

AN INTERVENTIONIST PERSPECTIVE ON CONTROVERSY STUDIES, OR WHY NOT ALWAYS TO TAKE CONTROVERSIES AT FACE VALUE

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Frans A.J. Birrer

Mathematics, Computer Science and Society
P.O. Box 9512
2300 RA Leiden
Netherlands
E-mail Birrer@Rulwinw.Leidenuniv.nl

Chemistry and Society
P.O. Box 9502
2300 RA Leiden
Netherlands

Controversies have been a topic in science studies for some time now.¹ The interest in controversies has fluctuated over time, and the death of controversy studies was announced already in 1988 by Arie Rip²; like so many other fields in the social sciences, science studies sometimes appears to hover restlessly from one fashion to the next.³ In this article I would like to explore why and how Rip's prophecy about the death of controversy studies may be falsified.

Why are there controversies?

Controversies are a natural phenomenon in all domains of social life. Particularly where new insights and needs are in the process of being articulated and being developed, it is only natural that this does not immediately lead to completely balanced views or balanced practices. Discussion and negotiation are opportunities for parties to explore each others arguments, views and preferences, in a continuous reshaping of the understanding of each other as well as of themselves. In this way controversy serves as a learning process for participants.

¹ For a comprehensive work, see [Engelhardt,Caplan,1987].

² 'What there is to say about controversies, has been said. A few years ago, controversy studies, both in science and in society, were sought as a research site to show that there are always conflicting explanations, methods and options. That's no news any more, we have to go further', said Rip in an interview by Steenkamp and Van Eijk [1988].

³ 'The whole field is susceptible to fashions, also because it depends so strongly on outside money', Leydesdorff was quoted by Steenkamp and Van Eijk [1988].

It would of course be misleading to suggest that controversies are *always* so productive and fruitful. A controversy may drift into a much more static phase : each party is totally convinced of its own, undivided right; participants are not listening any more to the arguments from the other side, nor do they bother to address these arguments; positions are no longer open to any reconsideration or negotiation. The conflict gets stuck in a fixed, self-reinforcing state.

This can be illustrated by examining some very simplified abstracts from rather familiar types of controversies⁴ :

Example 1

A (expert) : 'Fear of danger from this new technology is irrational, for I am an expert on the field, and I tell you that there is no risk'

B (non-expert) : 'Yet I have fear that there are serious risks.'

The underlying issue here is of course the credibility of experts. The non-expert cannot verify the expert's claims, and experts have in the past often been proven wrong and biased. This issue, however, is not directly addressed. There is no exchange of arguments as to under which conditions and for which reasons experts may or may not be trusted. The expert often does not even recognize that the discussion goes beyond 'scientific' questions.⁵

Example 2

A (mathematical modeller of complex systems) : 'This model is the best model currently available, policy decisions must therefore be based on the outcomes of this model.'

B (counter-expert) : 'This model shows major inadequacies (as do other models in the field), therefore its outcomes are no adequate basis for decisions.'

Underneath here is the question who has the burden of proof. Again, it is not directly addressed. The discussants argue about the qualities of the model, not about who should bear the burden of proof and why. They thereby tend to overlook a major confusion between contexts, or discourses : even if a model is in a scientific sense the best currently available, this does not imply that its outcomes are the only outcomes to take into account in practical decisions. Often, different models can be built with different outcomes, models which are only slightly less plausible than the 'best' model. For a scientist it might be a good bet to work on the most promising one; but in real life one should anticipate other outcomes too than the ones predicted by this most promising model. For the consequences of being wrong in

⁴ Cf. [Birrer,1990] for some general analysis.

⁵ Cf. [Birrer,1991b].

academic science are very different from the consequences of being wrong in real life.⁶⁷

Example 3

A (expert proponent of new technology) : 'There is no positive scientific evidence that this new technology will pose risks, so science tells us that there is no risk.'

B (opponent) : 'Our knowledge is yet so incomplete, that we cannot positively show there is no risk; therefore there is risk.'

Like in the previous example, the question of burden of proof is underlying the conflict, and again there is confusion of discourses : the expert proponent treats risk as a scientific most probable estimate, but in real life less plausible outcomes may have such severe consequences that they are crucial nevertheless.⁸

Example 4

A : 'For the present, we must assume that all human (cognitive) activities in principle can be taken over by computers, for there is no activity for which we can prove the opposite.'

B : 'The assumption that all human activities can eventually be taken over by computers must presently be considered false, for there are activities for which we cannot imagine how they can be done by computers.'

This is a typical case where each side 'proves' its case by showing that the other side is wrong. In fact both sides are overstating their case : the thesis that all human activities could in principle be done by computers can neither be proven nor disproved at this time. Again, a hypothesis is absolutised by the researcher as being doubtlessly true.⁹

So controversies often get into a phase where the proponents are not entering into each other's arguments any more, and where the arguments from both sides, taken literally, are not even addressing the same issue. We cannot take such controversies merely at face value, the core

⁶ This context of 'real life' is similar to what Weingart [1988] has called 'the context of relevance'.

