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Response to My Critics

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Authors should always be so fortunate as to have such thoughtful and stimulating readings of one's work. What follows: I turn some comments by Renee Anspach, Hugh Gusterson, and Thomas Hughes into invitations to do further research. I then discuss organizational frames in the context of other conceptions of frames. Last, I tackle the difficult issue of taking a stand on the science in *Whole World on Fire* (Eden, 2004) while claiming to be a thoroughgoing social constructivist.

Invitations

Renee Anspach comments that relatively few scholars of science and technology have studied the 'culture of power' – that is, 'studied up' as Laura Nader exhorted more than 30 years ago – but instead have most often studied those fairly similar to us: researchers in scientific or academic settings. Certainly *Whole World on Fire* is in excellent select company in studying up, that is, in studying government-funded researchers whose work was shaped by – but also shaped – government priorities.[1] Yet what an interesting review paper it would be to compare what appears to be a fairly small number of works that study up with the larger shape of the science and technology literature; at the same time to compare how those works that study up approach power differently from each other; and, finally, to compare the treatment of power, knowledge, and artifacts in science and technology studies with other fields, perhaps anthropology or architectural studies or diplomatic history or environmental history. Or journalism – for example, Richard Rhodes's *The Making of the Atomic Bomb* (1987), or, recently, William Langewiesche's (2005, 2006) articles on A.Q. Khan and Pakistan's nuclear weapons program.

At the same time, Hugh Gusterson criticizes me for not studying up enough, specifically, ignoring the politics and nuclear policies of US presidential administrations. It is true that *Whole World on Fire* is largely an internal history of hidden and largely autonomous organizations,[2] and for a reason: it is my judgment that the deep logic and details of damage calculations in the US military's nuclear war plans have not been closely guided by high-level political direction. Even though the war planning process is formally driven by presidential directives, these directives leave

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the military with a great deal of autonomy (Eden, 2004: 95–96). I saw no evidence, for example, that the 1992 decision not to rethink fire damage in the Pentagon war plan reflected, in Gusterson's words, a 'notorious lack of imagination' on the part of the George H.W. Bush administration. Whether the interest in collateral damage had anything to do with the priorities of the Clinton administration, I do not know, though I do know that the military was concerned with collateral damage in this period. Indeed, my position could be interestingly tested and possibly modified by looking anew at nuclear war planning from a top-down perspective that goes beyond the public articulation of nuclear strategy and carries all the way to the details of planning.[3]

Thomas Hughes suggests that US military planners did not predict nuclear fire damage both because they found the effects of mass fire too horrible to contemplate and because they may have been concerned that the public would have found fire damage to be morally repugnant. I considered both in *Whole World on Fire* and concluded that neither horror nor moral repugnance was salient. I found no particular sensitivities to fire effects on the part of the military and no fears that the details of their calculations would reach the public. (This is not to say that the government was unconcerned about public response to nuclear weapons, as demonstrated in efforts to show the survivability of nuclear war, including exhortations to engage in 'fireproof housekeeping' and to develop the survival skills of children by providing schools with skits such as 'Let's Plan What to Do Now' [Eden, 2004: 165–70, 201–02].) And yet, a different angle of attack from the one I take in my own research – in particular, a greater concentration on the thoughts and actions of high-ranking Air Force officers and others – might possibly reveal more repugnance or moral concern than I found deeper in the bowels of the bureaucracy, and it is possible that such findings could cast the history in a different light.[4]

Finally, Gusterson says it could be annoying – but it is not – to say that he would have liked more in *Whole World on Fire* comparing the US approach with those of the British, Russians, and others whom we would expect to have developed predictions of nuclear weapons damage. I too would have liked more. I asked historians about the British side but did not bend my shoulder to the wheel of archival inquiry there or pursue interviews. I asked a few retired Russian generals about the Soviet/Russian side; once I thought I hit pay dirt, but when I asked for further clarification, what had seemed clear earlier became murky. Putting the findings from the USA into comparative context is perhaps the most important and most difficult extension of the research in *Whole World on Fire*. Gusterson's questions are a very good guide to where to begin.

Frames, Specifically Organizational Frames

Here I address two comments: Anspach's, that organizational frames are ultimately traceable to Goffman's *Frame Analysis* (1974), and Hughes's challenge that *his* idea of *technological momentum* is better than what he sees

as my kludging together *social construction*, *path dependence*, and *organizational frames* – and, he might have added, *knowledge-laden organizational routines*. I'll also address his claim, if I understand it, that I only nod to theories of organizations. So, frames: whence and with what power?

