during the preceding five years. The awardee, who should be a member of SKAT during the year in which the award is given, will be honored at the ASA annual convention in San Francisco in August 1998. The deadline for nominations is **April 1, 1998.** For each nomination please send a brief letter identifying the work(s) to be considered, their publisher(s) (presses or journals), and any supporting materials that would help the committee understand the contribution (for example, published book reviews). Please send material to Edward J. Hackett, STS Program, Room 995, NSF, 4201 Wilson Blvd., Arlington, VA 22230 [email: ehackett@nsf.gov].

SKAT, the ASA section on Science, Knowledge, and Technology, invites nominations (including self-nominations) for the Hacker-Mullins Award. The award is given to a **graduate student** for a published article or unpublished paper concerning science, knowledge and technology that was completed during the preceding twelve months. Students are especially encouraged to nominate their own papers for this award. The award, which includes a cash prize, will be presented during the SKAT business meeting at the ASA annual convention in San Francisco in August 1998. The deadline for nominations is **June 1, 1998.** For each nomination please send a copy of the work nominated to Edward J. Hackett, STS Program, Room 995, NSF, 4201 Wilson Blvd., Arlington, VA 22230 [email: ehackett@nsf.gov].

Centre for Technology and Society in Trondheim evaluated. The STS unit at Trondheim University is well known to many readers of Technoscience. Recently in connection with the fusion of a number of different higher educational and research institutions into the new Norwegian University of Science and Technology, the Senter for teknologi og samfunn (STS) was evaluated. The purpose was to assess the scientific merit of the unit and delineate its role in the new technological university system. The Norwegian University of Science and Technology (NTNU) is a national facility that now includes humanities and social sciences faculties. The STS unit, created in 1988, is now recommended to continue in the new context to provide input to the higher education of engineers and natural scientists. The evaluation, carried out by a panel of three persons (Dr. Tarja Cronberg, Finland, Prof. Aant Elzinga, Sweden and Prof. Jon Gulowsen, Norway), concludes that the STS unit in Trondheim has been successful in creating a unique platform with regard to the sociology of technology and developing an approach which may be termed "pragmatic constructivism". The history of technology has followed a more traditional path in Trondheim, but here too scholarship is for the most part of a high quality. To a lesser extent the Centre has been involved in technology policy studies and technology assessment, but the panel sees a future potential in this field as well as in science studies, which is not so well represented compared to technology studies. It is found that the Centre in particular has contributed significantly within studies of technology and everyday life and gender related studies. Overall the empirical orientation of the centre is seen as a strength. One of the alternatives for the future of the Centre has been to close it down and integrate existing personnel into disciplinary departments, which lead a more stable existence since they have regular funding through a faculty structure which the STS unit does not. The panel strongly advises against this alternative, and suggests instead an upgrading of the status of the unit within NTNU. It is already clear that NTNU will retain the STS unit, but in what form is yet to be decided. Anyone interested in the **evaluation report**, which is about 40 pages plus an extensive bibliography that covers a wide range of publications coming from the Trondheim group, may write to Centre for Technology and Society, Norwegian University of Science and Technology (NTNU), 7055 Dragvoll, Norway.

FIELDNOTES

Captivity and Commitment
By Brian Martin

The May 1996 special issue of Social Studies of Science on the politics of SSK provides much intriguing commentary. Here I focus on two common though unstated threads: first, a resistance to generalization, specifically about becoming a "captive of controversy"; second, a distrust of overt commitment.

Captivity

Quite a few authors commented on the findings of Pam Scott, Evelleen Richards and myself (SRM) in our paper "Captives of controversy". This paper spelled out three main conclusions. The first was that "sociological studies of contemporary controversies must be viewed as potential resources in social struggles over scientific or technical knowledge claims". No one seems to have questioned this point; it is affirmed by Sheila Jasanoff. The third conclusion was that "intervention by the analyst perturbs the dispute". Again, there has been little questioning of this point. By contrast, the second conclusion has been both disputed and misinterpreted. It has two parts:

(2a) "an epistemologically symmetrical analysis of a controversy is almost always more useful to the side with less scientific credibility or cognitive authority"; (2b) the "side with fewer scientifically or socially credentialed resources is more likely to attempt to enroll the researcher". We reached this conclusion as a result of our experiences in studying three controversies and also found it compatible with theoretical expectations. Note that neither 2a nor 2b posits an ironclad rule: 2a contains the phrase "is almost always more useful"; 2b contains "is more likely".

A number of commentators seem to have difficulty grasping that SRM's conclusions are less about what the researcher does and more about what the "subjects" (participants in the controversy studied) do. SRM make no major point about whether the analyst should be neutral or take sides. The paper is about how social analysts and their work are used by participants in controversies, whatever the intent of the analysts.

Harry Collins says "SRM argued that one ought, therefore, to commit oneself to the weaker side" and that according to SRM, the analyst "was bound to be captured by the underdogs". The first claim misrepresents SRM and the second replaces SRM's likelihood with a claim of certainty. Dick Pels says that, according to SRM, "neutrality is wrong in principle". This was not a central conclusion of SRM, though it might be extrapolated from SRM's position.

Malcolm Ashmore presents two hypothetical case studies "dying smokers sue a tobacco company, and Challenger's O-rings" to suggest that a symmetrical analysis can end up supporting the side with more money or power. Contrary to Ashmore, this is quite compatible with SRM's conclusions. SRM predicted that a symmetrical analysis would support the side with less epistemological authority. Tobacco companies have less epistemological authority, so a symmetrical analysis of a dispute between dying smokers and a tobacco company would quite likely help the tobacco company. The issue of the money and power of the company is secondary.

