural clashes. The fight for epistemic and cultural resources and authority by necessity entailing the need to define an 'in' groups and 'out' groups, to attribute deficits of understanding, and the decent into ad hominem attacks.

The morning session at the meeting had been arranged as a joint meeting between the Sociology and Social Policy and History of Science sections of the BAAS to explore the sociology of science. Frank James, (the editor of this issue), as Recorder for the History of Science section was the organiser. In the spirit of academic exchange the stage was set for a range of views on the sociology of science. The first two speakers were sociologist Brian Wynne and scientist and chair of the ESRC's public understanding of science research programme John Ziman, to be followed by Lewis Wolpert and finally Harry Collins. Ziman, though a vocal critic of the institutional approaches to PUS was clearly not sold on all of the claims the sociology of science had to offer, commenting in his note of agreement to participate that he has been 'trying to sort wheat from the epistemological chaff that spews from the sociology of scientific knowledge combine harvester' (Ziman, 1993). Despite this scepticism, his presentation, alongside Wynne's made different but balanced cases for the role of the social in the production of scientific knowledge.



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Wolpert, in contrast, made it clear from the outset his presentation would be hostile to the sociology of science. Statements about science, he argued should only be made by scientists:

It's slightly odd giving a talk like this as I'm not a sociologist, I'm not a philosopher, I'm actually a developmental biologist. Nevertheless when one reads some of the comments that people make now about science, particularly from the sociologists of science I feel more and more incumbent for scientists to get involved in this debate.<sup>3</sup>

He was clearly very much aware of the work of Gross and Levitt, recommending their book in his talk and, taking a very similar line of attack as they had in *Higher Superstition*, describing any comment on science by those from within the social sciences as being motivated by a resentment on their part:

“What it seems to me is, that in some sense the sociologists have won. There's this curious view about the nature of the world and the nature of science that is presented to the non-scientific world which is totally at variance as to what I consider to be science ... what I want to talk about really are the sociologists of science, who ... have not only obfuscated, but they have been extremely hostile to science. And I think that one of the characteristics of the post-modern world is that many in the humanities have failed to come to terms with the enormous success of science. Now that's not a nice thing to say, but I regret to say that it's probably true.”

Social studies of science, continued Wolpert, were intellectually bankrupt:

“If you are in my field you are wildly excited, new results are coming out everyday, we're really making tremendous progress. I feel that that's not necessarily the case in the sociology of science. I've heard Brian Wynne recycle

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that particular story of sheep, I think at least three times now, where's the really new evidence?"

What he termed the 'relativist' methodological approach employed by some sociologists of science was, to Wolpert, interpreted to mean that they simply saw science as a social construct and therefore not any better a means to understand nature than any other form of knowledge. While some sociologists did at this time take this ontologically relativist position, many did not and, as Collins pointed out in his response, most simply used it as a methodology, a distinction that Wolpert did not make in his general characterisation of social scientific views.

His attacks on all sociologists as being anti-scientific and motivated by envy, and his claims that their discipline was empty of any evidence, were, as Collins and Pinch themselves had argued many years before, typical of scientists wishing to reject anything they saw as unorthodox, and maintain a boundary of expertise around science (Collins and Pinch 1979). Furthermore, by making personal attacks on the 'accused' rather than engaging with the academic content of their claims, Wolpert was explicitly denying any authority to those claims, and indeed, in another personal slight, though it contradicted his earlier statements, dismissed sociology of science as "arid", "empty" and so trivial it did not even concern most scientists. As he told the audience:

"If you think that this type of debate is happening within the scientific community then you are wrong. Scientists, as a community know absolutely nothing about this whatsoever, and have no interest in it whatsoever."

