

• **ABSTRACT**

*The author became involved with the theory that AIDS originated from contaminated polio vaccines by arranging for publication of a key paper, by interacting with prominent partisans and by writing articles himself. These experiences suggest some of the advantages and disadvantages of partisan intervention in the scientific reception system by a social analyst. Open partisanship should be added to the repertoire of social analysts of science.*

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## **Sticking a Needle into Science: The Case of Polio Vaccines and the Origin of AIDS**

**Brian Martin**

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**In studying a scientific controversy**, should the analyst ever intentionally intervene as a partisan? What are the advantages of partisan intervention by a social scientist in learning about the dynamics of science? These questions are asked too infrequently in the science studies field, where intentional, planned intervention seems rare.<sup>1</sup> The following case study is presented to illustrate some of the possibilities and problems opened by partisan intervention. It will be followed by comments about the approach adopted, and an attempt to position the approach among various alternatives.

### **Polio Vaccines and the Origin of AIDS**

In 1990, Richard Sylvan, an Australian philosopher, sent me a package of materials he had received from Louis Pascal. Sylvan knew of my interest in the suppression of intellectual dissent, and indeed his own case has featured in my publications.<sup>2</sup> Pascal, an 'independent scholar' living in New York City, had written articles arguing that AIDS originated from contaminated polio vaccines used in Africa in the 1950s, but had been unable to get them

published. I wrote to him, thus beginning an involvement in the controversy over the origin of AIDS.

A brief overview of theories of the origin of AIDS is in order. The standard theory of the origin of AIDS is that a virus called simian immunodeficiency virus (SIV), found in monkeys or chimpanzees, was transmitted to humans where it became, or mutated to become, the human immunodeficiency virus (HIV). The method of transmission is unknown. The usual explanations are that a hunter received a cut while butchering a monkey and got monkey blood in the cut, that a human ate undercooked monkey meat, that a monkey bit a human, or that monkey blood was injected into humans during certain sexual rites.<sup>3</sup> There are two main types of HIV, called HIV-1 and HIV-2, of which HIV-1 is the most prevalent world-wide. Based on the divergence of strains of HIV-1, it is generally thought to have originated in Africa just before 1960, though some scientists argue for a much earlier origin.

Given that humans have been butchering monkeys and being bitten by them for hundreds or thousands of years, why would AIDS have developed in the 1950s? One explanation is that urbanization and the turmoil of war led to the spread of the disease out of a calm rural setting. Another is coincidence.<sup>4</sup>

Pascal accepted the standard theory, except for one key component: the method of transmission of SIV to humans. On the basis of his study of evidence from the medical literature, plus some careful thinking, he argued that at least one type of SIV was introduced into humans — becoming HIV-1 — during the world's earliest mass polio vaccination campaigns, carried out in central and west Africa from 1957–59. At least 325,000 people had this early vaccine sprayed into their mouths. The vaccine was produced by Hilary Koprowski, who is less well known than two other polio vaccine pioneers, Jonas Salk and Albert Sabin, whose vaccines became widely used elsewhere.

Pascal provided a series of reasons why this early vaccination campaign could have led to the entry of SIV into humans. Koprowski's vaccine was cultured on monkey kidneys, which meant that monkey viruses could contaminate the vaccine. Many monkeys with viruses are not affected by them or do not show symptoms, and so would not have been excluded from vaccine production. The vaccine used live polio virus,<sup>5</sup> so it was certainly possible for monkey viruses to contaminate the vaccine. It is

known that one monkey virus, SV40, was inadvertently given to hundreds of millions of people around the world via polio vaccine.<sup>6</sup> AIDS could have been produced via SIV contamination of just one batch of vaccine. There was no screening for SIVs, which were not discovered until 1985.

Pascal also discovered that the vaccine had been given to thousands of children, including infants less than a month old. Furthermore, the infants received 15 times the normal dose. Since the immune systems of infants are underdeveloped, they provide an ideal way of transmitting a virus from one species to another. The locations of Koprowski's main campaigns are not only in Africa, where HIV is thought to have originated, but also in precisely those regions of Africa now thought to have among the highest rates of AIDS in the world.

If this theory is correct, it has enormous significance. First, the use of monkey kidneys for producing polio vaccines should be stopped immediately, since there remains the possibility of introducing additional new viruses into humans. Second, much more scrutiny should be given to other transfers of animal tissue into humans, such as transplants of baboon livers into humans or the use of bovine haemoglobin. Third, the theory gives some suggestions for seeking ways to respond to AIDS — for example, by searching for monkeys or chimpanzees with SIVs similar to HIV-1 and HIV-2, and seeing how they are able to survive with the virus.

In 1987, Pascal wrote a paper describing this theory. He sent it to numerous biologists, but obtained only one acknowledgement and no comments. The paper was rejected by *Nature*, *New Scientist* and *Lancet*. Encouraged by the editor, Pascal wrote a longer and different account for the *Journal of Medical Ethics*, but this was eventually rejected for being too long.

After corresponding with Pascal, during which time the *Journal of Medical Ethics* rejected his paper, I arranged publication of the paper in a working paper series at my university.<sup>7</sup> There were several justifications for this. First, from his writings and responses to challenges and from his letters to me, I judged Pascal to be a highly intelligent, rigorous and meticulous researcher. Second, he had pretty much exhausted orthodox channels for publication. Third, his theory has important social implications, as noted above. Fourth, Pascal's paper included a forceful argument about the responsibility of scientists and editors. Fifth, Pascal's paper included some intriguing comments on HIV as an actor, outwitting

human resistance (including peer review systems), which would be of special interest to science studies scholars.

Publication of Pascal's paper also provided an opportunity for a social science experiment: to see how his ideas spread as a result of this intervention into the 'scientific reception system'. In other words, the publication was, among other things, a probe designed to test the response of the scientific community.<sup>8</sup>

Although I was trained and worked as a scientist, no branch of biology was ever an area of my expertise. My position in championing Pascal's work was to argue that it deserved a hearing. But why pick out the polio-vaccine theory? After all, there are untold unorthodox theories that might be promoted. One reason for promoting this theory was the opportunity to do so. Another was the need. For example, another unorthodox theory — namely, that HIV is neither necessary nor sufficient to cause AIDS — has been energetically promoted by Peter Duesberg, a prominent scientist.<sup>9</sup> There was no such prominent scientist pushing the polio-vaccine theory. I also used my own judgement, assessing the theory on both scientific and social scientific grounds. Pascal argues that even a schoolchild can understand his arguments. This may be an exaggeration, but it is certainly the case that advanced study, whether in mathematics or molecular biology, is not required to decide whether the theory is worthy of further investigation. The potential of the theory was supported by a few specialists, notably Gerasimos Lecatsas, Professor of Virology at the Medical University of Southern Africa, Pretoria, and Jennifer J. Alexander, Professor of Microbiology at the University of Witwatersrand, Johannesburg.<sup>10</sup>

From the social science point of view, there are several reasons why Pascal's submissions to journals would have been rejected. He had no name in the field and, indeed, had no formal position as a scientist. Writing from a private address, his contributions were likely to be dismissed. Also, perhaps due to working outside the scientific establishment, his writing was not quite in the standard style. Finally, and most importantly, linking polio vaccines and AIDS is highly threatening to immunologists. It is likely that even a prominent scientist would have encountered hostility trying to promote this theory.<sup>11</sup> Sociologically, then, there was every reason to believe that the response of the scientific reception system (primarily editors and referees) to Pascal's submissions was much more hostile than it would have been to an orthodox, unthreaten-

ing theory presented by a recognized scientist working at an established institution.<sup>12</sup> Thus, I used a sociological assessment of the issues to help draw a conclusion about the scientific merits of the theory — namely that it deserved more examination than had so far occurred.<sup>13</sup>

The view that a scientific hypothesis deserves more examination is hardly a daring one;<sup>14</sup> for a social scientist who claims no special expertise in the field, it is a suitably modest one. Although my formal stance has been indifference to the correctness of the polio-vaccine-AIDS theory, but simply concern that it be given a fair hearing, it would be disingenuous to pretend that I have no personal view on the matter. Two important reasons why I chose this theory to study and promote, out of many rejected by the scientific establishment, were that, in my judgement, it has significant social implications and a reasonable chance of eventually being judged by the scientific community to be correct. Furthermore, studying the arguments and becoming aware of efforts to deny and squash the theory meant building up some degree of commitment to it.

Pascal's paper, 'What Happens When Science Goes Bad', was published in December 1991 in the working paper series of a research programme at the University of Wollongong.<sup>15</sup> Pascal gave me a list of some 25 names, including some well-known scientists and philosophers, to whom I sent copies.<sup>16</sup> I also sent copies to my contacts on the suppression of intellectual dissent, and to some of the people with whom I was corresponding anyway. For the most part, though, I simply sent copies to anyone who requested them, and to anyone to whom I was recommended to send them. The paper was provided free of charge.

