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VOLUME 37 Number 4 December 2006

ISSN 0039-3681

Studies in History and Philosophy of Science

Volume Contents and
Author Index for Vol. 37 (2006)
are included at the back of this issue

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Discussion

Symmetry, asymmetry, and the real possibility of radical change: reply to Kochan

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Abstract

In his critique of my book *Heidegger and Marcuse*, Jeff Kochan (2006) asserts that I am committed to the possibility of private knowledge, transcendent truths, and individualism. In this reply I argue that he has misinterpreted my analysis of the Challenger disaster and Marcuse's work. Because I do not dismiss Roger Boisjoly's doubts about the Challenger launch, Kochan believes that I have abandoned a social concept of knowledge for a reliance on the private knowledge of a single individual. In fact, I consider Boisjoly's observations just as social, if not as scientific, as the results of rigorous scientific study. Kochan's reliance on a principle of symmetry derived from science studies to explain such politically charged technological controversies tends to mask the role of power and ideology in social life. Kochan interprets Marcuse as a failed Heideggerian who regresses from Heidegger's social conception of human being to traditional individualism. I am accused of sharing this view. This interpretation overlooks the importance of the Hegelian–Marxist category of 'real possibility' in Marcuse's work and so mistakes his critique of conformist politics for individualist romanticism. Marcuse always attempted to ground radical opposition in a community of struggle without abandoning the heritage of a long critical tradition. This view I willingly share.

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Keywords: Philosophy of technology; Principle of symmetry; Challenger disaster; Herbert Marcuse; Martin Heidegger; Knowledge

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While I am grateful to [Jeff Kochan \(2006\)](#) for his generous evaluation of my contribution to philosophy of technology, I must admit that I do not recognize myself in his portrait of my work. He accuses me of believing in the possibility of private knowledge, non-social transcendent truths, and individualistic aestheticism, a triple whammy that is surely fatal from the perspective of science and technology studies. But in reality these are not my views. This is fortunate because it suggests that we may share more common ground than Kochan thinks.

Although I have no doubt supplied Kochan with the elements of his critique, the theoretical context in which I placed the remarks he cites supports a very different interpretation more consistent with my frequent explicit advocacy of positions much closer to Kochan's own. To the extent that his mistaken impressions are not the fault of my limitations as a writer, I expect they are due to the unfamiliarity of the philosophical tradition in which I work.¹ In that tradition the various dichotomies he chooses between—social or individual, public or private, transcendent truth or communal consensus—all appear as antinomies to be transcended. Whether my discourse effectively transcends them is a good question, but I do not think Kochan perceives this as my goal and so he has squeezed me into a procrustean bed of familiar dimensions.

The first part of Kochan's critique concerns my remarks on Collins and Pinch's analysis of the Challenger disaster ([Collins & Pinch, 1998](#)). He argues here that I uphold the 'private knowledge' of the engineer who opposed the launch against the public knowledge of a scientific community which (tragically) recommended it. Presumably, because I privilege private knowledge, I must also believe that truth transcends the social conditions of its establishment in an epistemic community. Rationality would be a non-social effect of personal insight.

I am puzzled by this misreading of my argument for this is not at all what I intended. In fact the essay Kochan analyzes begins with an extended critique of Habermas for advocating a non-social concept of scientific–technical rationality. Surely, I would not follow up that critique by myself advocating the very position I had just dismissed! I conclude that 'Rationality is not an alternative to culture that can stand alone as the principle of social order, for better or worse. Rather, rationality in its modern technical form mediates cultural expression in ways that can in principle realize a wide range of values in the design of artefacts' ([Feenberg, 2003, p. 102](#)).

Nowhere in my essay do I suggest that Roger Boisjoly possessed private rather than public knowledge. The alternative—scientific consensus/private insight—is not exhaustive. Kochan has overlooked a third possibility. I argue that Boisjoly offered the fruits of an experienced engineer's 'observation' (*ibid.*, p. 86). Observation in this sense is certainly inferior to a proper scientific study, but it is not private. Other experienced people can share and debate observations and arrive together at informed, if non-scientific, judgments.