The attack by Wolpert was ironic, given his own interest and the large amounts of time he and other scientists had spent engaging with social studies of science. As the video of the session shows, it was shocking to many in the audience. Several audience members in the question session afterwards criticised him for having a lack of understanding of sociology.<sup>4</sup> John Ziman denounced Wolpert's talk:

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“I’m not sure that what Lewis is saying has much content to it other than rejecting a view from his own emotional stance ... I am actually appalled by Lewis Wolpert’s anti academic way of dealing with these matters, I regard that as really inappropriate and really rather scandalous, and I hope that he will mend his ways. I do think that that is something that we should not tolerate in our senior academics and intellectuals.”

Harry Collins, who visibly angry and “embarrassed” as he stood up to follow Wolpert’s talk, accused Wolpert of lying to the audience, and attempted to defend the sociology of science from his charges of being anti-scientific. He argued that if Wolpert had actually spoken to any sociologist of science he would have found that they were in fact “science lovers” who loved “nothing better than to be in science laboratories, speaking with scientists, seeing how it works, trying to understand it. He further questioned Wolpert’s understanding of the field:

...if you’d read books like this more carefully you would have read that quite explicitly one of the things that we think we’re doing is trying to make the world a bit safer for science, trying to protect it against those who might react against science, because they see the failures of some of the over the top promises that have been made.”

Collins continued with a defence of both the relativistic approach of sociologists of science as a methodological principle, and the charge that there was no evidence in the field. The defence was as much a personal attack on Wolpert and his outdated idea of science, as it was a defence of sociology of science:

“I’ve been asked to speak on a platform with you before and I’ve refused because it seems to me that what you’re engaging in is a pantomime act rather than a scientific methodology ... They’re [sociologists] honest, they provide evidence, they argue among themselves, like cats and dogs, and we feel

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ourselves to be going into science as Merton described it, although there is this slight irony at the same time because it's not quite like Merton described it and you of course demonstrate the point quite adequately.”

Many of the audience members accused Wolpert of not behaving as an academic should and the meeting concluded with John Ziman urging the audience to remember that neither academic represented the scientific community or the social scientific community at large, and there was a middle ground for more fruitful discussion. The subsequent *THES* coverage was, however, constructed around the question of whether science was a social construct, a narrow interpretation of the debate, which also, in a similar fashion to Wolpert, ignored the many sociologists who did not take this view. As Wynne had argued at the end of the meeting, “sociological explanations of science do not mean that nature doesn't have a role in the construction of knowledge”, yet the press coverage presented the extreme sides of the argument. This, however, says as much about standard media practice of reporting a controversy, as it might about any particular stance the paper may have had. As sociologist Hilary Rose later reflected, this focus had left little scope for a meaningful debate about the sociology of science, nor did it reflect the diversity of opinion or approaches within either discipline:

The options for those sociologists who disagreed with Collins or those biologists who disagreed with Wolpert (to say nothing of those who as feminists thought both were unreconstructedly macho in thought and style) were shrivelled down to the binary choice of either Collins or Wolpert (Rose 1996).

The coverage both prolonged the debate, and moved it further into the public domain. While Wolpert's and Collins' positions were extremes the multitude of replies to the *THES* revealed many tensions and ensuing boundary work between certain science studies academics and scientists over who had the expertise and legitimacy to define and discuss science in public. Collins continued his defence of

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the social studies of science in the coverage, here resorting to similar *ad hominem* tactics to Wolpert, branding the conflict a witch-hunt and suggesting that the violent attacks on sociology were a result of a newfound insecurity of science, in a world where scientists did not enjoy unquestioned authority. Richard Dawkins on the other hand was quoted as calling the sociology of science 'chic drivel', and accused sociologists of hypocrisy, suggesting that the role of the sociologist of science should be limited to defending the claim that scientists were influenced by their own social and cultural background (Irwin 1994). Peter Atkins, a lecturer in physical chemistry at Oxford University contributed a commentary, which argued that science was independent of society. Painting a picture of science as progressive, objective, and universal, he argued that those who suggested that science could be any other way were 'motivated by jealousy in one of its disguises (such as political correctness) or by a wish to see science's progress tripped' (ibid). Thus, despite Wolpert's claim that science studies was not the concern of scientists, the coverage suggests otherwise. Indeed, this vocal response lent support to an earlier argument by sociologist Steve Fuller that underlying the attacks on science studies was the fear that the field would have 'a dangerous effect on the thinking of non-scientists who make science policy' (Fuller 1994: 143).