Some recipients wrote to Pascal (whose address was included in the working paper, though not in a conspicuous place) and some to me, and Pascal and I exchanged copies of much correspondence sent and received.<sup>17</sup> In this way I developed a feeling for the impact of the paper as it was disseminated. Three significant early responses were from W.D. Hamilton, Professor of Zoology at Oxford University and an acclaimed evolutionary biologist,<sup>18</sup> who was largely supportive of Pascal's ideas; Robert M. May, also a Professor of Zoology at Oxford University and a prominent mathematical biologist,<sup>19</sup> who was critical both of Pascal's style and ideas; and microbiologist Jennifer Alexander.<sup>20</sup>

After receiving a number of these responses, in February 1992 I

took another step in promoting Pascal's paper, sending it to science journalists at the major Australian print media (all six of them!).<sup>21</sup> At first this appeared not to produce any response, but a month later I received a call from Julian Cribb of *The Australian*, a national daily newspaper. He had spent weeks in medical libraries tracking down references cited by Pascal and finding others, and was horrified by what he had found. On the basis of his investigation, Cribb wrote a major story.<sup>22</sup> He later told me that he was contacted by numerous readers about the story but that the Australian medical establishment remained silent.<sup>23</sup>

Pascal asked me to send a copy of his Wollongong working paper to the *Journal of Medical Ethics*, which earlier had rejected it. The editor of the journal, Raanon Gillon, wrote an editorial announcing that the paper had been published at the University of Wollongong and explaining why the *Journal of Medical Ethics* had refused to publish it. He provided quite a favourable account, saying that Pascal's thesis 'is an important and thoroughly argued one and ought to be taken seriously by workers in the AIDS field'.<sup>24</sup> The editorial also gave full details on how to obtain Pascal's paper. This had a significant long-term effect, leading to dozens of requests for copies of Pascal's paper over the following years, many from doctors and medical ethics centres.

There were also requests from people outside the mainstream. At the behest of a friend, I sent a copy to the Australian magazine *Nexus*, which features sensational stories about unorthodox views and fringe science. The editor wrote a brief notice about the paper (saying 'I recommend you obtain this paper — IT'S INCREDIBLE!') and as a result, numerous requests for the working paper were received, most typically from small Australian towns.<sup>25</sup> A year later, after giving a talk about the theory to a meeting of a homeopathic society which was reported in the society's journal, I received quite a number of requests for copies of Pascal's paper from readers of the journal. As well as the hundreds of copies that I distributed, many recipients made photocopies and circulated them. Judging from the correspondence and the amount of interest in further circulating the paper, it was quite a hit. Certainly it generated vastly more interest than any other working paper in the series.

My social science experiment in following the distribution and impact of Pascal's paper was soon interrupted by another development: independent discovery and publication of the same theory. Blaine Elswood, an AIDS activist in San Francisco, developed the

same ideas as Pascal. Elswood then fed information to freelance journalist Tom Curtis, who investigated further, interviewed Koprowski, Salk, Sabin and others, and wrote a comprehensive story on the theory published in *Rolling Stone* in February 1992,<sup>26</sup> as well as a series of articles for the *Houston Post*. *Rolling Stone* is hardly a scientific publication, but it has a circulation of one million. Curtis's story led to follow-up stories in numerous media outlets and journals, including the *Los Angeles Times*, *Washington Post*, *Science*, *Nature* and *New Scientist*. There is a nice irony here. Pascal could not get his sober treatments of the theory published in scientific journals, but they were willing to publish stories following an account in *Rolling Stone*!

The *Rolling Stone* article was quickly brought to my attention by various contacts. I despatched two copies of Pascal's paper to Elswood, who in turn sent a copy to Curtis as I requested, and thus I began communication with each of them. From Elswood I learned, among many other things, about the difficulties that he and co-author Raphael Stricker had in publishing a scientific paper in Luc Montagnier's journal *Research in Virology*.<sup>27</sup> From Curtis, I learned that he sent several versions of a letter, along with copies of references, to *Science* taking issue, point by point, with a long letter by Koprowski that had been published in that journal. Curtis told me that *Science* refused to print his letter.<sup>28</sup> The independent publication of the polio-vaccine-AIDS theory by Curtis and by Elswood and Stricker provided me with more insights into the response of the scientific community to this particular threatening idea.

By being the central distribution point for Pascal's paper, I was also the recipient of much information. For example, when English journalist Andrew Tyler wrote a story about the theory in *The Independent Magazine*, I soon received copies of the story, and, later, a copy of correspondence between Tyler and Pascal.<sup>29</sup> The simple explanation is that people are more likely to provide information to someone who is sympathetic and who is providing them with something in exchange.

In spite of all this activity, in August 1992 I decided that a further intervention into the debate was in order. I wrote an article about peer review aspects of the theory, recounting the experiences of Pascal, Lecatsas and Alexander, Elswood and Stricker, and others. Pascal, Curtis and Elswood provided comments which corrected mistakes and sharpened the arguments. My paper was

rejected by the *British Medical Journal* and then accepted by *BioScience*.<sup>30</sup>

In response to the intense publicity following the publication of Curtis's article in *Rolling Stone*, the Wistar Institute in Philadelphia — the manufacturer of Koprowski's polio vaccines, and the place where Koprowski still worked — set up an independent inquiry into the polio-vaccine-AIDS hypothesis. In September 1992, the committee of scientists reported that the chance that the hypothesis was true was 'extremely low', but nevertheless recommended that serious efforts be made to switch away from monkey kidneys for culturing polio vaccines.<sup>31</sup> This conclusion was favourably reported by *Science* which did, however, publish a reply by Curtis.<sup>32</sup> Through my correspondence I obtained Pascal's detailed critique of the committee's report and also confirmed what, from my reading of the report, was fairly obvious — namely, that the committee had not contacted Curtis, Elswood or Pascal.

Meanwhile, a new development occurred: in December 1992, Koprowski sued Curtis and *Rolling Stone* for defamation. Whatever the motivation behind this legal action, I was not the only one to note that a prime effect was to shut down most media discussion of the theory. As part of the 'discovery' phase of the case, each side had to provide copies of relevant documents. Initially, Koprowski provided some 40 pages, but Curtis had to provide hundreds if not thousands, including copies of all his notes made preparing the *Rolling Stone* article — he had not promised confidentiality to people he interviewed and with whom he corresponded.<sup>33</sup> Curtis was also inhibited in doing further investigation of the theory, since he would have to tell any informant that 'Anything you say to me might end up in the hands of Koprowski's lawyers', or words to that effect.

On 25 February 1993, I posted a notice on the Sci-Tech-Studies electronic mailing list,<sup>34</sup> telling about the theory and Koprowski's defamation case, and asking for examples of the use of legal action to stop discussion of scientific theories. This led to a number of suggestions and quite a few requests for Pascal's paper. I sent a note to *Nature* about the dangers of legal action squashing scientific discussion,<sup>35</sup> and incorporated information about the legal action in my paper for *BioScience*.

In November 1993, just before Koprowski had to undergo deposition, his lawyers settled out of court. *Rolling Stone* paid Koprowski a grand total of \$1, and published a statement.<sup>36</sup> Of



course, a statement made under financial duress — *Rolling Stone's* legal expenses were some \$500,000 — should have little credibility. Curtis believes that my correspondence in *Nature* and my article in *BioScience* may have influenced Koprowski to settle, by showing that his legal action might have stimulated rather than discouraged interest in the theory.<sup>37</sup>

Ironically, my *BioScience* article, published in October 1993,<sup>38</sup> did not produce many requests for Pascal's paper. Instead, I received several letters from scientists with strong interests in peer review. However, the article has been distributed widely and was a useful source for an article in a high-circulation Dutch magazine.<sup>39</sup>

In 1993, I was visited by a worker in the AIDS field who is well known due to studies circulated world-wide, who had received a copy of Pascal's paper from a colleague and had then put considerable effort into further investigation of the arguments.<sup>40</sup> This scientist, whom I will call Dr A, prefers to keep a low profile on this issue because s/he feels it can undermine one's credibility to get involved with it. In any case, Dr A took the opportunity of meeting me to learn more about what I knew about the theory and its social context, and to inform me of the progress of a personal investigation.