There are numerous notions of frames in use. Indeed, when I was writing, my friends would bring me various definitions – sort of like the cat bringing in yet another mouse. Very nice, thank you, but after a while you don't actually want to see any more. Anspach has caused me to look again.

I discuss three conceptions here.

(1) The idea of organizational frames that I use in *Whole World on Fire* directly draws on Wiebe Bijker's (1987) concept of technological frame – in particular, Bijker's emphasis on problem-solving, including problem representation and solution strategies and requirements. However, I focus not on technological communities but on the role of organizations in problem-solving. To explain technological artifacts, Bijker (1987: 168) starts with communities of practitioners or scientists of varying sensibility and capability and maps them into larger society. To explain 'knowledge-laden organizational routines', I start with organizational history and goals, which causes the recruitment of practitioners of varying sensibility and capability (Eden, 2004: 49–50).[5] I don't just 'nod' to organization theory but draw on, among others, Herbert Simon's notions of problem-solving and hierarchy; James March and his colleagues' work on organizational learning; and Richard Nelson and Sidney Winter, and Martha Feldman, on organizational routines.[6]

(2) Probably the most prominent notion of 'frame' and 'framing' is that used in the psychology of choice. This idea is not about how problems are selected and solved but about how possible consequences are represented. In Robyn Dawes's (1988: 35) clear definition: 'The way in which a consequence is presented is termed its *frame*.' The key work is by Amos Tversky and Daniel Kahneman (1986), who demonstrated that the 'framing of outcomes' (p. S258) significantly affects the choices people make: for example, people will choose differently depending on whether identical outcomes are presented in terms of the probability of survival (what Tversky & Kahneman [1986: S254] call a 'survival frame') or the probability of mortality ('mortality frame'). More broadly, 'Framing the consequences of a public policy in positive or in negative terms can greatly alter its appeal' (1986: S261).[7] The work of Richard Fryklund (1962) provides an illustration from the nuclear realm. In the early 1960s, Fryklund argued that the USA should procure an expensive and more accurate nuclear arsenal and change its nuclear war plans so that in the event of a nuclear war, many lives could be saved. Indeed, the book title was cast in a survivability frame: *100 Million Lives: Maximum Survival in a Nuclear War*. Had the idea been put in terms of a mortality frame, it probably

would have had less appeal: *Half the Population Dead: The Return on Your Increased Investment in a 'Counterforce' Arsenal in the Event of a Nuclear War.*[8]

(3) Erving Goffman's (1974) concept of frames referred to by Anspach is more closely related to Bijker's technological frames and my organizational frames than to the psychology of choice. For Goffman, frames are collectively held definitions of situations, sustained and changed by individual understandings and performances. A frame defines what is happening: the scene, rules, and roles that organize experience.[9] Goffman (1974: 561) provides many examples: a man gives instructions to the postman; he greets a passing couple who return the pleasantries; he gets into his car and drives off into traffic. Each activity is governed by mutual understandings of the situation: what is happening, what is real, and the rules and range of appropriate behavior. Does this mean that individuals always share mutual understandings of a situation? No, and it is these breaches, including accidental misunderstandings and deliberate con jobs, that Goffman uses to explain how most people, most of the time, operate within commonly understood frames.

One such frame is the military briefing – for example, on the US nuclear war plan. Such briefings entail an officer rapidly presenting a large number of slides with far more information than can be taken in and with very little time for questions. In such a briefing, the results of 'executing' the war plan are cast as percentages of various types of enemy target expected to be destroyed by US nuclear forces. The frame of such a briefing, well understood by participants, is one of crisp professional presentation in which basic premises may not be questioned; and an emphasis on the physical damage inflicted on what are considered to be military targets, though these may well include industrial or governmental or communication structures in the midst of large cities. When an audience member asked at such a briefing years ago, 'How many people do you kill?' he broke from the common understanding of the situation. The briefer replied, 'What we're talking about here is counter-military operations', and continued on.[10]

Goffman's frames encompass many more situations than those involved in problem-solving. But were we to examine the taken-for-granted assumptions about problems to be solved or understood rules of interaction, we would arrive at Goffman's frames.