The Loughborough meeting continued to perform as a lightning rod for debate into the following year being described as an 'extraordinarily vituperative and horrible' argument between the two men, in the *New Statesman* (Johnson 1995), the popular magazine on the left of the political spectrum which incidentally had carried Snow's original two culture piece. Taking a more sympathetic view towards sociologists of science than the *THES* had done, the article claimed that the debate had little to do with philosophical matters, and more to do with scientists becoming 'aware of a public mood that has turned distinctly against science' and 'more mundane concerns like status and the contest between disciplines for a diminishing pot of funds' (Johnson 1995). Indeed Wolpert appeared to agree with this view; sociologists were playing politics, (though incidentally there was no suggestion from him that scientists were doing this also):

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They have a political agenda ... and that is to show that science does not have the authority to describe, in any particularly reliable way, the external world. By propagating this dangerous nonsense, they are undermining science, and since they do not understand science, they should be stopped. I think they want to get control of science funding. (ibid.)

Sociologist Andrew Pickering was also reported in the same article to claim that while scientists tended to think that they knew how best science was carried out, the way they presented the process of scientific inquiry was a 'story they trot out' and 'nothing more than Sunday theorising' (ibid). What was at stake, the article also quoted one historian of science as saying, was 'the right of non-scientists to comment on the way science seems to work' (ibid), and, more implicitly, who was allowed to popularise these views to the public. Certainly this whole episode showed that many scientists felt that it was their right to maintain control over their own narrative and where Snow had lamented the lack of influence and role of scientific thinking on policy, here, the opposite seems clear. Scientists were concerned with losing the influence they now felt they had to a new body of professional knowledge and expertise - Snow's proposed 'third culture' and were fighting to deny it any epistemic authority.

### **What goes around comes around...**

While the events and rhetoric surrounding the Loughborough meeting should certainly not be taken as indicative of the stances and views of all scientists and social scientists (much as Snow's views should not be taken to stand for all scientists, nor Leavis's for all literary intellectuals) it neatly highlights the ongoing boundary work between some scientists and non scientists towards the end of the twentieth century. Snow provided a useful rhetorical repertoire for scientists and others in mobilising support for their side or against another which continued to be used to justify science communication efforts or to discount others views about science in

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the new millennium. For example, Susan Greenfield, Director of the Royal Institution writing in *The Guardian* in 2003, lamented a 'widening gulf' between a science cognoscenti and everyone else. The public, according to Greenfield, now feared science, as it became ever removed from the ordinary person. As she argued:

Small wonder there is a knee-jerk to veto all this confusion and scary technology in one go. How can Joe Public, after a hard day at work, come home and be expected to tease out the pros and cons, weigh up the risks, consider all the implications, and differentiate the "yuck" from the reality? (Greenfield 2003)

The only way to evaluate the implications of science was, she continued, to be scientifically literate. Science studies academic Jon Turney, responding to her arguments the following week, claimed Greenfield, in making the arguments for a greater scientific literacy on the part of the public, was simply joining 'a long line of well-intentioned but not very well informed scientists', who were the only people that had believed in an anti-science movement in the first place (Turney 2003). Most recently, the prominent physicist Professor Brian Cox claimed that 'CP Snow's "two cultures" are still all too apparent in today's society.' In his endorsement of *The Geek Manifesto* (2012), a popular science book by Mark Henderson, intended as a call to arms for all science minded members of the public to defend it against irrational and unscientifically informed views.