As mentioned earlier, eminent biologist W.D. Hamilton was favourable towards the polio-vaccine-AIDS theory. In January 1994, he sent a letter to *Science* responding to Koprowski's 1992 letter in *Science*, which remained the only substantive published criticism of the theory. *Science* earlier had refused to publish Curtis's reply to Koprowski's letter; it now refused to publish Hamilton's. Hamilton then wrote a personal letter to the editor of *Science*, Daniel E. Koshland, Jr, asking for a reconsideration of the decision in the light of the enormous significance of the issues involved. This was one of the most eloquent letters I had read in quite some time. In response, Koshland reiterated *Science's* rejection of the letter. This development confirmed my earlier assessment that the rejection of Pascal's submissions owed as much to the threat posed by his ideas as it did to his status as an outsider and the style of his writing.<sup>41</sup>

In February 1994, I received a call from John Roberts, who said he was working for a foundation in San Francisco that had been working on blood and blood-product-related legal aspects of AIDS for nine years.<sup>42</sup> He said he was supporting a massive lawsuit against the Federal Drug Administration and others for

giving children AIDS via polio vaccines, and claimed to have informants in the Wistar Institute and elsewhere. He told me much else, called again a week later, and sent me a letter and a videotape of US television programmes. I gave him the names and addresses of Pascal, Elswood, Curtis, Lecatsas, Alexander and Hamilton, and Roberts later contacted them. Elswood and Curtis each became suspicious about Roberts. Though there was independent evidence, including several news stories, about a legal case in which the parents of a young girl who contracted AIDS alleged that the polio vaccine was responsible,<sup>43</sup> confirmation of Roberts' involvement was not easy. For example, Curtis obtained information from Roberts about the year he claimed to have graduated from Harvard Medical School, but found that Harvard had no record of any graduate that year named John Roberts. Who 'John Roberts' is and who (if anyone) he is working for remains somewhat of a mystery. My role in the affair had been to provide the immediate means for Roberts to contact others.<sup>44</sup> However, since early 1994, 'John Roberts' has dropped from sight.

In October 1994, I visited the US to attend the annual meeting of the Society for Social Studies of Science (4S), held in New Orleans. It was an ideal opportunity to meet Curtis, who lives in Galveston. Elswood came down from Utah and Malcolm Davidson, an engineer from New York who has followed the theory, also joined our discussions. John Douard organized a successful symposium at the 4S meeting about the polio-vaccine-AIDS theory, in which Curtis and I spoke as well as Michael Curtis, a lawyer and Tom Curtis's brother. Though not a formal speaker, Elswood participated in a major way. We also gave a seminar at the University of Texas Medical Branch in Galveston. However, the most important part of my visit was a two-day meeting at Curtis's house with Elswood and Malcolm Davidson, before going to New Orleans. As well as ongoing discussion about the theory and its reception, I took the lead in collating options for action and focusing discussion on how to proceed. We discussed angles for pursuing further work, including SV-40, dangers from animal viruses, legal threats to discussion of scientific theories and the social responsibility of scientists. We also discussed the interests and attitudes of various individuals who might investigate the theory, and possible publication outlets. A major outcome of this meeting was that I agreed to compile a package of information about the theory,<sup>45</sup> and to send it to a range of scientists and

journalists. In December 1994 and January 1995 I sent more than a dozen packages. This generated a further round of correspondence — for example, with Mirko D. Grmek, author of the highly regarded *History of AIDS*.<sup>46</sup>

The 24 March 1995 issue of the British newspaper *The Independent* had a dramatic front-page headline story entitled ‘World’s First Aids Case was False’.<sup>47</sup> A Manchester man who died in 1959 had previously been recognized by most scientists as the first victim of AIDS for which verification had been obtained, using sophisticated tests for HIV.<sup>48</sup> This case, which received enormous attention when these tests were published in 1990, was used by the Wistar Institute Committee as the most telling evidence against the polio-vaccine-AIDS theory.<sup>49</sup> The story in *The Independent* broke the news about further investigations led by David Ho — director of the Aaron Diamond AIDS Research Center in New York — which concluded that the Manchester man’s remains contained no evidence of HIV and that someone else’s tissues, which revealed HIV in 1990, were mixed in. It appeared to the investigators that the earlier positive results could only be explained by a mix-up or fraud.<sup>50</sup> There are a number of intriguing aspects to this development,<sup>51</sup> but for present purposes the important point is that the elimination of this case removes a key objection to the polio-vaccine-AIDS theory, as was recognized by one of the original stories in *The Independent*.<sup>52</sup> However, there seems to be no sign of a reconsideration of the theory in the light of the new developments.<sup>53</sup>

There is much else that could be said about the polio-vaccine-AIDS theory and its reception, including new evidence and arguments, the involvement or non-involvement of other scientists and activists, and further correspondence and conflicts. Here I have emphasized my own role in order to throw light on the strategy of partisan intervention by an analyst of science.

### **Some Advantages and Disadvantages of Intervention**

#### *Access to Large Quantities of Correspondence*

In my case, this includes my own correspondence as well as copies given to me of others’ correspondence. The value of this material is immense: it includes questions, tentative ideas, confidential material, drafts of letters and much else that provides insight into the issue. Much of it would be unavailable to someone who was

not seen as a partisan. Even more significantly, there is enormous value in the correspondence to me personally, as much was often sent in response to my earlier distribution of documents, letters or articles. By being involved as a participant, I can advance queries, raise issues and provoke responses in a way impossible for someone viewing the issue as an outsider.

Being involved in the debate also introduces limitations in regard to some correspondence and documents. In a few instances, I have received material whose content I have been asked not to reveal. Nevertheless, such material provides valuable background insights into the issue. Also, insider documents from the other side are unlikely to be available. (Exceptions include responses from *Nature*, *Science*, *Research in Virology* and other journals to submissions from Pascal, Curtis, Elswood and Hamilton.)

### *Contact with Leading Participants*

In many cases, this would involve face-to-face discussions and telephone conversations. In my case, due to the distances involved, I have had few face-to-face meetings even with interested parties in Australia (such as Julian Cribb). My contact with leading participants has included a voluminous postal correspondence with Pascal (his letters to me alone total more than 50,000 words), extensive electronic mail with Elswood (hundreds of messages), both post and occasional telephone conversations with Curtis, conversations with John Roberts, meetings with Dr A, Curtis and Elswood, plus correspondence with a number of others. This is not to mention face-to-face and telephone discussions with many people who had read or heard about the theory. Another feature of this kind of contact is that it 'occurs naturally' in the midst of the issue as it develops, and takes the form of discussions, comment and consultation rather than interviews. Formal interviews with participants at a later stage typically involve reconstructions of events that give a different slant.<sup>54</sup>

### *A Degree of Control Over the Issues*

By publishing Pascal's paper and writing papers myself, I have played some role in shaping the course of the debate. For example, both my introduction to Pascal's paper and my article in

*BioScience* make a connection between the response to this theory and the suppression of intellectual dissent. In my meeting with Curtis and Elswood in 1994, I helped set the agenda for future promotion of the theory. The more prominent a person's role in a debate, typically the more influence they can have over the issues debated. But, of course, there are severe limits to this influence, since many others have their own ideas about what issues are important.

### *Understanding of Communication Networks*

By being a key node in the network of people discussing and promoting the polio-vaccine-AIDS theory, I have been in an ideal situation to understand how the network itself operates. For example, there have been 'high-density' channels — such as between Elswood and me — that would be unsuspected from anyone examining published documents only. Another example is the strong interest and support of W.D. Hamilton, known to me via correspondence with Pascal.<sup>55</sup> The 'network' as a whole has been more coherent than it would have been without my own involvement. Insights from participation in the communication network include a keen appreciation of the contributions of key players, knowledge that there is no central control over the agenda and awareness of heated disagreements between key figures concerning theory and tactics. Perhaps the greatest value gained by being involved in the communications network is the ability to evaluate hypotheses about the social dynamics of ideas on this issue. An outsider trying to understand a controversy may assume or conclude that certain arguments are important or certain interests play a role. An insider can quickly assess many such assumptions and conclusions. Another aspect of understanding the communications network is a more acute sense of the course of developments over time. Bursts of activity and slack periods are experienced personally, rather than just noted via dates on documents.

### *Some Disadvantages of Intervention*

The pitfalls and problems of partisan involvement are many; here I mention a few obvious ones. The 'other side' is likely to be less

than helpful, and many of their documents are likely to be 'unavailable'. Then there is the shaping of the analyst's understanding due to partisanship, which, to use positivist language, can simply be called bias or distortion. The analyst's involvement may so perturb the issue that it is impossible to guess how it would have proceeded otherwise. Also, the intervening analyst may spoil the issue for others who want to study it, by setting agendas, causing participants to be wary of other analysts, and so on. By being perceived to be a partisan, a social analyst's credibility can be reduced, which can affect further investigation or future studies on other topics.<sup>56</sup>

Intervention can be a form of cultural imperialism if and when the analyst behaves in a way that is oppressive for the community being studied. This is unlikely to be a problem when social scientists intervene in the scientific enterprise, which itself is the dominant culture of knowledge in western societies — at least, that is, when social scientists tackle the stronger rather than the weaker elements in the scientific community.

