

RESILIENCE AND TRUTH: A Response to Berkes

Paul Nadasdy

Department of Anthropology and the American Indian Program, Cornell
University
penadasdy@wisc.edu

In his thought-provoking paper, Fikret Berkes argues that conventional resource management is inadequate because it is rooted in a positivist approach to theorizing nature, an approach that casts nature and society as distinct, views nature as completely knowable through the use of rationalist-reductionist methods, and relies on outmoded assumptions about ecosystem stability and the linear relationship between social-environmental cause and effect. Because of such inadequacies, Berkes suggests, conventional resource management institutions are incapable of preventing – and likely even contribute to – the over-exploitation of natural resources. This is precisely what happened, he argues, in the spectacular collapse of the north Atlantic cod fishery, one of his principle examples. But there is hope. Over the past few decades, Berkes tells us, the science of ecology has undergone a paradigm shift from an equilibrium to a non-equilibrium perspective. He urges resource managers to embrace the new non-equilibrium paradigm and accept its theoretical and practical implications. This means rethinking such fundamental concepts as *management* and *natural resources* so as to bring them into better accordance with current ecological theory. To this end, he exhorts resource managers to adopt the conceptual tools (such as *resilience* and *adaptive management*) that he and his colleagues have developed for managing resources in a non-equilibrium world. Unfortunately, his analysis of the problem and so also his solution to it seem to me a bit too simplistic. Ironically, this causes him to adopt just the sort of positivist stance toward science (at least of the non-equilibrium variety) that he purports to eschew. To see what I mean, it is necessary to take a closer look at Berkes' account of the relationship between politics and knowledge production.

I have argued elsewhere (Nadasdy 2007) that most resilience theory deals inadequately, if at all, with questions of power. Although Berkes' paper does suggest that politics can play an important role in resource collapse, his account depends upon a problematic, if implicit, distinction between culture/politics on one hand and 'good' science/management on the other. For him, culture and politics are external to processes of science and scientific management and can thus play only a stymieing role. Culture and politics can contaminate science when, for example, scientists' own cultural assumptions and/or political agendas unduly influence the production of scientific knowledge, as was the case with now-outmoded models of ecosystem equilibrium (which is therefore an example of 'bad' science). Similarly, 'bad' management results when politicians and managers choose to ignore science altogether in their decision-making, as when they fail to implement adaptive management for political reasons. Implicit in Berkes' account

is the idea that old, now discredited science (for example, equilibrium ecology) was wrong because it was rooted in cultural beliefs (for example, about the separation between humans and the environment) and/or tainted by the political agendas of scientists themselves (for example, the environmentalism of 1930s ecologists, about which so much has been written). By contrast, Berkes implicitly casts non-equilibrium ecology as 'good' science, a true and value-free reflection of the world untainted by culture or politics. Thus, in Berkes' view, non-equilibrium ecology, unlike equilibrium ecology, can serve as the basis for proper management – if only scientific managers could be shielded from political influence.

In essence, then, Berkes presents us with a constructivist account of equilibrium-based ecology (that is, he sees it as socially constructed) and a positivist account of non-equilibrium ecology (that is, he views it as a true reflection of the world). As he himself argues, however, the latter stance is deeply problematic. For one thing, philosophers of science long ago rejected as untenable positivist accounts of scientific knowledge. What is more, as geographer David Demeritt (1994:32) notes, there are also political problems with positivist accounts of nature: 'exclusive knowledge claims serve only to divide, to empower a few anointed experts, and to exclude the many different voices that can and should speak – and that we need to hear – in the ongoing conversation about nature'. Demeritt argues that non-equilibrium ecology, 'like every other science, is a discourse with its own particular rules and disciplinary structures that produce representations of nature. These representations involve the exercise of power, and we should treat them as such' (*Ibid*:33). Power, in the view of Demeritt and many others, plays a *constitutive* (rather than contaminating) role in the production of *all* ecological knowledge (non-equilibrium ecology included). If this is so, then we can hardly accept Berkes' implicit claim that resource management based on non-equilibrium ecology will work because it reflects the way the world really is. Indeed, geographer William Adams (1997) has argued that the application of non-equilibrium ecology to environmental conservation and management has actually lead (through the creation of new technologies of knowledge-production like computer modeling and automated data collection) to an *increase* in reductionism and instrumental rationality, qualities Berkes identifies as especially problematic culturally-derived aspects of the old paradigm.¹

Berkes' implicit treatment of non-equilibrium ecology as 'good' (value-free) science and equilibrium science as 'bad' requires him to make a radical distinction between the two; hence his characterization of non-equilibrium ecology as resulting from a Kuhnian paradigm shift in the discipline. Although this Kuhnian narrative is common in the literature on resilience (including, to some extent, in my own writings), careful historical analysis of scientific ecology and resource management simply does not support it. Indeed, several scholars (Hagen 1992; Murdoch 1991; Norton 1996; Worster 1994) have argued that non-equilibrium ecology is not quite the radical paradigm shift its proponents claim it to be. In his nuanced history of scientific ecology, for example, historian Joel Hagen (1992:194-195) accuses Daniel Botkin and other champions of non-equilibrium ecology of caricaturing the theoretical views of their predecessors to make their own work seem more radical than it is.² In fact, he shows that late nineteenth century biolo-

gists and (most of) their successors in the discipline of ecology always sought to 'account for both stability and instability in the natural world ... Far from dogmatic adherence to naïve notions of equilibrium, [they] forged a set of flexible concepts for dealing with the evolutionary complexities of the modern world' (*Ibid*:3). What Botkin and others portray as a Kuhnian paradigm shift, Hagen suggests, is more properly viewed as a shift in emphasis.

