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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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29   While I am grateful to Jeff Kochan (2006) for his generous evaluation of my contribution
30 to philosophy of technology, I must admit that I do not recognize myself in his portrait of my
31 work. He accuses me of believing in the possibility of private knowledge, non-social transcen-
32 dent truths, and individualistic aestheticism, a triple whammy that is surely fatal from the
33 perspective of science and technology studies. But in reality these are not my views. This is
34 fortunate because it suggests that we may share more common ground than Kochan thinks.
35   Although I have no doubt supplied Kochan with the elements of his critique, the the-
36 oretical context in which I placed the remarks he cites supports a very different interpre-
37 tation more consistent with my frequent explicit advocacy of positions much closer to
38 Kochan's own. To the extent that his mistaken impressions are not the fault of my limi-
39 tations as a writer, I expect they are due to the unfamiliarity of the philosophical tradition
40 in which I work.[1] In that tradition the various dichotomies he chooses between—social or
41 individual, public or private, transcendent truth or communal consensus—all appear as
42 antinomies to be transcended. Whether my discourse effectively transcends them is a good
43 question, but I do not think Kochan perceives this as my goal and so he has squeezed me
44 into a procrustean bed of familiar dimensions.
45   The first part of Kochan's critique concerns my remarks on Collins and Pinch's analysis of
46 the Challenger disaster (Collins & Pinch, 1998). He argues here that I uphold the 'private
47 knowledge' of the engineer who opposed the launch against the public knowledge of a scien-
48 tific community which (tragically) recommended it. Presumably, because I privilege private
49 knowledge, I must also believe that truth transcends the social conditions of its establishment
50 in an epistemic community. Rationality would be a non-social effect of personal insight.
51   I am puzzled by this misreading of my argument for this is not at all what I intended. In
52 fact the essay Kochan analyzes begins with an extended critique of Habermas for advocat-
53 ing a non-social concept of scientific–technical rationality. Surely, I would not follow up
54 that critique by myself advocating the very position I had just dismissed! I conclude that
55 'Rationality is not an alternative to culture that can stand alone as the principle of social
56 order, for better or worse. Rather, rationality in its modern technical form mediates cul-
57 tural expression in ways that can in principle realize a wide range of values in the design of
58 artefacts' (Feenberg, 2003, p. 102).
59   Nowhere in my essay do I suggest that Roger Boisjoly possessed private rather than pub-
60 lic knowledge. The alternative—scientific consensus/private insight—is not exhaustive.
61 Kochan has overlooked a third possibility. I argue that Boisjoly offered the fruits of an expe-
62 rienced engineer's 'observation' (ibid., p. 86). Observation in this sense is certainly inferior to
63 a proper scientific study, but it is not private. Other experienced people can share and debate
64 observations and arrive together at informed, if non-scientific, judgments.
65   In practical life this is often all we have. Certainly it was all they had at NASA on that
66 fateful day. Most of us rely on such judgments by physicians when scientific diagnostic
67 tests are inconclusive or unavailable in time to determine appropriate treatment. However,
68 Kochan simply dismisses observation as no 'kind of evidence at all'. This reflects a narrow
69 scientism which I do not share.

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[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).

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

80   I argued that at NASA itself symmetry was not respected since the results of observa-
81 tion were dismissed *even when no better evidence was available*. Perhaps I am too suspi-
82 cious, but this seems to require explanation. And, contra Collins and Pinch, I am
83 convinced by the standard explanation—that management pressure to meet the schedule
84 overruled the evidence. The effect of the symmetrical account was to obscure the asymme-
85 try at the heart of the affair: a 'technoscientific community' open to a wider variety of evi-
86 dence and under less pressure from management might have delayed the launch and saved
87 lives and reputation.

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

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

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

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

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[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.