### Insights from the Case Study

Although the main aim of this paper is to illustrate an intervention approach in science studies, rather than to document results, it is worth noting some of the things I have learned about the dynamics of this issue. (Some of these were mentioned in the description of the case.)

Louis Pascal is undoubtedly a key figure in the development of the polio-vaccine-AIDS theory. Yet he has had only one significant publication in the field, the working paper at the University of Wollongong whose publication I arranged. By contrast, Elswood, Stricker, Curtis, Lecatsas and Alexander have published in the scientific literature and Curtis (in *Rolling Stone*) has achieved a much wider circulation of the ideas. It is only by looking behind the scenes that Pascal's contribution becomes apparent. He is an incredibly meticulous and prolific correspondent. In his letters he has provided long and detailed analyses of arguments and evidence and has engaged in intense scrutiny of writings by Curtis, Hamilton and Elswood, as well as responding to many others.

While there has been concentrated discussion of the theory within the network of supporters, there has been very little direct

dialogue with scientist-critics. There are exceptions, though: Curtis interviewed Koprowski, Sabin and Salk, among others, although these scientists dismissed the theory out of hand; Robert M. May and I exchanged a few letters; some scientists offered comments after receiving a copy of Pascal's paper. But the main pattern has been lack of direct dialogue.<sup>57</sup> This replicates the published record in this case.<sup>58</sup>

Pascal's paper generated enormous interest. Most of the copies distributed have been provided or requested on the recommendation of others who have read it. For example, one recipient made 200 photocopies for further circulation. The article in *Nexus* led to numerous requests from within Australia, the article in *Journal of Medical Ethics* to many from around the world. By being the publisher and distributor of Pascal's paper, I have kept a record and obtained a good idea — from their geographical locations, institutional affiliations and comments made — of the sort of people interested in the issue. There are no equivalent data for readers of Tom Curtis's article in *Rolling Stone*.

The fact that many hundreds of copies of Pascal's paper have been circulated, and that dozens of people have responded with comments, suggests a greater impact than most scientific papers enjoy. There was relatively little direct response to my articles in *BioScience* and *Townsend Letter for Doctors*.<sup>59</sup> Julian Cribb's article in *The Weekend Australian* generated relatively little correspondence, and none from the medical establishment.<sup>60</sup> In early 1992, I circulated copies of Pascal's paper to several magazines of social criticism, such as *Earth First!*, *Mother Jones* and *New Internationalist*, with a covering letter suggesting the potential importance of the issues covered. There were only a few acknowledgements and, to my knowledge, only one short article published as a result.<sup>61</sup>

The above observations give an idea of some of the insights gained by my participation in the issue. There are many other observations that I could make, especially about the theory itself, although some of these would have been available to a non-participant reading the published literature.

Could a nonintervening analyst gain the same perspective on this case as I have? Even if, somehow, an analyst could obtain all the documents about the case, this could not possibly provide the same perspective because it would miss the way that my own involvement has shaped the evolution of the issue.<sup>62</sup> I arranged for

the publication of Pascal's paper, which in turn influenced the development of the network of people examining the theory. I interacted with Pascal and, for example, saw his responses to my suggestions for revising his paper. I helped to shape the direction of the debate over the theory and the distribution of material about it and, in doing so, gained insight into the positions and responsiveness of various individuals, both supporters and critics. Intervention perturbs the issue being studied, and so the insights are bound to be different from those available to a nonintervening analyst. In other words, probing the social system of science can trigger responses that reveal aspects of science not accessible to a nonintervening observer, just as carrying out scientific experiments can lead to understandings of nature not available through observation alone.

As stated earlier, understanding more about the dynamics of the polio-vaccine-AIDS issue and about the operation of the scientific reception system was only one aim of my involvement. Another aim was to help change the way with which unorthodox theories are dealt. Certainly my involvement has led to greater visibility of the polio-vaccine-AIDS theory, through my own articles and through helping maintain the network of the theory's supporters. It is less easy, however, to observe any great impact on the scientific reception system more generally.

### **Positioning the Case Study**

There are many different ways to categorize case studies in the analysis of science. Here, three different areas are examined: the purpose and effect of the study, the involvement of the analyst in the issue being studied, and the type or degree of partisanship of the analyst. These categories are neither exhaustive nor mutually exclusive, but are convenient here for showing how the polio-vaccine-AIDS study relates to other approaches.

#### *Purpose and Effect*

There are several possibilities here, including understanding the operation of science, effecting change in science and society, and advancing the analyst's career.<sup>63</sup> Each of these possibilities,



**TABLE 1**  
Possible Purposes and Effects of Science Studies

Purpose	Effect
Understanding	Understanding
Change	Change
Career advancement	Career advancement
. . .	. . .

among others, may be the intent of the analyst (the purpose), the effect of the analysis, neither or both (see Table 1). Understanding of the operation of science is a standard purpose in academic science studies, effecting change is a standard purpose in much science policy, and career advancement is a common but seldom acknowledged purpose.<sup>64</sup>

Just to say that the intent and/or effect of a study is understanding or change is not the end of the matter. More specifically, what sort of understanding or change is intended or caused? In this case study, my intent was to learn about how the scientific community responds to an unorthodox and threatening theory by being an insider in the promotion of the theory. I also intended to help change the operation of science by giving the polio-vaccine-AIDS theory added support. Specifically, my aim has been to help promote serious examination and testing of the theory, followed by serious action if it seems to have a reasonable chance of being correct — including stopping using monkey kidneys for making polio vaccines, and stopping other practices that may allow cross-species transfers of viruses. More generally, my purpose has been to alert some people to the obstacles facing such theories.

My assessment is that my involvement has certainly helped to promote the theory. My activities have given much greater visibility to the work of Pascal, have directly or indirectly encouraged the participation of others such as Hamilton, and indirectly may have contributed to the resolution of Koprowski's defamation case against *Rolling Stone* and Curtis. Whether it has contributed to understanding, or to any of the other effects, is difficult to say.<sup>65</sup>

### *Involvement*

A second category is the involvement or participation of the analyst in the issue being studied. The usual conceptualization distinguishes

between complete observation at one extreme and complete participation at the other. At the observation extreme, the social scientist uses 'unobtrusive measures' to study the phenomenon in question, such as reading documents or watching from a concealed location. At the participation extreme, the social scientist joins in as a member, as in Festinger's classic study *When Prophecy Fails*, in which social analysts joined an end-of-the-world cult, pretending to be believers, in order to find out how belief systems could be maintained in the face of disconfirming evidence.<sup>66</sup>

Another framework for conceptualizing participation has been proposed by Harry Collins: 'participant comprehension'.<sup>67</sup> In this mode of participation, the social scientist learns about the area by trying, however temporarily, to pass as a competent member or native — parapsychologists, in the case of Collins and Pinch.<sup>68</sup> This provides the social scientist with an insight into what constitutes the native 'form of life'.

Another way for participatory fieldwork to occur is for a native to become a social scientist. The native has complete participation and comprehension, but often lacks the conceptual tools of the social scientist. There are quite a few cases in which scientists have written accounts that in some sense count as social science.<sup>69</sup> Whether such cases can legitimately be categorized as participatory fieldwork cannot be addressed here.

My study of the polio-vaccine-AIDS theory is certainly a type of participatory fieldwork. It is partially captured in the usual conceptualization as much closer to complete participation than complete observation. In terms of Collins' ideal-type model, my study goes part of the way towards participant comprehension, though not so far as Collins' involvement in parapsychology research, since I have not myself undertaken any investigations into the scientific aspects of polio vaccines and AIDS. Since I have retained my role as a social scientist and have been openly involved as a social scientist, it may be necessary to set up another participation-related conceptualization, with the extremes being nondisturbance and major change (see Table 2). Festinger and his colleagues did not aim to change the psychosocial dynamics of the group they joined (though their participation certainly had an effect). Similarly, Collins and Pinch do not say that they intended to change the way the scientific community dealt with psychic phenomena, though their work certainly had a considerable impact. In my case, my participation *was* intended to change the way the polio-vaccine-AIDS theory

**TABLE 2**  
**Three Conceptualizations of Degrees of Participation**

<i>Normal conceptualization (following Collins)</i>		
complete participation	_____	complete observation
<i>Collins's conceptualization</i>		
participant comprehension	_____	unobtrusive observation
<i>Intervention conceptualization</i>		
major change	_____	no disturbance

was dealt with; and it has had some impact on the circulation of the ideas and the coordination of the work of leading proponents.