So, what about technological momentum, which Hughes says is a more unified and powerful idea than those I use of social construction, organizational frames, and path dependency? In fact, I think we're both trying to get purchase on some very similar things: organizational problem-focus; organizational capacity to solve problems; and self-reinforcing processes, or momentum, or inertia, which give weight to the past and make change difficult. We can liken social science concepts to architecture: we're trying to build explanatory houses and we choose slightly different designs

and timbers. Hughes likes to take concepts from other domains – for example, reverse salients or inertia – and develop the analogy. I prefer to work with a cluster of ideas to convey social processes through time. Of course, I don't mean that these differences don't matter, but I'm more struck here by the similarities. In fact, it would be interesting to give students Hughes's (1969) paper on 'Technological Momentum in History: Hydrogenation in Germany 1898–1933' and have them compare what you get when you use Hughes's idea of technological momentum with Bijker's idea of technological frame and mine of organizational frame.

Taking a Stand on the Science while being a Social Constructivist?

Anspach argues that for socially consequential issues there are powerful ethical and sociological reasons to take a stand when the researcher 'has reason to believe that one side is correct', and she celebrates my taking a stand as an act of 'enormous intellectual daring'.[11] Gusterson laments that I have taken this stand and wishes I had 'taken a more deconstructive approach' – for example, that I had done a more MacKenzian analysis and inquired more into the social construction of 'firestorms'. Certainly, the advice, 'Donald MacKenzie: be like him', is, if impossible to fulfill, excellent to aim for. Further, Gusterson says that I should have adhered to David Bloor's 'strong programme' according to which, in Gusterson's words, 'sociologists of science are not to invoke the "real" in explaining why knowledge claims either were or were not accepted by scientists'.

The keys here are what Bloor refers to as *impartiality* and *symmetry*. In other words, one's explanation should be *impartial* with regard to truth and falsity, success and failure; both parts of these dichotomies must be explained. Further, they must be explained *symmetrically*, that is, the same *type* of cause explains both 'true' and 'false' beliefs, and, following from this, the true state of the world cannot explain 'true' beliefs *and* why certain beliefs – those that are true – prevail (Bloor, 1991 [1976]: 7).[12]

Gusterson argues that I have, in fact, written two books: one that 'probes with meticulous care the ambiguities and lacunae in a field of knowledge' – in other words, a science studies book that adheres to conventions of impartiality and symmetry – and another 'bureaucratic politics' book that asks why organizations failed to accept 'persuasive scientific knowledge'.

Gusterson is correct that there are two different kinds of argument in *Whole World on Fire*, though I would not characterize them as Gusterson has. I agree that I bookend the historical account with an opening and horrifying description of the effects of a nuclear detonation over Washington; I take physicists Theodore Postol's and Harold Brode's claims as given in the same chapter; and I conclude by categorizing the failure to predict fire damage with other failures to make accurate predictions about the physical world.

It seems to me there are two issues here: is the analytical historical account that comprises the bulk of the book sufficiently dispassionate, that is, impartial and symmetrical? And do the 'bookends' violate neutrality and mar the account? Let me discuss each in turn.

Regarding the narrative, Gusterson says he's reading a little against the grain to observe that the text is replete with evidence that Brode's claims about fire were highly contested – but he is not. Indeed (to return to my house metaphor), I specifically laid the floor and polished it along the very grain that Gusterson is reading. The positions Gusterson describes are as I presented them (though I would say that the contestation was not by Brode's colleagues but by a separate research community with a different orientation). And I did not reify 'firestorms' as a natural category but explained how the mass fires that burned down cities in World War II were first understood and how ideas about them changed and diverged over time. But perhaps Gusterson and I simply disagree about how well I pulled this off.

Beyond these specifics, I think there is greater impartiality in the main text than Gusterson posits – and I mean here in the more formal sense, not in the sense that my tone betrayed me, which apparently it did to some degree, despite my best efforts to avoid it (though I never saw Brode in heroic terms). But more formally, in the sense that Bloor uses impartiality, I don't think I ever say in the narrative that Brode and the few other physicists in his camp saw the world the way they did because that's how the world was. I do explain their conceptual apparatus – for example, their holistic large-scale regional analysis of mass fire – or firestorm – and I think I contrast it dispassionately with the kind of percolation model used by those who said firestorms could not be predicted. In both cases I explain the disciplinary and institutional backgrounds that led to such differences. And of course I never explain historical outcomes on the basis of who I think was doing the better science. That was impossible since the historical outcome that ensued, and ensues to this day, is that those who advocated inclusion of predictions of fire damage in US nuclear war planning lost.