⁷ Cf. [Birrer,1986].

⁸ This example is very similar to example 2. It is taken from current work with Rob Pranger on biotechnology. Recently Van Schomberg [1992] discusses examples close to our nr. 3, but with a rather different interpretation. He relates it to a philosophical distinction between 'theoretical empirical discussion' (about truth, where arguments in principle have consensus-enforcing power) and 'epistemic discussion' (about plausibility). I do not see how this distinction can be applied to actual statements made in discussions. I would say e.g. that plausibility arguments play a role in *both* A's and B's statements; the point is rather that they originate from different *contexts*, in which the relevance of possible outcomes is entirely different.

⁹ Cf. [Birrer,1986].

of the conflict is underneath of what is actually said.¹⁰ The examples also show that, taken literally, claims are sometimes outright wrong.

Summarizing so far : a major aspect for understanding controversies is their role as potential learning process, which may become blocked when the controversy enters into a rigidified state. It is therefore appropriate to ask to what degree various approaches are able to understand (scientific) controversy as learning process (or as blocked learning). Let us start to examine this question by contrasting two very schematically defined types of approach.

Classical versus modern approaches to controversies

The classical approach to science is based on the idea that in a controversy at least one of the parties must be wrong. We may not be able to decide in any controversy on the basis of current results only, but with more research we will eventually find ultimate answers to the questions the controversy was about. In the classical perspective, the history of science looks like a one-directional progression towards the truth. There is a very strong learning perspective in the classical approach, so strong in fact that it is *too* strong. For who shall decide what is right and wrong, and on what grounds? Moreover, how can any application of the current state of knowledge do justice to a historical situation, in which the state of scientific knowledge was quite different? As a reaction, science studies (amongst others) developed a quite different perspective, which shall be called the modern approach.

Modern perspectives on science are based on a considerable degree of pluralism and relativism. It is assumed that there are many different rationalities, one not a priori better than the other. According to some, rationalities may even differ as strongly as being 'incommensurable'¹¹, a term borrowed from Kuhn¹². The goal of science studies therefore cannot be to determine the ultimate truth. Quite the contrary, a rather common methodological principle in science studies tells us not to judge the truth content of claims at all.¹³

¹⁰ A recent study by Brante and Hallberg [1991] on a controversy over the definition of death shows similar implications. The authors suggest [p.394] that the fear that doctors might declare someone dead because they need an organ for transplant rather than for the interests of the patient is a basic underlying issue, although a large part of the debate is not at all conducted in these terms (this would lead to an analysis somewhat similar to the one in example 1).

¹¹ E.g. [Brante,Elzinga,1990], [Brante,Hallberg,1991].

¹² [Kuhn,1970]

¹³ E.g. in constructivism, following Berger and Luckmann [1966]; in a somewhat different sense also in the symmetry principle, as put forward in the Strong Program [Bloor,1976].

But now the learning perspective seems to be lost entirely. For if no judgement on the truth content of claims is allowed, how can we judge whether the participants were learning in any sense? As we saw already in the earlier examples, we do not understand what is going on unless we are willing to evaluate claims ourselves. Also we would like to know e.g. whether the fact that after initial struggle one party accepts the viewpoint of the opposing party is due to misleading, or to improved insight.

It seems natural then to ask whether there is an approach which does not have the absolutism of the classical approach, and which at the same time preserves some kind of learning perspective. In the following I will sketch a brief outline of such an approach.

An alternative approach¹⁴ : controversy studies as interactive feedback

We have of course no prefixed yardstick at our disposal to measure 'learning progress' (that would bring us back into the absolutist camp). This has the following implications:

(1) Preferably, a perspective on the learning of participants is not simply to be imposed upon the participants by the 'observer' (i.e. the sociologist of science) without consultation. If possible, the observer will enter into a dialogue with the participants. The third type of approach could therefore be characterized as a dialogical, interactive one. The observer (sociologist of science) is part of the process, and tries to help the relevant parties to learn.

(2) Rather than looking for utopia, the focus must be on what to avoid or eliminate, i.e., on negative priorities, which in the end must be recognized and accepted by the parties themselves. Observers can (i) expose rigidifications in the discussion, and show ways to eliminate and avoid them, and (ii) indicate cases where the interests of some parties are unjustly excluded from discussion.¹⁵ Rigidifications and 'incommensurabilities' will no longer be taken as given, but as an indication that the discussion may not be conducted in the most adequate way.¹⁶

One could say that the 'observer' is in fact checking the participants' process for major 'faults',

¹⁴ This approach is along the lines of what I have called 'counteranalysis' elsewhere [Birrer,1990].

¹⁵ So an observer may even initiate a new controversy, by pointing out to a party silent so far that its interests could be violated.