These alternative conceptualizations of participation are linked to the stated purpose of the research. In the cases of Festinger and his colleagues, and of Collins and Pinch, their stated primary purpose was understanding, though their participation also led to considerable impacts on the subjects and fields of the studies. My purpose, by contrast, was at least as much to effect change as to gain understanding.

*Partisanship*

In terms of the stated intention of the analyst, the issue of partisanship seems relatively straightforward (see Table 3). On the one hand, there are studies in which the analyst intends to remain neutral with respect to the issues under study. On the other hand, there are numerous studies with the aim of improving society,

**TABLE 3**  
**Some Types of Partisanship**

<b>Intention:</b>	Neutrality Partisanship (covert) Partisanship (overt)
<b>Effect:</b>	No effect <i>De facto</i> partisanship Capture by (other) participants Overt partisanship

often going by the name of 'action research', in which partisanship is open and usually acknowledged. Generally speaking, action researchers see the process of gaining knowledge and changing society as interlinked, even inseparable. Intervention to change society provides understanding — including new perspectives of fundamental theoretical significance — which in turn can be used to develop more effective intervention. In many cases, a key goal is involvement by members of the 'community': those who are commonly the objects of the research instead become the subjects. Studies of this sort are often called 'participatory action research'.<sup>70</sup> Examples include interventions in schools,<sup>71</sup> studies of industrial democracy,<sup>72</sup> investigations of, and support for, social movements by Alain Touraine and colleagues,<sup>73</sup> and action anthropology.<sup>74</sup>

There are a number of examples where science studies researchers have been open partisans. For example, Sharon Beder, in her study of the Sydney sewerage issue, was a silent member of a group challenging the Sydney Water Board and fed crucial information from her investigations to journalists, whose stories she then had dutifully to cite in her own writing.<sup>75</sup> My own study of the views of two leading supporters of nuclear technology in Australia falls into this category, as it was intended to be (and was used as) a partisan intervention into the nuclear power debate.<sup>76</sup>

Whatever the intention of the analyst, there are several possible ways in which the research may end up being partisan in effect. One can be called '*de facto* partisanship', a process by which choices about research topics and methods partially shape both the conclusions reached and the social use of the study.<sup>77</sup> In undertaking the study of an issue, a social scientist makes a number of decisions: what issue to study; which particular time periods, locations or facets of the issue on which to concentrate; and what theoretical frameworks to adopt. A researcher may choose to study repetition strain injury (RSI) rather than automobile incidents; may choose to study RSI in Australia in the 1980s rather than RSI in the US in the 1980s, or in Australia in the 1960s; and may choose a framework based on one or more of positivism, relativism, social psychology, political economy, and many other theories. These choices inevitably shape the conclusions reached. This meta-level partisanship, due to choices of methods and research topics, is appropriately called '*de facto* partisanship' because it is built into assumptions underlying the enquiry, without any requirement that the researcher be overtly or consciously partisan.

Another way in which the effect of a study may be partisan, sometimes in spite of the intent of the analyst, is through the 'capturing' of the analyst's work by participants in the controversy.<sup>78</sup> That is, certain partisans in the controversy may find the social analysis so useful that they attempt to deploy it for their own purposes.

The involvement of the analyst in the issue being studied introduces a new dimension to the phenomenon of capturing. Relativist analysts of scientific controversies are susceptible to being captured by one set of partisans — typically, but not always, the side with lesser cognitive authority.<sup>79</sup> Hess extends this notion to multiple processes of capture and attempted capture, by various partisans, of the analyst, as well as of other participants or observers.<sup>80</sup> A social analyst's deliberate intervention can be conceived as an attempt to capture selected participants for the analyst's own purposes: in this case, the capturing is *by* rather than *of* the analyst. The analyst's purpose will, in general, both overlap with and diverge from the purposes of the participants in question.<sup>81</sup>

In the case of polio vaccines and the origin of AIDS, the proponents of this theory — especially Pascal, Elswood and Curtis — can be interpreted as capturing me, a social analyst, to help promote the theory. But it makes just as much sense to see me as capturing them to mount a more effective and unified campaign, not only to promote the theory but also to probe the scientific reception system and to expose bias against theories threatening to the scientific establishment. In both cases, the term 'capture' is perhaps the wrong word since it connotes unwillingness on the part of the captured. 'Mutual enrolment' or 'joining forces' are more appropriate descriptions.

The polio-vaccine-AIDS example also illustrates another type of enrolment or capture process: attempts to take over or shift entire areas of discourse.<sup>82</sup> At root, the struggle is about what is to count as the legitimate and authoritative discourse on the scientific aspects of AIDS. Control over this discourse is normally exercised by scientific élites, such as editors of scientific journals: proponents of the polio-vaccine-AIDS theory found the greatest difficulty gaining access to this arena. Consequently, they used alternative media, more accessible to them, and writing styles more accessible to nonscientists; the most obvious cases of this being the Wollongong publication of Pascal's paper, and Curtis's *Rolling Stone* article. Supporters of the orthodox theory of the origin of AIDS reacted in

various ways: by ignoring the challenge, by Koprowski's reply in the letters column of *Science*, and by the Wistar Institute's appointment of a scientific advisory committee to comment on the challenging theory. Each of these responses can be seen as an implicit assertion of the primacy of the arena of peer-reviewed scientific publication and mainstream scientific authority. Koprowski's action for defamation against Curtis and *Rolling Stone*, on the other hand, both challenged the legitimacy of the mass media arena and did so by depositing the issue in yet another arena, the law, with its own characteristic discourse.<sup>83</sup> The law has certain similarities to science, in that it is ostensibly neutral but has built-in biases in favour of certain interests, such as parties that have more money. While an assessment of the media and the law as platforms for adjudicating scientific disputes is outside the ambit of this paper, it is enough to point out that shifting the struggle to the legal domain can be seen as a means of enrolling an entire institutional structure.

It should be obvious from this account that types of partisanship are far from distinct. Partisanship can be unintended or intended; it can be associated with the methods used, the topics chosen, the conclusions reached, the style in which findings are couched and the audiences to which they are imparted, among other factors. Nor does partisanship associated with these different facets of research always serve the same cause: conflicts and contradictions are to be expected.

Some science studies scholars may believe that partisanship is incompatible with impartiality and symmetry, two of the four tenets of the Strong Programme (SP) in the sociology of scientific knowledge (SSK).<sup>84</sup> It is worth quoting David Bloor's original formulation of these tenets. According to Bloor, SSK should 'be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation'. Also, it should 'be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs'.<sup>85</sup> It is obvious from these statements, and from Bloor's discussion, that 'impartiality' and 'symmetry' apply to *explanations of beliefs*. The method of the SP analyst should be to use the same sorts of explanations to explain (what are taken to be) different sorts of beliefs. These tenets say nothing about the personal beliefs or engagement of the analyst. Some social analysts may interpret the SP as implying a need for neutrality — namely, not supporting one belief over another. But this does not follow

from the tenets of impartiality and symmetry themselves, which can also be interpreted as compatible with partisanship. One can both explain a belief *and* support it.

There is a certain irony in the situation here. Bloor modelled the SP on science itself: 'it will embody the same values which are taken for granted in other scientific disciplines'.<sup>86</sup> In practice, scientists are highly partisan; it can even be argued that partisanship is necessary for science to operate.<sup>87</sup> If SSK is to model itself on scientific practice rather than on idealizations of science, then partisanship should be expected rather than avoided. Furthermore, if SSK is to be reflexive, it should be acknowledged that sociologists of scientific knowledge have often been partisan concerning their views about SSK itself.

## **Conclusion**

Although potentially there are many insights and social benefits to be gained by intervention in science studies, my aim is certainly not to argue for intervention as an inherently superior approach. Rather, it is to say that it should be recognized as an approach that is useful for certain purposes. It can provide insights unavailable through other methods, but, at the same time, it can also preclude certain insights. Intervention should be recognized and used as part of the repertoire of social scientists studying science. Different types and degrees of involvement and intervention by the social analyst each have characteristic advantages and disadvantages. The implication is that the science studies community should support a variety of noninvolvements and involvements.

When should intervention be used? Among other criteria, decisions about science studies methods can be made on the basis of their value to social science, to science and to society.<sup>88</sup> Judgements in each case are likely to be contested. In the case of the polio-vaccines-AIDS theory, I decided to intervene for several reasons: because intervention has so seldom been used in social studies of science and therefore there were insights to be gained; because I believe challenges need to be made to the scientific establishment's frequent rejection of theories threatening to powerful interests; and, not least, because this particular theory has serious and wide-ranging social implications that deserve attention. This is clearly a position based on values: social

scientists, by using their own insights and commitments, can use intervention to seek to change science and society.