The more vexing issue is the bookends, particularly the opening chapter.[13] Here, Gusterson criticizes me for endorsing Brode's (and he might have added, Postol's) perspective on the nature and predictability of firestorms.

Let me approach this historically and say that when I began, I intended to do just as Gusterson argues that I should have done. I saw (and see) myself as a social constructivist, and it was my aspiration to open with an impartial account: here's how one community of physicists sees the effects of a nuclear detonation, and here's an alternative view by another community of government officials and engineers. The question, then, was simply: why do they understand the physical world so differently?

However, as I proceeded, I increasingly found myself wrapped around a conceptual axle: I wanted to be impartial, but I was uneasy. First, I actually had been convinced by Postol and Brode that for higher-yield nuclear weapons, detonated in or near urban areas, under most weather

conditions including most rain, fog, and snow conditions, the range of fire damage was likely to be greater – significantly greater depending on the details – than the range of blast damage and could be predicted approximately as well. (Why was I persuaded by Postol and Brode? I think it was the cumulative effect of the following: they reasoned from straightforward physical understandings; they explained why the physical intuition they employed was appropriate to the scale of the phenomena they were examining; Postol, in particular, explained the errors of employing a too-small scale in the physical models used by those who thought fire damage not predictable; their findings were both self-consistent and consistent with historical data; they were judicious, not tendentious, in their approaches; and they had each spent significant time and concentration studying the issues.) Second, and relatedly, it just seemed boring to explain how each group saw the world and to leave it at that. I was left with a sense of 'so what?'. Somehow, the aesthetics did not work. Yet I thought I was committed to an even-handed treatment in which I would not take a position on the nature of nature.

So I went round and round between taking a position on the science or not until a fateful conversation with the political scientist and theorist David Dessler. In this conversation, Dessler said something close to what I subsequently wrote in the book: in Dessler's words, just because people are still arguing doesn't mean that the science is not, in fact, resolved. Dessler advised me to go ahead and take a strong stand on two grounds. First, I should not ignore that I thought the basic question resolved. Second, it would be a far more interesting piece of social science to take a position on the science. It would set up a much more compelling puzzle. Further, Dessler saw no philosophical problem with taking a scientific realist view of the physics of nuclear weapons effects and a social constructivist view of historical process. In other words, it's *not* turtles – that is, ideas – all the way down.

Exemplary works in science and technology studies – on large technological systems such as electricity (Hughes, 1983), Bakelite (Bijker, 1987, 1995), bicycles (Pinch & Bijker, 1987), inertial guidance (MacKenzie, 1990), the US space shuttle system (Vaughan, 1996), gravitational wave detection (Collins, 2004), and so on – are premised on significant technological or scientific closure. For example, it was clear decades before MacKenzie (1990) that inertial navigation was a viable technology; there was no question that the *Challenger* launch had failed catastrophically; and Harry Collins's (2004: xv) work is premised on the understanding that gravitational waves exist and 'sometime in the not-too-distant future' will be detected.[14]

As I say in the introduction to *Whole World on Fire* (p. 7), I did not have the luxury of closure, and so what I did was to go with my best judgment and treat the science as closed, that is, treat the characterization of mass fire by Postol, Brode, and Brode's colleagues as reliably established. In fact, this is not inconsistent with Bloor's own argument that:

> the sociology of knowledge is committed to some picture of what is really happening. Some characterization must be offered of what actors are responding to, of what experience they have in their environment, and of what purposes inform their interaction with it and with one another. Such assumptions must be made to get explanation underway, and sometimes (though not always) these may carry logical implications about the truth of the actors' beliefs . . . . The interesting question [then] is how the world is going to be described by the actors. (Bloor, 1991 [1976]: 177)

I realize that this rhetorical device of closing an issue that, politically, is far from closed, has its risks. It could be that the physical world does not confirm to the understandings of the physicists who have most closely studied the problem. Or perhaps some will say I made an epistemological botch of it. I do know, however, that this was the only way I could see to write *Whole World on Fire*.