In practical life this is often all we have. Certainly it was all they had at NASA on that fateful day. Most of us rely on such judgments by physicians when scientific diagnostic tests are inconclusive or unavailable in time to determine appropriate treatment. However, Kochan simply dismisses observation as no 'kind of evidence at all'. This reflects a narrow scientism which I do not share.

¹ On occasion Kochan confuses my summaries of other positions with my own position as for example in the discussion of the relation of principle and application, which I attribute to Habermas but which Kochan attributes to me, or the reference to a critique of reason which purifies it of 'sociological accretions' which I attribute to Marxism and dismiss with the question 'Is this enough?' and answer with what I thought was a clear 'No' ([Feenberg, 1999, p. 162; 1988, pp. 238–239](#)).

Nevertheless, Kochan may well be right that the scientific prejudice of Boisjoly's community left him without a leg to stand on in his opposition to the launch. But that is precisely *my* critique, not Pinch and Collins's critique. As far as I can tell they think the real problem at NASA was the arrogant claim that the Shuttle was a reliable means of transportation. This seems to mean that they consider the dismissal of Boisjoly's observations appropriate despite the fact that he was right. This remarkable conclusion flows from their application of the principle of symmetry according to which outcomes do not count. As managers like to say after screwing up, 'Let's have no Monday morning quarter-backing'. The appropriate application of the principle of symmetry must carefully avoid endorsing such management nihilism.

I argued that at NASA itself symmetry was not respected since the results of observation were dismissed *even when no better evidence was available*. Perhaps I am too suspicious, but this seems to require explanation. And, contra Collins and Pinch, I am convinced by the standard explanation—that management pressure to meet the schedule overruled the evidence. The effect of the symmetrical account was to obscure the asymmetry at the heart of the affair: a 'technoscientific community' open to a wider variety of evidence and under less pressure from management might have delayed the launch and saved lives and reputation.

Is such openness compatible with a scientific outlook? Why not? I do not accept Kochan's notion that scientists and engineers cannot take seriously the evidence of an experienced expert's observations. I believe my own cognitive pluralism is a fairly standard common sense position in many 'technoscientific communities'.

What is more, I point out that the demand for strictly quantitative evidence often serves to delegitimize the observations and judgments of professionals such as physicians and teachers. Of course quantitative studies are useful, but sometimes they are manipulated to yield foregone conclusions. In such cases it is wise to look for the hidden agenda behind the preference for this specific type of evidence. For this purpose a symmetrical treatment of the case can be an obstacle rather than a help. Kochan concedes that ideology is involved where 'a decision clearly violates a community's immanent shared standards of rational judgment'. I think the problem is more general. It is important that the principle of symmetry not be invoked by researchers in cases where real world asymmetries significantly bias outcomes.

I would argue that just such violations are all too common where technology is concerned and, worse yet, often several communities with different standards of judgment confront each other and vie politically for control. I do not see how it is possible to sort out such technological controversies with a principle of symmetry originally designed to understand much simpler scientific disagreements where adversaries are more nearly equal in power and standards shared by all. Constructivists originally introduced the principle to demystify Whig history of science and the technocratic pretensions it supports, but today cynical appeals to symmetry now excuse inaction on global warming and other controversial issues.²

There is a second aspect to Kochan's 'counter-reflection' on the Challenger case which opens up an area of agreement between us once a misunderstanding is addressed. Kochan nicely explains my distinction between substantive and formal bias but then applies it in an

² See for example the interesting article of M. Lahsen (2005). At what point should ideology critique replace symmetrical treatment? This is a difficult question of degree, but after five years of the Bush administration's manipulation of scientific evidence it is impossible to dismiss the problem on general methodological grounds.

odd way. I am not sure how he came to the conclusion that I consider science substantively biased but he attributes this view to me. On the contrary, I introduced the concept of formal bias to criticize rationality generally and science and technology in particular. This sort of bias appears in the context-bound application of relatively neutral principles or procedures (Feenberg, 2002, pp. 75–79). I agree with Kochan that this is precisely the case in the Challenger affair. If NASA's reliance on quantitative evidence can be said to be biased, this is not because of some substantive defect in the very idea of quantity, but because the context in which just this type of evidence was privileged supported a foregone conclusion. In a very broad sense, the term 'ideology' seems to describe such cases, but this is not the place for a discussion of that hoary topic.