Some analysts may protest that it is inappropriate to impose their values: their social analyses should be kept separate from their social commitments. I believe that this stance is misleading. Even a stance of detachment involves a value choice, namely not to intervene.

Throughout much of this paper I have emphasized the contributions of intervention to social science understanding. But social science understanding cannot be separated entirely from benefits and costs to different groups in society. Knowledge is never socially neutral, either in its origins or in its potential applications. It is possible to argue that a judgement about intervention should always be made, ultimately, on the criterion of benefit to society, just as doctors' surgical interventions should be judged by their benefits to patients, present and future. In both cases, needless to say, there are difficult ethical issues. These issues are not peculiar to interventionist social analysis; they are only more obvious here. They are common to all social science.

## • NOTES

I thank Tom Curtis, Blaine Elswood and Louis Pascal for their advice and support. Malcolm Ashmore, Stan Aungles, Gabriele Bammer, Sharon Beder, Harry Collins, Tom Curtis, David Edge, Blaine Elswood, Steve Epstein, Richard Gosden, David Hess, Louis Pascal, Stewart Russell, Pam Scott and anonymous referees provided valuable comments on earlier drafts of this paper.

1. One possible reason for this is that most sociologists of science, whether positivist or relativist, basically support science as it exists: see Sal Restivo, 'Modern Science as a Social Problem', *Social Problems*, Vol. 35 (June 1988), 206–25, at 207–08.

2. Brian Martin, 'The Scientific Straightjacket: The Power Structure of Science and the Suppression of Environmental Scholarship', *Ecologist*, Vol. 11 (January–February 1981), 33–43; Richard Routley and Val Plumwood, 'The "Fight for the Forests" Affair', in Martin, C.M. Ann Baker, Clyde Manwell and Cedric Pugh (eds), *Intellectual Suppression: Australian Case Histories, Analysis and Responses* (Sydney: Angus & Robertson, 1986), 70–73. Richard Sylvan's previous name was Richard Routley.

3. M.D. Grmek, *History of AIDS: Emergence and Origin of a Modern Pandemic* (Princeton, NJ: Princeton University Press, 1990); D.B. Hrды, 'Cultural Practices Contributing to the Transmission of Human Immunodeficiency Virus in Africa',



*Reviews of Infectious Diseases*, Vol. 9 (1987), 1109–19; A. Karpas, 'Origin and Spread of AIDS', *Nature*, Vol. 348 (13 December 1990), 578.

4. An accessible discussion of these theories, including ones involving polio vaccines, is given by Jared Diamond, 'The Mysterious Origin of AIDS', *Natural History* (September 1992), 24–29.

5. In live virus vaccines, a strain of virus is chosen that does not cause symptoms in humans. This is called an attenuated strain. Contrary to what the word 'attenuated' might suggest, the virus is not weakened by chemical treatment, or in any other way. I thank Louis Pascal (personal communication, 15 December 1993) for clarification on this point.

6. K. Shah and N. Nathanson, 'Human Exposure to SV40: Review and Comment', *American Journal of Epidemiology*, Vol. 103 (1976), 1–12.

7. Louis Pascal, *What Happens When Science Goes Bad: The Corruption of Science and the Origin of AIDS: A Study in Spontaneous Generation*, Science and Technology Analysis Working Paper No. 9 (Wollongong: Department of Science and Technology Studies, University of Wollongong, NSW 2522, Australia, December 1991).

8. My introduction to Pascal's paper (Brian Martin, 'Introduction to Louis Pascal's article', in Pascal, op. cit. note 7, 1–2, at 2) includes the following:

Perhaps it is time for social analysts of science, rather than just studying the way things are, to contribute to a changed communication pattern. Since social analysts routinely make judgements about interests associated with a position or about whether a particular perspective is worth studying — and hence make de facto judgements about claims about scientific knowledge — it is a short step to the open promotion of the consideration of particular scientific ideas.

Another way to justify this working paper is to argue that interventions into the scientific communication system provide a fruitful way to study the system. For this reason, I look forward to your response to this publication.

9. A detailed treatment is given by Steven Epstein, *Impure Science: AIDS, Activism, and the Politics of Knowledge* (unpublished doctoral dissertation, University of California, Berkeley, 1993). There are a large number of unorthodox theories concerning AIDS. A useful survey is given by Robert Lederer, 'Origin and Spread of AIDS: Is the West Responsible?', *CovertAction Information Bulletin*, Vol. 28 (1987), 43–54, and Vol. 29 (1988), 52–65. Steve Epstein has pointed out to me (personal communication, 11 December 1993) an aspect of my own partisanship, namely not mentioning theories of AIDS originating from other vaccines such as smallpox and malaria.

10. The work and experiences of Lecatsas and Alexander could have provided an alternative entry point into the controversy. The main differences are that they were established professionals in the field and that their treatments of the theory are much briefer. Their first published discussion of the theory was in one paragraph out of a six-paragraph letter to the editor: G. Lecatsas and J.J. Alexander, 'Safe Testing of Poliovirus Vaccine and the Origin of HIV Infection in Man', *South African Medical Journal*, Vol. 76 (21 October 1989), 451.

11. Like Pascal, Lecatsas and Alexander had difficulty in publishing their views. According to Pascal, Lecatsas tried over a period of three years to publish in *Nature*, the *British Medical Journal* and *New Scientist*. Lecatsas wrote to Pascal to say: 'All were rejected with negative and often nonsensical comments' (Pascal, op.

cit. note 7, 43). See also the response to W.D. Hamilton's submission to *Science*, discussed later.

12. An excellent discussion, with examples, is given by David F. Horrobin, 'The Philosophical Basis of Peer Review and the Suppression of Innovation', *Journal of the American Medical Association*, Vol. 263 (9 March 1990), 1438–41. More generally, see Roy Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, *Sociological Review Monograph No. 27* (Keele, Staffs: University of Keele, 1979). On particularism versus universalism in science, see Stephen Cole, *Making Science: Between Nature and Society* (Cambridge, MA: Harvard University Press, 1992).

13. Malcolm Ashmore, as referee of this paper, commented in relation to this statement that *scientific* merit is 'an honorific status granted or withheld by relevant members of the scientific community' and that a social analyst should not endorse or refuse to endorse claims on this basis, but rather to socially analyze them. To this I would respond in two ways. First, it is impossible to fully separate 'scientific' and 'nonscientific' issues. Whenever they make any judgements at all about an issue, social analysts are involved in making judgements, at some level and to some degree, about 'scientific merits'. This is certainly the case in the present example, in which social factors are obviously involved in the choice, justification and endorsement of evidence and theories. Second, there is never a complete disjuncture between the craft skills of those who are acknowledged as scientists and as specialists in particular fields and those who are outside what is called the 'scientific community'. For example, although my expertise in biology is limited, I have worked with probability theory and exponential growth and therefore am able to judge (even if only at a basic level) claims by Pascal and others about probabilities of transmitting SIV to humans, growth in AIDS cases, and the like, which are an important part of the argument. All social scientists have *some* understanding of science, and this is deployed routinely — although not always acknowledged — in selecting topics for study. Among typical modern understandings are notions such as the irreversibility of death, the rotation of the earth and the conversion of electrical energy into light and heat. Such common understandings provide a likely partial explanation for why there have been few science studies treatments, symmetrical or otherwise, of claims that seem ludicrous in terms of understandings of science that most children receive, such as that Elvis has appeared in various parts of the world in recent years, that the moon landings were elaborate frauds, or that the world will end on 27 May 1997. This is not to say that these ideas are right or wrong, merely that social scientists commonly make their own judgements about their scientific merit in helping to decide whether they are worthy areas for science studies and whether they deserve more examination than has so far occurred. These examples are not entirely whimsical. See, respectively, Raymond A. Moody, Jr, *Elvis after Life: Unusual Psychic Experiences Surrounding the Death of a Superstar* (Atlanta, GA: Peachtree, 1988); Rogier van Bakel, 'The Wrong Stuff', *Wired*, Vol. 2 (September 1994), 108–13, 155; Stanley Young, 'The End: Countdown to the Millenium', in Ted Schultz (ed.), *The Fringes of Reason: A Whole Earth Catalog* (New York: Harmony Books, 1989), 8–21.

14. In the science studies literature, the most useful warrant for this review is Paul Feyerabend's epistemological anarchism: see Feyerabend, *Science in a Free Society* (London: New Left Books, 1978).

15. Pascal, op. cit. note 7.

16. For example, among the scientists were Richard Dawkins, Paul Ehrlich, Luc Montagnier and Linus Pauling.

17. We avoided this when confidentiality was requested or seemed indicated.

18. For his work in evolutionary theory, in 1992 and 1993, Hamilton won the Wander Prize, the Crafoord Prize and the Kyoto Prize. The last two of these are intended to be of equivalent status to a Nobel Prize, but in areas which no Nobel Prize covers.