Finally, let me address Gusterson's concern that Brode and his colleagues, in borrowing a hydrodynamic code, may have inappropriately extrapolated it through 'knobs', or fudge factors, to a largely unknown physical regimen – and produced unreliable results.

This specific question is not one I asked when I wrote *Whole World on Fire*. Gusterson's query led me to do some preliminary research. At a first cut it appears that weapons designers and weapons effects scientists use codes somewhat differently from each other. Weapons designers engage in vast numbers of repeated calculations and for most of the nuclear age have validated their designs in nuclear weapons tests. They generally do not need to know the deepest workings of the programs themselves. Nuclear weapons effects calculations have tended to be less routine, more singular, and more transparent. Fudge factors – that is, adjustments to code that help match test data to experimental or other theoretical information – play less of a role since the goal is not to build something that works but to understand in detail the physical phenomena being modeled. Finally, I'm not clear on whom Gusterson thinks is engaged in 'the phony façade of certainty that shields the overconfident projections of defense planners about . . . nuclear war.' *Whole World on Fire* is an effort to break through one such apparent certainty: that fire damage is unpredictable and can therefore be ignored in war planning or in possible decisions to use nuclear weapons.

Notes

I thank my critics. I also thank Rebecca Slayton and Alex Montgomery for some helpful exchanges and conversation, though I don't know if they will agree with what I have written.

1. Works that 'study up' include Gusterson (1996), Hughes (1998), MacKenzie (1990), Spinardi (1994), and Vaughan (1996).
2. Nonetheless, I do map the activities of war planning organizations into the early 20th century origins of the US Air Force, the prosecution of World War II, briefly the political landscape at the end of World War II, several pre-existing professions, civil defense efforts under President John Kennedy, and the nuclear winter campaign of the 1980s.

3. Here, one would want to start with the work of David A. Rosenberg and others cited in Eden (2004: 320–21, notes 2–6). See also the more recent work by William Burr (2005a, 2005b).
4. I did explore the thought of Air Force officers in secondary sources, particularly in Schaffer (1985), and Sherry (1987), which is where anyone examining these issues should start, along with Biddle (2002).
5. As Bijker notes, the connection to Kuhn's paradigms is evident. I think of frames, whether technological or organizational, as paradigms writ small. Orlikowski & Gash's (1994) definition of technological frame shares features of Bijker's, Eden's, and Goffman's notions. A brief definition of frames emphasizing power and sensemaking in organizations is found in Milliken et al. (2005: 248).
6. For citations to the organizational literature, see Eden (2004: 309–14). On the relevance of *Whole World on Fire* to organization theory, see Weick's (2005) review of the book.
7. I'm grateful to Barry O'Neill for leading me to this literature and explaining it succinctly a number of years ago. See also Kahneman & Tversky (2000).
8. Fryklund (1962: 3) gave the population of the USA as 195 million. In all fairness, this outcome was an improvement over the 150 million dead he claimed would occur under older targeting strategies – but it would work only if the Russians aimed their weapons in the way *we* thought made the most sense.
9. According to Gamson (1975: 604), 'Goffman uses the term "frame" to refer to the set of rules governing a given type of activity. People normally adjust easily to the appropriate frame and operate within it without ever recognizing the principles involved'. An important strand of work on frames and social mobilization is based on Goffman; Benford & Snow (2000) provide a full review of the literature. Also, Turner (2003: 12) defines frames as 'conventional packets of knowledge', which usually include roles; some of Turner's illustrations are similar to Goffman's.
10. Not for attribution, conversation with Eden, 15 July 1988.
11. On taking a stand, see especially *Social Studies of Science*, Special Issue on "The Politics of SSK: Neutrality, Commitment and Beyond" (Ashmore & Richards, 1996); in that issue, I find especially congenial Jasanoff (1996).
12. See the nice exposition of the 'strong programme' in Woolgar (1988: 39–45).
13. The closing chapter can be read as: Reader, if you accept the premise of the book, what is this book a case of? I did not write the last chapter to further persuade the reader that nuclear fire damage is predictable – at least I did not think that was what I was up to at the time. Gusterson's reading is an interesting one.
14. I agree with Collins that scientific realism and methodological relativism, that is, a focus not on scientific arguments per se but on the social relations of science, need not be in conflict with each other; see Collins (2004: chapters 42–43).

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