Thus, we can hardly trace the failure of conventional resource management back to an irrational 'adherence to naïve notions of equilibrium' on the part of scientific ecologists, as some have suggested. Perhaps, then, it is resource managers, rather than ecologists, who have been guilty of *naïveté* when formulating their non-flexible equilibrium-dependent management strategies. Unfortunately, this alternate narrative does not hold up to historical scrutiny either. Aldo Leopold, widely regarded as the founder of scientific wildlife management in the us, was clear in his call for a holistic approach to management based on the insights of scientific ecology (Leopold 1939). Indeed, he was quite suspicious of the 'balance of nature' concept, which he saw as a holdover from pre-ecological ways of thinking. Although he acknowledged that the concept was 'commonly employed in describing the biota to laymen, ... ecologists among each other accept it only with reservations'. He went on:

To the ecological mind, balance of nature has its merits and also defects. Its merits are that it conceives of a collective total, that it imputes some utility to all species, and that it implies oscillations when balance is disturbed. Its defects are that there is only one point at which balance occurs, and that balance is normally static (*Ibid*:727).³

It is clear, then, that Leopold viewed the notion of equilibrium not as an inherent quality of nature, as the narrative of paradigm-shift would have it, but rather, as a flawed though potentially useful heuristic device for attempting to understand ecological processes. This suggests that he would have been quite open to discussions of resilience, multiple equilibrium states, and flexible management institutions like those discussed by Berkes. Indeed, Leopold's view of the complexity and dynamism of ecological processes – and of the complex ways these relate to social and economic processes – led him to articulate an approach to wildlife management that seems oddly anachronistic if one accepts the narrative of paradigm shift. In *Game Management*, he wrote:

The detail of any policy is an evanescent thing, quickly outdated by events, but the experimental approach to policy questions is a permanent thing, adaptable to new conditions as they arise. ... There are conflicting theories on how to bring the land, the means of payment, and the love of sport into productive relationship with each other. No one can confidently predict which theory is 'best'. The way to resolve differences is to bring all theories susceptible of local trial to the test of actual experience (Leopold 1933:411).

That Leopold, in 1933, could have so clearly articulated the principle of adaptive management, supposedly one of the cornerstones of management under the new non-equilibrium paradigm, calls into question just how new and radical the approach actually is. Nor can it be argued that Leopold was a visionary whose views were marginalized; *Game Management*, the foundational textbook in that emerging field, was widely used and extremely influential (see Nadasdy in press). All of this suggests that the failure of conventional resource management cannot be attributed simplistically to the positivistic and rational-reductionist aspects of a worn-out paradigm, nor can it be written off as an artifact of scientific managers' stubborn adherence to practices engendered by that paradigm. What is required instead is a more detailed and sophisticated analysis of the political dimensions of resource management science and its use.

An example of such an approach is Alan Finlayson's (1994) nuanced sociological analysis of knowledge-production among DFO fisheries biologists and their role in the collapse of the North Atlantic cod fishery. Finlayson shows that things were quite a bit more complex than Berkes makes them out to be in his own account of the collapse. To attribute the failure of the cod stocks to the rational-reductionist nature of positivist science (as Berkes does on page 26) simply does not do justice to the complexities of the scientific management process, nor is it fair to the fisheries biologists themselves, who had a much more sophisticated understanding of the situation (including, to some extent, of the constructed nature of their own knowledge) than Berkes' account suggests. Indeed, Finlayson makes it clear that doing fisheries science is itself an inherently political process. Politics and culture, in his view, are not merely obstacles to 'good' science and appropriate management; rather, they play a *constitutive* role in the production of scientific knowledge about fish. Arthur McEvoy (1986), whose work Berkes cites with approval, paints an equally complex picture of the interrelationships among marine ecosystems, fisheries science, society, and the law in California fisheries.⁴

So, although I agree with Berkes that we must view social-ecological systems in all their holistic complexity, it is important to bear in mind that science and scientists (even those who champion the non-equilibrium view) are embedded in those systems along with everyone else. Far from occupying some privileged epistemological position that gives them unique access to 'true' knowledge about the world, they, too, view the world from a particular socio-political position that both reflects and shapes the knowledge they produce. Thus, we cannot hold up non-equilibrium ecology as the 'truth' upon which resource management should be based. Rather, we must recognize that resilience theory, like all perspectives on the environment, is socio-politically situated and so necessarily reflects particular interests and political-economic configurations (Nadasdy 2007). This is not to say that non-equilibrium thinking is useless. It is merely to argue, with Demeritt (1994:33), that we should look to science 'not as a mirror to nature but as a useful tool for engaging our world critically'. To this end, we must attend closely to the political dimensions of *all* knowledge-production (including non-equilibrium ecology and associated management strategies) and to the 'material and discursive effects' of the different environmental narratives they engender.

Notes

- 1 Also see Nadasdy (2007) and Timmerman (1986) for analyses of the political-cultural underpinnings of non-equilibrium friendly concepts, such as resilience, advocated by Berkes and others.
- 2 Historian Donald Worster (1994:425) agrees, citing Arthur Lovejoy's observation that 'every age tends to exaggerate the scope and finality of its own discoveries, or rediscoveries, to be so dazzled by them that it fails to discern clearly their limitations and forgets aspects of truth of prior exaggerations against which it has revolted.'
- 3 See also Norton 1996 for a discussion of this passage.
- 4 Also along these lines, legal scholar Dan Tarlock (1994:1121) argues that adaptive management is difficult to implement not because of stubborn adherence to equilibrium models among resource managers but, rather, because adaptive management 'poses a profound challenge to our legal system because it undermines a core principle of procedural and substantive fairness: finality.'

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