¹⁶ Kuhn's main point seems to be that paradigms are somehow organic wholes, and that therefore one cannot simply map one paradigm onto the other. He does not say that different paradigms could not be brought into interaction with each other (possibly changing both paradigms). Brante and Hallberg, in their main analysis, stick to the scientific rethoric of death controversy, and find two different perspectives, both internally consistent. But the underlying conflict is, as Brante and Hallberg suggest themselves, perhaps not so much a scientific conception of the essence of death, but rather the politics of medical practice. It is this 'context of relevance' where we should look for the core of the conflict, the scientific dressing may be merely strategic talk to impress opponents. We must not take 'scientific' claims too seriously.

reporting presumed faults to all for whom this might be relevant. This implies that the 'observer' is not just another participant. Straight partisanship is not likely to contribute to learning in an already formed conflict. The observer purposively distances himself from the process, looks at it from the outside, and informs those inside of his diagnosis. The observer is, however, part of a larger system which includes both participants and observer; the 'observer' operates as a kind of negative feedback with respect to the subsystem of the participants.

The activities of observers could be organised in programmes. The checking for major 'faults' can easily be transformed into research programmes, simply because major 'faults' in the use of scientific expertise can be systematized.¹⁷ In this brief exposition I can do no more than indicate just a few of such patterns that could serve for systematization purposes¹⁸ :

(1) Institutional patterns

Much of the politics of scientific expertise can be analyzed in terms of three types of actors :

(i) researchers

(ii) users and financiers of research

(iii) actors who are affected by the research or its use but do not belong to (i) or (ii)

Important origins of 'faults' in general, and in rigidification in particular, involve coupled prisoner dilemma's within each of the three groups, as well as exclusion of relevant parties (e.g. by a coalition between researchers and users/financiers).

Note that the scheme of analysis is not deterministic. Rather, it specifies 'temptations' (leading to the common identification of 'major faults') to which actors *may* yield, but not necessarily so.

(2) Rethorical patterns

For an example I refer to the recurring phenomenon of confusion of discourses discussed earlier.

All this leads naturally to what Brante and Elzinga¹⁹ call an 'integrative approach', blending epistemological, descriptive and political aspects.

Conclusion : controversy studies, why and how?

¹⁷ Cf. [Birrner,1991a]

¹⁸ See [Birrner,1990]

¹⁹ [Brante,Elzinga,1988]

For many decisions in our society we are dependent upon scientific expertise. Controversies between experts clearly pose a problem as to how these decisions should be made. The study of controversies has a great deal to contribute to the learning processes of participants in controversies. It could try to 'unfreeze' discussants in rigidified conflicts, show ways to avoid severe rigidification, and check whether the interests of all are considered sufficiently. At a more theoretical level, controversies are an obvious locus to study the interplay between 'intellectual' and 'social' factors, giving us a better understanding of the role of science and scientific expertise in society.

No controversy fits on unique description. There are always many different descriptions possible. To choose one description is to make an implicit or explicit choice of what particular problem one wants to address. In the foregoing I have argued for a rather practice-based problem definition, more or less 'in the service of society at large'. In doing so, my intention has not been to argue against historical research, where of course the direct interaction with participants in a controversy is often impossible. Historical cases can be illuminating.²⁰ On the other hand, I doubt if making sense of historical controversies can be completely detached from a learning perspective, with respect to the historic situation as well as in relation to the present. Neither did I intend to argue that most existing controversy studies are useless. On the contrary, there is much research that could easily be fitted into the approach proposed. My arguments have been about the self-understanding of controversy studies (and implicitly also about the self-understanding of science studies in general).

Is my proposal then, at least in a political sense, not a rather utopian one? Certainly, there are forces that will oppose a development in the direction I have sketched (researchers who are stuck in old frameworks, interest coalitions between scientists and decision makers, etc.). A shift in the direction indicated, however, is bound to appear sooner or later. The politics of expertise, originating in knowledge inequality between expert and non-expert, is a structural problem. Already, bias in science as a result of fraud is catching more and more attention. Sooner or later, the more general problem of potential bias in science will lead to some form of 'checking feedback-control' devices as described; for they are the only logical solution to the problem.

The classical as well as the modern perspective on science have become essentially unstable, since they cannot adequately accommodate the learning of participants in a controversy.

²⁰ It seems obvious to me though, that historical cases are more difficult, precisely because we cannot ask questions to the parties there and then.

Controversies are open ended learning processes, that may get blocked. This perspective affects not only the typology of controversies (adding e.g. the dimension of rigidification), it suggests a new role for the sociologist of science, one that is neither neutrality nor partisanship.²¹ During the years there have been several pleads for a more normative approach²²; it is time that we start *building* such programmes.

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²¹ For a recent discussion on neutrality and partisanship between Scott et al. and Collins, see [Scott,Richards,Martin,1990], [Collins,1991], [Martin,Richards,Scott,1991].

²² [Chubin, Restivo,1983], [Maxwell,1984], [Verhoog,1990], [Lynch,Fuhrman,1991], [Radder,1992]

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