19. Among other things, May is the author of hundreds of scientific papers, a member of the American National Academy of Science and simultaneously a Royal Society professor at both Oxford and London Universities. May, when in the Department of Theoretical Physics at the University of Sydney in the early 1970s, was my initial PhD supervisor. (We have each changed fields!) Following May's letter to me about Pascal's paper, we maintained a cordial correspondence though disagreeing strongly concerning the operation of peer review.

20. See Lecatsas & Alexander, *op. cit.* note 10.

21. I sent the paper to correspondents at eight newspapers and the Australian Associated Press; only some of these had specialist science journalists at the time.

22. Julian Cribb, 'Was This Science's Biggest Blunder?', *Weekend Australian* (25–26 April 1992), Review, 1–2.

23. Personal communication, 18 January 1994. See also Julian Cribb, 'Research Fails to Nail AIDS Virus', *Australian* (22 December 1993), 16.

24. Raanon Gillon, 'A Startling 19,000-word Thesis on the Origin of AIDS: Should the *JME* Have Published It?', *Journal of Medical Ethics*, Vol. 18 (1992), 3–4.

25. *Nexus*, Vol. 2. No. 6 (January–February 1992), 49. *Nexus* is published from PO Box 30, Mapleton, Queensland 4560, Australia (and lists offices in the USA, UK and The Netherlands).

26. Tom Curtis, 'The Origin of AIDS', *Rolling Stone*, No. 626 (19 March 1992), 54–61, 106, 108. Despite its publication date, this issue was distributed at the end of February.

27. Their paper was eventually published as a letter: Blaine F. Elswood and Raphael B. Stricker, 'Polio Vaccines and the Origin of AIDS', *Research in Virology*, Vol. 114 (1993), 175–77. The full version was published as Elswood and Stricker, 'Polio Vaccines and the Origin of AIDS', *Medical Hypotheses*, Vol. 42 (1994), 347–54; Elswood and Stricker, 'Polio Vaccines and the Origin of AIDS: Clarification', *ibid.*, Vol. 44 (1995), 226.

28. *Science's* treatment began with Jon Cohen, 'Debate on AIDS Origin: *Rolling Stone* Weighs in', *Science*, Vol. 255 (20 March 1992), 1505. Curtis commented on this article: Tom Curtis, letter, *Science*, Vol. 256 (29 May 1992), 1260, stimulating Hilary Koprowski, letter, *Science*, Vol. 257 (21 August 1992), 1024–27. *Science* did not publish Curtis's reply to Koprowski.

29. Andrew Tyler, 'Monkey Business', *The Independent Magazine* (London, 19 September 1992), 24–29.

30. Brian Martin, 'Peer Review and the Origin of AIDS: A Case Study in Rejected Ideas', *BioScience*, Vol. 43 (October 1993), 642–27; see also Martin, 'Polio Vaccines and the Origin of AIDS: The Career of a Threatening Idea', *Townsend Letter for Doctors*, No. 126 (January 1994), 97–100. The *Townsend Letter* is published from 911 Tyler Street, Port Townsend, WA 98368–6541, USA.

31. C. Basilio, C. Buck, R. Desrosiers, D. Ho, F. Lilly and E. Wimmer,

Report from the AIDS/Poliovirus Advisory Committee (unpublished typescript, 18 September 1992); copies available from me.

32. 'Panel Nixes Congo Trials as AIDS Source', *Science*, Vol. 258 (30 October 1992), 738; Tom Curtis, letter, *Science*, Vol. 259 (1 January 1993), 14.

33. Curtis told me (12 January 1994) that Koprowski later provided some other documents which were protected from public disclosure by a confidentiality order.

34. The electronic mailing list 'sci-tech-studies' was run by listserv@ucsd.edu, and now is run by listserv@kasey.umkc.edu

35. Brian Martin, correspondence, *Nature*, Vol. 363 (20 May 1993), 202.

36. '“Origin of AIDS” Update', *Rolling Stone*, No. 671 (9 December 1993), 39.

37. Elswood (personal communication, 30 November 1993), on the other hand, believes that Koprowski settled because he did not want to answer questions under oath about certain aspects of his vaccine and the trials of it in Africa.

38. Martin (1993), *op. cit.* note 30.

39. Henk Hanssen, 'Hebben Artsen AIDS Veroorzaakt?', *Paranorma* (Haarlem, The Netherlands, 11–18 November 1993), 42–44. I also informed this journalist about other sources of information.

40. It is worth noting that the arguments can be investigated by studying various medical reports and other documents, by interviewing participants in the 1950s' polio vaccine campaigns, by testing seed stocks of 1950s' polio vaccine for SIVs, by evaluating the types and patterns of SIVs in monkeys and humans, and various other measures.

41. Published in Martin (1993), *op. cit.* note 30.

42. In his letter to me of 4 February 1994, the letterhead reads: 'The Medical-Legal-Foundation' (Dedicated to truth in public policy), 1564-A Fitzgerald Dr., Suite #240, Pinole, California 94564, [phone] 510-724-0406. The letter is signed 'Dr John Roberts, President'.

43. See Peter Korn, 'The New AIDS Mystery', *Redbook* (New York, July 1994), 80–83, 106, 108.

44. The information that I provided to Roberts was not confidential. Via the information in my *BioScience* article, it would be straightforward to track down Pascal, Elswood, Curtis, Lecatsas, Alexander and Hamilton. My letter to Roberts simply made it somewhat easier for him to contact them.

45. The standard package included Pascal's *What Happens When Science Goes Bad* (*op. cit.* note 7), Curtis's article from *Rolling Stone* (*op. cit.* note 26), my articles in *BioScience* and *Townsend Letter for Doctors* (*op. cit.* note 30), W.D. Hamilton's personal letter to the editor of *Science* requesting a reconsideration of his rejected letter about the theory, Koprowski's 1992 letter in *Science* (*op. cit.* note 28), Pascal's unpublished 'Carelessness with Human Lives' (a rebuttal of Koprowski's letter), Elswood and Stricker's *Medical Hypotheses* paper (*op. cit.* note 27), and a list of key publications in the area that I had compiled.

46. Grmek, *op. cit.* note 3.

47. Steve Connor, 'World's First Aids Case Was False', *The Independent* (London, 24 March 1995), 1; Connor, 'How Scientists Discovered False Evidence on the World's "First Aids Victim"', *ibid.*, 2–3.

48. Gerald Corbitt, Andrew S. Bailey and George Williams, 'HIV Infection in Manchester, 1959', *Lancet*, Vol. 336 (7 July 1990), 51.

49. Basilico et al., *op. cit.* note 31, 4: '... he [the Manchester man who died of AIDS in 1959] had returned to England by the first half of 1957 before the Congo

trial was begun. Therefore, it can be stated with almost complete certainty that the large poliovaccine trial begun late in 1957 in Congo was not the origin of AIDS'.

50. The published technical account is Tuofu Zhu and David D. Ho, 'Was HIV Present in 1959?', *Nature*, Vol. 374 (6 April 1995), 503–04.

51. One is that the co-authors of the original study (op. cit. note 48), Gerald Corbitt, is reported as admitting that he knew in 1992 that the Manchester man could not have died of AIDS but saw no reason to publish this finding since his letter to *Lancet* 'was not intended as "a peer-reviewed article" on the origin of AIDS': Ehsan Masood, 'Anomaly Admitted in "First" AIDS Case', *Nature*, Vol. 375 (4 May 1995), 4. Another is that David Ho, who exposed the flaws in the evidence for the Manchester man having AIDS, was a member of the Wistar Institute Committee (op. cit. note 31).

52. Steve Connor, 'Rethink Begins into How the Virus Originated', *The Independent* (London, 24 March 1995), 3.

53. According to Pascal, the case of the Manchester man should never have been considered a decisive argument against the theory in any case; see Pascal, 'Carelessness . . .', op. cit. note 45.

54. All accounts of interactions are reconstructions, of course. The point here is that reconstructions during the events are likely to be different from those later on, and that comments made to a participant during a campaign are likely to be different from comments made during a formal interview.

55. His general position would be known to others through my own article in *BioScience*.

56. I thank Richard Gosden for suggesting this latter point.

57. This does not mean that partisans are ignorant of the other side's position. The published literature is monitored by each side. Among those requesting a copy of Pascal's paper was the director of a division at the National Center for Infectious Diseases, Atlanta, Georgia.