If bias is due to the social context of application, does this not imply that pure scientific rationality is unbiased and non-social? This is a possible consequence of my position which I want to avoid. Like many STS researchers, Kochan claims that 'technoscience is not qualitatively different from other forms of social life'. This certainly eliminates the possibility of a transcendent non-social truth, but it also obviates the need for a category such as formal bias. If it's all everyday life, then just as my preference for chocolate over cheese is substantively biased, so would be the chemists' preference for oxygen over phlogiston. The only reason to introduce a concept such as formal bias is to recognize the 'qualitative' difference between these cases. But in what exactly lies the difference if not in the transcendent purity of pure reason?

I believe there is a real blind spot among some science studies scholars as regards the difference between formal reasoning and everyday rationality. In their desire to reduce the gap between science and society, they have abolished it altogether. The ability of human beings to construct elaborate formal systems held together by chains of reasoning is surely something quite special that cannot be assimilated to the modes of action involved in taking a stroll or engaging a conversation. The clash between these different ways of thinking and acting has been highlighted practically in the world of computers where engineering assumptions about rationality come up against the resistance of ordinary users with their very different ideas about order and action (Suchman, 1987). This is one 'case history' with devastating implications for the claim that 'technoscience is not qualitatively different from other forms of social life'.

To make such a distinction is not to claim that formal systems are transcendent, just that they are different in ways that have something to do with their very nature. No amount of strategic analysis of how these systems are deployed socially can fully account for their structure and claims to validity. This seems self-evident (at least to philosophers) and yet it is sometimes contested in science studies on the grounds that, as Foucault once said, 'truth is a thing of this world'. I think this dilemma indicates an unsolved problem. A social account of reason is necessary but it must make sense of the unique formal capacity of the human mind.³

I would like to turn now to Kochan's critique of my interpretation of Marcuse. Kochan finds all sorts of problems in Marcuse and attributes them to me as an advocate of Mar-

³ Doppelt (Forthcoming) argues that the social character of scientific controversy is best captured through the study of differences in epistemic standards rather than through attention to the immediate impact of substantive social values. Of course epistemic standards may reflect social values, but standards are internal to the formal rationality of science rather than extrinsic 'influences' distorting rational thought. This position in the philosophy of science is close to the notion of formal bias I have introduced in the philosophy of technology.

cuse's views. But once again there is a missing moment in his critique. He notes that I consider the Hegelian element in Marcuse's position to be the theory of 'real possibility' but he drops this topic in his exclusive focus on Marcuse's relation to Heidegger. In Marcuse's interpretation 'real possibilities' are immanent tensions in the social world, potentialities that can give rise to a new society. Ignoring this concept leads Kochan to the conclusion that Marcuse's position, which he claims I endorse, is individualistic and relies on transcendent concepts of rationality and aesthetics.

The case is more interesting. Marcuse does not dismiss the social character of *Dasein* in his early work but interprets it *more* socially than Heidegger. In an early essay written while he was working as Heidegger's assistant, Marcuse asks:

Is the world 'the same' even for all forms of *Dasein* present within a concrete historical situation? Obviously not. It is not only that the world of significance varies among particular contemporary cultural regions and groups, but also that, within any one of these, abysses of meaning may open up between different worlds. Precisely in the most existentially essential behaviour, no understanding exists between the world of the high-capitalist bourgeois and that of the small farmer or proletarian. Here the examination is forced to confront the question of the material constitution of historicity, a breakthrough that Heidegger neither achieves nor even gestures toward. (Marcuse, 1978, pp. 364–365)

This passage disaggregates '*das Man*' and situates the individual in a concrete community of shared life conditions and interests. The danger of Heidegger's own undifferentiated position became clear when he identified the community with the nation, minus its Jewish population. Marcuse was a Marxist and so all his early talk of resistance and confrontation with the times refers to class struggle and not to the isolated individual as Kochan believes. The 'real possibility' of radical social change rests on the dynamic of this struggle rather than an individual utopia.