## **Conclusion**

We can see the legacy of Snow's two cultures debate continued well into the twenty-first century. In particular, in and around debates concerned with the public understanding of science in the 1980s. The perception of and subsequent identification of a gulf of understanding and/or particular attitudes towards science, provided a rationale for scientists and scientific institutions to mobilise, be this popularisation, education efforts, or attempts to raise public feeling. As we have seen, however, this diagnosis and course of action was not new at this time, but has been an ongoing aspect of professionalization activity for scientists.

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Many of the main actors in PUS were also caught up in the 'science wars' of the 1990s which, in one sense could also be seen as a replaying of the two culture's debate, this time with social scientists and cultural theory scholars largely taking the place of literary scholars. Though we might also, as Leane (2007) has argued, see it is a diagnosis of the two culture's problem, with one side (science studies and related disciplines) arguing that there is only one culture, with science and literature both forms of "discourse" with equal epistemological validity and scientists arguing for the uniqueness of science.

This paper has argued that we should think about all these episodes as rhetorical boundary work, reflective of strains and stressors on science as an institution. Gieryn (1985, 1995) argues that the rhetoric of scientists has been important in constructing the authority of scientists and, more broadly, the professionalisation of science as a distinct community. Indeed, Luhmann (1995) has argued that communication is the defining characteristic of all communities, thus we can apply the same principle to other professional groups literary intellectuals, or social scientists. Within debates over the two cultures or the public understanding of science, we should therefore consider all acts of popular discourse as boundary work, with actors selecting, for example, particular attributes for science and scientists or literary intellectuals and mobilising them as part of a wider discourse concerned with professional expertise, resource management, and cultural status.

An explicit example of this boundary-work is the so called 'science wars'. The rise, over the latter half of the twentieth century, of a body of expertise – social science – that claimed ownership over the specific issue of the science and its definitions in public, meant that the public sphere became a key battle-ground over the legitimacy of these differing expertises. Each professional group, particularly within debates over PUS, defined 'the public' differently while attempting to align themselves with it. The boundary work, as much as it attempted to demarcate science and scientists from non-science and non-scientists also crucially served to construct each



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professional group as 'on the side of the public', or working towards their interests; a normative assumption that members of each profession know what is best for the public. For scientists this was largely a continuation of what Gieryn calls *protection* boundary work: science is at one and the same time separate from the public, yet is also something which can help to restructure society and the public for the better. Here, many scientists argued for a need to act to save the public from both itself (or its ignorant uptake of 'pseudoscience') and from social scientific approaches that they felt undermined their expertise and objectivity. Particular social scientists, on the other hand, were engaging with their own boundary work, mobilising what they believed was a more honest and realistic construction of science, which portrayed social interests as part of the scientific enterprise, and questioned the hegemony over public authority that science was perceived to hold. Many of these arguments relied on broad stereotypes, personal attacks and strawmen arguments to be effective. Yet if we shift our gaze away from whether such claims are true or false we can, and should, view these skirmishes instead as rhetoric; rallying cries and manoeuvres in a larger and longer battle that has been played out through Snow and others' claims before him, to establish or question science as the dominant form of knowledge in society and delineate who is allowed to speak for it, and wield its power.

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<sup>1</sup> Transcribed from archive video footage at [www.bbc.co.uk](http://www.bbc.co.uk), accessed on May 20<sup>th</sup> 2008.

<sup>2</sup> Survey results published around this time had also suggested a general public discontent with science. See, for example, Garfield, E. (1982), 'Is public confidence in science declining?' *Current contents*, 45.; Herman, R. and Kenward, M. (1985), 'What do people think of science?' *New Scientist*, 105.

<sup>3</sup> All quotes in this section are taken from a transcript of a video recording of the session made available to the author and referenced as: The British Association for the Advancement of Science (1994), *Meeting on the social sciences, Annual Meeting Loughborough*, transcribed by Simon Lock, May 2005.

<sup>4</sup> Personal observation from video footage of debate.