58. The only significant published discussions by critics are Koprowski's letter in *Science* (op. cit. note 28), an editorial response to Elswood & Stricker's letter in *Research in Virology* (op. cit. note 27) and an attack on Lecatsas & Alexander: B.D. Schoub, C.J. Dommann and S.F. Lyons, 'Safety of Live Oral Poliovirus Vaccine and the Origin of HIV Infection in Man', *South African Medical Journal*, Vol. 77 (6 January 1990), 51–52.

59. Martin, op. cit. note 30.

60. Cribb, op. cit. note 22.

61. 'Hypothesis on the Origin of AIDS', *Green Synthesis*, No. 35 (March 1992), 13–14.

62. It would be possible to imagine *another* social analyst collecting all my materials, and others, and proceeding to undertake a (noninterventionist) examination of the issue. This rather begs the question of the insights made available by my own intervention.

63. Some other purposes and effects are conforming to peer expectations, forging collaborations, attacking opponents and having fun. Needless to say, few of these are mutually exclusive.

64. It is relatively easy to determine the stated intent of the analyst. Attempting to determine the 'real' or subjective intent of the analyst raises difficult issues of psychology, the attribution of interests and so forth. Career advancement of the analyst is almost never the stated intent of a study, although arguably it is a

common effect. Informal conversations attest to the important role of career interests in most academic work, and career advancement is widely recognized as an effect of scholarly work. That this issue has so seldom been studied or even mentioned in the science studies literature points to a major gap in the application of reflexivity in the sociology of scientific knowledge.

65. I believe that my own understanding has been increased, but whether my writings and talks have aided the understanding of others is difficult for me to assess. As for career advancement, again it is difficult for me to judge the effect of studying this issue.

66. Leon Festinger, Henry W. Riecken and Stanley Schachter, *When Prophecy Fails: A Social and Psychological Study of a Modern Group that Predicted the Destruction of the World* (Minneapolis, MN: University of Minnesota Press, 1956).

67. H.M. Collins, 'Researching Spoonbending: Concepts and Practice of Participatory Fieldwork', in Colin Bell and Helen Roberts (eds), *Social Researching: Politics, Problems, Practice* (London: Routledge & Kegan Paul, 1984), 54–69. I have drawn on Collins' article in my accounts for both the usual conceptualization and participant comprehension, and I thank him for emphatically drawing my attention to his work.

68. H.M. Collins and T.J. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge & Kegan Paul, 1982).

69. The most well-known account of this sort is James D. Watson, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (London: Weidenfeld & Nicolson, 1968). Another excellent account of science on the inside, jointly written by a journalist and a scientist, is Lydia Dotto and Harold Schiff, *The Ozone War* (Garden City, NY: Doubleday, 1978). When I was a scientist and an active participant in the debate over nuclear power, I wrote some analyses: B. Martin, *Nuclear Knights* (Canberra: Rupert Public Interest Movement, 1980); Martin, 'The Naked Experts', *Ecologist*, Vol. 12 (July/August 1982), 149–57. Whether such studies are considered to be social science is another question. In technology studies, an excellent insider analysis is Lous L. Bucciarelli, *Designing Engineers* (Cambridge, MA: MIT Press, 1994).

70. William Foote Whyte (ed.), *Participatory Action Research* (Newbury Park, CA: Sage, 1991).

71. Stephen Kemmis and Robin McTaggart (eds), *The Action Research Planner* (Geelong, Victoria: Deakin University, 3rd edn, 1988). I thank Chris Fox for pointing me to this literature.

72. See, for example, F.E. Emery and E. Thorsrud, *Democracy at Work* (Leiden: Martinus Nijhoff, 1976); Trevor Williams, *Learning to Manage our Futures: The Participative Redesign of Societies in Turbulent Transition* (New York: Wiley, 1982).

73. See especially Alain Touraine, *The Voice and the Eye: An Analysis of Social Movements* (Cambridge: Cambridge University Press, 1981). For a perceptive analysis of the relations between social analysts and social movements, see C.A. Rootes, 'Theory of Social Movements: Theory for Social Movements?', *Philosophy and Social Action*, Vol. 16, No. 4 (October–December 1990), 5–17.

74. David J. Hess, *Science and Technology in a Multicultural World: The Cultural Politics of Facts and Artifacts* (New York: Columbia University Press, 1995), Chapter 8; Robert A. Rubinstein, 'Reflections on Action Anthropology:

*Some Developmental Dynamics of an Anthropological Tradition*, *Human Organization*, Vol. 45, No. 3 (Fall 1986), 270–79. I thank David Hess for referring me to this literature.

75. Sharon Beder, *Toxic Fish and Sewer Surfing: How Deceit and Collusion Are Destroying our Great Beaches* (Sydney: Allen & Unwin, 1989). She became more open about her partisanship — as in this book — after completing her research. See also Beder, 'Sewerage Treatment and the Engineering Establishment', in B. Martin (ed.), *Confronting the Experts* (Albany, NY: State University of New York Press, 1996), in press; and Beder, 'Controversy and Closure: Sydney's Beaches in Crisis', *Social Studies of Science*, Vol. 21 (1991), 223–56.

76. Martin, op. cit. note 69.

77. Gabriele Bammer and B. Martin, 'Repetition Strain Injury in Australia: Medical Knowledge, Social Movement, and De Facto Partisanship', *Social Problems*, Vol. 39 (1992), 219–37.

78. Pam Scott, Evelleen Richards and B. Martin, 'Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies', *Science, Technology, & Human Values*, Vol. 15, No. 4 (Autumn 1990), 474–94. For discussion, see Malcolm Ashmore, 'Ending Up On the Wrong Side: Must the Two Forms of Radicalism Always Be at War?', *Social Studies of Science*, Vol. 26, No. 2 (May 1996), 305–22, this issue; H.M. Collins, 'Captives and Victims: Comment on Scott, Richards, and Martin', *Science, Technology, & Human Values*, Vol. 16, No. 2 (Spring 1991), 249–51; Collins, 'In Praise of Futile Gestures: How Scientific is the Sociology of Scientific Knowledge?', *Social Studies of Science*, Vol. 26, No. 2 (May 1996), 229–44, this issue; Martin, Richards and Scott, 'Who's a Captive? Who's a Victim? Response to Collin's Method Talk', *Science, Technology, & Human Values*, Vol. 16, No. 2 (Spring 1991), 252–55.

79. Scott et al., op. cit. note 78.

80. David J. Hess, *Science in the New Age: The Paranormal, Its Defenders and Debunkers, and American Culture* (Madison, WI: University of Wisconsin Press, 1993). Hess makes the important point that there may be multiple 'sides' to any debate and that a whole range of attempts at 'capture' may occur. The idea of 'capture' has obvious affinities with that of 'enrolment' used in actor-network studies of science-technology-society (see, for example, Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* [Milton Keynes, Bucks.: Open University Press, 1987]); in these terms, 'capture' refers to the specific enrolment of the analyst.

81. See also Dick Pels' theory of 'third positions': Pels, 'The Politics of Symmetry', *Social Studies of Science*, Vol. 26, No. 2 (May 1996), 277–304, this issue. I thank Malcolm Ashmore for making this connection.

82. I thank David Hess for suggesting this line of thought.

83. Shifting the arena from public debate to the courts is the aim of what have been dubbed 'strategic lawsuits against public participation' or SLAPPs: see Penelope Canan and George W. Pring, 'Strategic Lawsuits Against Public Participation', *Social Problems*, Vol. 35, No. 5 (December 1988), 506–19; Pring and Canan, '“SLAPPs” — “Strategic Lawsuits Against Public Participation” in Government — Diagnosis and Treatment of the Newest Civil Rights Abuse', *Civil Rights Litigation and Attorney Fees Annual Handbook*, Vol. 9 (1993), 379–406. I pointed to a connection between Koprowski's legal action against Tom Curtis and *Rolling Stone* and SLAPPs in my letter to *Nature*, op. cit. note 35.

84. One referee of this paper stated that 'The concept of being partisan is in direct contravention of symmetry and seems to be contingent on hidden assumptions of truth and correctness . . .'. The other two tenets of the Strong Programme are causality and reflexivity.

85. David Bloor, *Knowledge and Social Imagery* (London: Routledge & Kegan Paul, 1976), 5.

86. *Ibid.*, 4.

87. Ian I. Mitroff, *The Subjective Side of Science: A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists* (Amsterdam: Elsevier, 1974).

88. There are various ways to assess these, of course. For example, 'value to social science' might mean anything from value to the career of a social scientist to expanding the scope of social theory.

**Brian Martin** has carried out research on scientific controversies, suppression of intellectual dissent, non-violent alternatives to military defence, and information in a free society.

**Author's address:** Department of Science and Technology Studies, University of Wollongong, New South Wales 2522, Australia. Fax: +61 42 213452; e-mail: b.martin@uow.edu.au