Brian Wynne refers to "SRM's argument that SSK practitioners should deliberately side with the underdogs". Again, this was not SRM's argument. Part of the confusion by Collins, Ashmore and Wynne may be due to their use of the expressions 'underdog' and 'overdog'. SRM did not use these terms, but rather referred to "the side with less scientific credibility or cognitive authority". This side may be an 'underdog' epistemologically but not necessarily in terms of power and money. Jasanoff says that symmetrical analysis of

scientific controversies, they [SRM] charge, necessarily strengthens the weaker party' and 'the analyst may as well embrace commitment from the start by choosing sides in the controversy. Again, this is a misreading of SRM.

If SRM's claim had been that a symmetrical analysis always helps the side with less epistemological authority, then just one counterexample would be sufficient to reject the generalization. SRM's actual claim is obtained by replacing 'always' by 'almost always'. To test it, an enumeration of cases is in order. Among others, consider:

* Bammer and Martin on repetition strain injury ;
* Collins and Pinch on parapsychology ;
* Martin on fluoridation ;
* Martin on nuclear winter ;
* Richards on vitamin C and cancer ;
* Scott on the Australian Animal Health Laboratory .

By my assessment, each of these cases is compatible with SRM's generalizations 2a and 2b and none goes directly against them.

Collins, Ashmore and Wynne raise a number of points about SRM's conclusions, such as that 'sides' in the controversy were not problematized and that it is impossible to know in advance how a social analysis will be used.

True enough. Whenever one makes a generalization, it is possible to quibble with concepts, propose hypothetical exceptions and raise picky objections. But it seems to me that there is a reasonable group of case studies showing the value of SRM's conclusions. Just because these generalizations do not hold in every conceivable case and are a bit rough on the edges does not mean that they are useless. I view them as a fairly reliable starting point when undertaking a controversy study. It is well known that scientists prefer to publish reports of original investigations rather than replications of previous work. Does the same apply in social science? Is it more attractive to criticize someone else's generalization than to undertake a study that is likely to confirm it?

Commitment

SRM did not say that analysts should make a commitment to a particular position yet, as noted above, several commentators have attributed such a claim to SRM. Furthermore, commitment is taken to be something inappropriate: attributing advocacy of commitment to SRM is taken as a criticism. It can be anticipated, then, that open advocacy of commitment is likely to encounter hostility.

In my paper 'The Critique of Science Becomes Academic', I presented an interpretation of social influences on the development of the academic critique of science, including SSK, essentially seeing it as shaped by academic imperatives. I pointed out that there is relatively little analysis in the field that addresses pressing social issues such as war, repression, poverty and patriarchy. I commented on the limitations of theory for aiding social action and provided some suggestions for a critique of science oriented to activists.

On the other hand, I did not say that everyone undertaking an analysis of science should address pressing social issues, nor that anyone is obliged to be committed to one particular side on an issue. The paper is a comment on the uselessness of much social analysis of science to social activists and some reasons why this is the case.

Collins says that in this article I argue for SSK practitioners to be committed, and then undertakes a critique of "commitment to Commitment". This was not my argument. I asked for more scholars "not all" to deal with pressing social issues. I presumed that most scholars studying torture technology, for example, would oppose its production, trade and use, though of course there are arguments on the other side. My assumption is that more attention to these issues will be a good thing. Collins also contested my interpretation of the history of SSK, arguing that SSK drew its inspiration from "academic questions" without any significant social contextual shaping.

Pels, as part of his sophisticated critique of the use of symmetry in SSK, argues the case for a "third position" beyond the "neutralists" such as Collins and the 'politicals' such as myself. He states that "professional autonomy and the institutional distance it measures out remain a crucially important precondition for any kind of serious critical work", both for science studies and science. This sounds good in principle, but what does it mean in practice? Few fields of scientific research can be said to be professionally autonomous, given extensive funding by militaries, governments and corporations plus the dependent position of scientists as employees. Does Pels' 'serious critical work' mean intellectual work? What is its relation to social action? Pels' paper is too abstract to provide clear answers to such practical questions.

Evelleen Richards says that 'Brian Martin has recently urged the obligatory politicization and commitment of all S&TS analysis'. This was not my argument: I offered suggestions for 'those who favor a more activist critique of science'. Certainly I did **not and could not** attempt to impose 'political correctness or activism' on colleagues, as Richards implies.

From the responses of several writers, it seems that it is risky for an SSK analyst to be seen as committed. SRM did not argue for commitment but are accused of doing so. I argued the value of addressing pressing social issues and am interpreted as arguing for 'commitment to commitment' and 'obligatory politicization'. Unfortunately, 'commitment' is not a very useful category to address the issues involved, since it is oriented to the psychology of the analyst. This is why SRM focused on capturing, a process in which the psychology of the analyst is not crucial, and why I focused on the relevance of science studies to 'crucial social issues'.

It is well known that scientists are highly committed to particular theories and methods, but that this apparent violation of objectivity is well disguised through standard portrayals of science. My informal meetings with science studies scholars show that they too are highly committed, variously to particular social causes, methodological positions, styles of discourse and, not least, their careers. As yet, though, acknowledging such commitments is not popular in spite of ample rhetoric about reflexivity.

In my critique of science studies, I commented that most work in scholarly journals 'is couched in an inaccessible academic style and deals with topics of peripheral interest' to activists. That applies to most of the contributions concerning "The Politics of SSK". This does not mean it is all a waste of time. I would never have bothered with my critique except that I know that there are quite a lot of science studies students and academics who are keen to be involved in current issues. This also applies to a number of the contributors who, whatever they consider their commitments, have played a prominent role in public debates about science. Whatever our disagreements, I am pleased to be part of a professional network in which people are concerned about the politics of their own work.

* NOTES
I thank Stewart Russell for helpful comments.

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