The later Marcuse had problems with this early view not because he regressed to individualism but because he could no longer believe that the working class constituted an oppositional community. This realistic appreciation of the working class calls into question the Hegelian–Marxist 'real possibility' of socialist revolution. Yet there is no way back from Hegel to a pure ethical exigency. Marcuse's solution went through several stages which I outline in my book. In the end he believed the New Left once again formed a community of resistance offering real possibilities of radical change.

There is nothing inconsistent about Marcuse's position although he did change his emphasis in response to historical events. Even in *One-dimensional man*, his most pessimistic book, he admits that he is undecided as between two hypotheses, that advanced industrial society can contain all opposition or that 'forces and tendencies exist which may break this containment and explode the society' (Marcuse, 1991, p. xlvii). He would certainly agree with Kochan, as I do, that the new social movements constitute a continuing link in the historical chain of resistant communities. Thus it is unfair to claim that Marcuse abandoned Heidegger's social conception of individual authenticity for a standard individualistic conception. What he abandoned was Heidegger's potentially nationalistic effacement of social differences.

Kochan has similar difficulties with Marcuse's notion that scientific–technical rationality is distorted in advanced industrial societies. This notion is not meant to refer us to a pure transcendent rationality as Kochan thinks, but on the contrary is a critique of that

very idea. It is 'one-dimensional' thought that affirms the purity of transcendent rationality and thereby, according to Marcuse, masks the true nature of modern science and technology. In his view rationality is always situated historically as the project of a social subject, a dominant or subordinate social group (*ibid.*, p. 146). I have my own difficulties with Marcuse's formulation, but in any case his view is different from the one Kochan attributes to him and to me.

Finally, Kochan appears to buy Alasdair MacIntyre's claim that Marcuse's aesthetics is anti-democratic. MacIntyre's embarrassing polemic is hardly a reliable authority. For Marcuse, art is the repository of the hopes of the human race, hopes that are taken up by rebellious social groups at various times in the course of history. The New Left had this significance for Marcuse but he was well aware that it represented only a small minority. He advocated the generalization of critical consciousness, not an educational dictatorship.

The dismissal of Marcuse as a totalitarian elitist is a serious distortion of his thought as I show textually (*Feenberg, 2005, p. 90*). It is true that Marcuse was a fierce critic of the conformist American public that cheered on the troops in Vietnam. If that is elitist and totalitarian, then presumably criticism of Hitler's voters would reveal a similar disrespect for the wisdom of the democratic public that elected him.

The achievement of individuality is indeed more complex in twentieth-century societies saturated with media propaganda and consumer goods than it was when the ideal was first formulated. Heidegger's notion of authenticity is a flawed response to this complexity since he does not address the concrete obstacles to mental independence. Marcuse's reflection on individuality is at least focussed on these obstacles. Taking notice of them seems a reasonable response to the subversion of democracy by new techniques of manipulation. Postmodern scepticism about the very idea of media manipulation belongs to a happier time when it appeared that proliferating popular appropriations and resistances constituted a force that enfeebled nation states, parties and faiths could not resist. Today we know better, or ought to.

Were its ideal of prosperity Marcuse's only objection to this society, one could easily dismiss his critique as elitist, but he was just as concerned with the nationalism and imperialism that flourish under these conditions. Recent events appear to confirm his worries. Of course questions can be raised about Marcuse's formulations, but he never gave up hope that someday people would rise above the illusory threats and satisfactions and create a peaceful and solidary society.

In sum, my position is much closer to Kochan's than he imagines. I am not committed to either individualism or a transcendent notion of truth. I agree with him that reason is in a significant sense social and public, although not necessarily scientific. I also agree with him that progressive political movements must be rooted in a community, on condition that the community in question is not defined by reactionary racial, national, or religious ideology. I even agree that the principle of symmetry is an important methodological tool in the study of scientific controversy. However, I do not agree that this principle is a viable basis for a critical politics of technology. For that purpose we would do better to look to older traditions of social and political theory that address questions of power and ideology with more appropriate theoretical resources. In my own work I have started out from these traditions but have tried also to learn from recent work in science and technology studies. I welcome reciprocal gestures from STS scholars.

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