113 odd way. I am not sure how he came to the conclusion that I consider science substantively
114 biased but he attributes this view to me. On the contrary, I introduced the concept of for-
115 mal bias to criticize rationality generally and science and technology in particular. This
116 sort of bias appears in the context-bound application of relatively neutral principles or
117 procedures (Feenberg, 2002, pp. 75–79). I agree with Kochan that this is precisely the case
118 in the Challenger affair. If NASA's reliance on quantitative evidence can be said to be
119 biased, this is not because of some substantive defect in the very idea of quantity, but
120 because the context in which just this type of evidence was privileged supported a foregone
121 conclusion. In a very broad sense, the term 'ideology' seems to describe such cases, but this
122 is not the place for a discussion of that hoary topic.
123   If bias is due to the social context of application, does this not imply that pure scientific
124 rationality is unbiased and non-social? This is a possible consequence of my position
125 which I want to avoid. Like many STS researchers, Kochan claims that 'technoscience
126 is not qualitatively different from other forms of social life'. This certainly eliminates
127 the possibility of a transcendent non-social truth, but it also obviates the need for a cat-
128 egory such as formal bias. If it's all everyday life, then just as my preference for chocolate
129 over cheese is substantively biased, so would be the chemists' preference for oxygen over
130 phlogiston. The only reason to introduce a concept such as formal bias is to recognize the
131 'qualitative' difference between these cases. But in what exactly lies the difference if not in
132 the transcendent purity of pure reason?
133   I believe there is a real blind spot among some science studies scholars as regards the
134 difference between formal reasoning and everyday rationality. In their desire to reduce
135 the gap between science and society, they have abolished it altogether. The ability of
136 human beings to construct elaborate formal systems held together by chains of reasoning
137 is surely something quite special that cannot be assimilated to the modes of action
138 involved in taking a stroll or engaging a conversation. The clash between these different
139 ways of thinking and acting has been highlighted practically in the world of computers
140 where engineering assumptions about rationality come up against the resistance of ordin-
141 ary users with their very different ideas about order and action (Suchman, 1987). This is
142 one 'case history' with devastating implications for the claim that 'technoscience is not
143 qualitatively different from other forms of social life'.
144   To make such a distinction is not to claim that formal systems are transcendent, just
145 that they are different in ways that have something to do with their very nature. No
146 amount of strategic analysis of how these systems are deployed socially can fully account
147 for their structure and claims to validity. This seems self-evident (at least to philosophers)
148 and yet it is sometimes contested in science studies on the grounds that, as Foucault once
149 said, 'truth is a thing of this world'. I think this dilemma indicates an unsolved problem. A
150 social account of reason is necessary but it must make sense of the unique formal capacity
151 of the human mind.[3]
152   I would like to turn now to Kochan's critique of my interpretation of Marcuse. Kochan
153 finds all sorts of problems in Marcuse and attributes them to me as an advocate of Mar-

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[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.

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

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

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

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

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

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

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

200 very idea. It is 'one-dimensional' thought that affirms the purity of transcendent rational-
201 ity and thereby, according to Marcuse, masks the true nature of modern science and tech-
202 nology. In his view rationality is always situated historically as the project of a social
203 subject, a dominant or subordinate social group (ibid., p. 146). I have my own difficulties
204 with Marcuse's formulation, but in any case his view is different from the one Kochan
205 attributes to him and to me.
206   Finally, Kochan appears to buy Alasdair MacIntyre's claim that Marcuse's aesthet-
207 ics is anti-democratic. MacIntyre's embarrassing polemic is hardly a reliable authority.
208 For Marcuse, art is the repository of the hopes of the human race, hopes that are
209 taken up by rebellious social groups at various times in the course of history. The
210 New Left had this significance for Marcuse but he was well aware that it represented
211 only a small minority. He advocated the generalization of critical consciousness, not an
212 educational dictatorship.
213   The dismissal of Marcuse as a totalitarian elitist is a serious distortion of his thought as
214 I show textually (Feenberg, 2005, p. 90). It is true that Marcuse was a fierce critic of the
215 conformist American public that cheered on the troops in Vietnam. If that is elitist and
216 totalitarian, then presumably criticism of Hitler's voters would reveal a similar disrespect
217 for the wisdom of the democratic public that elected him.
218   The achievement of individuality is indeed more complex in twentieth-century societies
219 saturated with media propaganda and consumer goods than it was when the ideal was first
220 formulated. Heidegger's notion of authenticity is a flawed response to this complexity
221 since he does not address the concrete obstacles to mental independence. Marcuse's reflec-
222 tion on individuality is at least focussed on these obstacles. Taking notice of them seems a
223 reasonable response to the subversion of democracy by new techniques of manipulation.
224 Postmodern scepticism about the very idea of media manipulation belongs to a happier
225 time when it appeared that proliferating popular appropriations and resistances consti-
226 tuted a force that enfeebled nation states, parties and faiths could not resist. Today we
227 know better, or ought to.
228   Were its ideal of prosperity Marcuse's only objection to this society, one could easily
229 dismiss his critique as elitist, but he was just as concerned with the nationalism and impe-
230 rialism that flourish under these conditions. Recent events appear to confirm his worries.
231 Of course questions can be raised about Marcuse's formulations, but he never gave up
232 hope that someday people would rise above the illusory threats and satisfactions and cre-
233 ate a peaceful and solidary society.
234   In sum, my position is much closer to Kochan's than he imagines. I am not com-
235 mitted to either individualism or a transcendent notion of truth. I agree with him that
236 reason is in a significant sense social and public, although not necessarily scientific. I
237 also agree with him that progressive political movements must be rooted in a commu-
238 nity, on condition that the community in question is not defined by reactionary racial,
239 national, or religious ideology. I even agree that the principle of symmetry is an impor-
240 tant methodological tool in the study of scientific controversy. However, I do not agree
241 that this principle is a viable basis for a critical politics of technology. For that pur-
242 pose we would do better to look to older traditions of social and political theory that
243 address questions of power and ideology with more appropriate theoretical resources.
244 In my own work I have started out from these traditions but have tried also to learn
245 from recent work in science and technology studies. I welcome reciprocal gestures from
246 STS scholars.

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