Rejoinder

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I thank the three respondents for insightful comments leading to deepening the discussion. In reflecting on 'Living with Uncertainty and Adapting to Change', the 2009 conference theme, my paper ended up addressing a range of areas, starting with the ecological context of uncertainty and ending with a plea to reconceptualize the notions of 'natural resources' and 'management' using resilience as an organizing idea. It is difficult to do justice to an interdisciplinary mix of topics, especially when many of them are controversial and intractable. Coastal governance itself is a 'wicked problem', as Jentoft and Chuenpagdee (2009) might put it, one with no definitive formulation, no stopping rule and no test for a solution.

David Symes contributes a cautionary addendum to my paper, pointing out that much of European fisheries policy shows a continuing attachment to a conventional management approach and thus a far cry from the kind of thinking that resilience implies. Indeed, as articulated by *The Economist*, the conventional wisdom seems to be to seek even stronger individual fishing rights with expanded trade in these rights (Anonymous 2009). On the one hand, the trend here seems to be for the *increased* commodification of resources. On the other hand, other European Union policies are surely in the direction of protection of marine ecosystems and ecosystem services for human well-being, as a revised concept of 'resources' would require. Further, as David Symes points out, for small-scale coastal fleets, European Union policies specify that decision-making should be made as close as possible to the community, providing an entry point for a resilience oriented approach. This subsidiarity principle, emphasizing the continuing importance of individual persons and local systems in the vast and globalized world, is a unique product of European history and philosophy (O'Brien 2008).

David Symes correctly notes that my thinking seems to be influenced more by the fishery experience in developing countries than the developed. My paper is neither North America nor Euro-centric. In fact, the complexity of European fisheries scares me, for all the reasons that David Symes explains. Rather, I am more interested in the fisheries of South and Southeast Asia and Central and South America, for two reasons. First, that is where the vast majority of the world's fishers are. Second, I think of small-scale, community-based fisheries as a 'laboratory' in which some of the key issues can be explored. These include issues such as multi-level governance, dealing with shrinking space and time scales in a globalized world (Berkes *et al*. 2006), collaborative management, and use of local and indigenous knowledge for management and for marine environmental monitoring (Berkes *et al*. 2007).

Paul Nadasdy points out that 'doing fisheries science is itself an inherently political process' and argues that 'we must attend closely to the political dimen-

sion of *all* knowledge-production ...'. That seems fair enough. I think Paul Nadasdy and I also agree that resilience theory deals inadequately, if at all, with power. That is why I spent considerable time and effort on the politics of management, starting with the historical disempowerment of the community, *Gemeinschaft* (I had to do battle with one referee who recommended I take out all of that contextual material). Resilience theory does not start with the Age of Enlightenment or the historical disempowerment of the community but with ecological history. It starts with the observation that the ideas of stability and balance do not fit with the findings of half a century of ecological research. But resilience theory is not responsible for the demise of equilibrium thinking. That started a long time ago, as Paul Nadasdy correctly points out, including Aldo Leopold's observations in the 1930s on the 'balance of nature', and was almost complete by the time resilience ideas emerged in the 1970s.

Given that the resilience approach emphasizes cycles, uncertainty, threshold effects, change, transitions, scales ('panarchy'), and social-ecological systems (like fisheries) as coupled and co-evolving systems, it is puzzling that Paul Nadasdy chooses to talk endlessly about non-equilibrium ecology. Equilibrium/non-equilibrium ecology is a historically important argument regarding the origins of scientific resource management, but it is only a minor part of the argument about resilience as a theoretical basis for the reconceptualization of 'natural resources' and 'management'. As well, Paul Nadasdy attributes some very strange language to my paper. First of all, I do not use terms such as 'true knowledge', and I do not talk about non-equilibrium ecology as 'good' (value-free) and equilibrium ecology as 'bad' science. Neither of them is value-free, and neither can be characterized as good or bad science. Equilibrium ecology is merely 'old' science, from the days when ecology was natural history.

Second, my argument is that equilibrium ecology is the basis of fishery yield models that developed in the 1950s onwards, and that these models seem to persist into present day. Their development was a response to the 'overfishing problem' (Sáenz -Arroyo and Roberts 2008), thus probably driven by history, economics and biology, rather than by politics – although one can be sure that there were political dimensions as well. However, I think it would be overly simplistic to attribute the persistence of inappropriate management models to merely 'politics'. Many factors are at play here, including (yes) 'managers' stubborn adherence to practices engendered by that [equilibrium] paradigm.' Paul Nadasdy argues that there was 'a shift of emphasis' regarding equilibrium, not a Kuhnian paradigm shift. I am not so sure about that. Resource management, not just in fisheries but in many areas, has been undergoing major change, and to many observers, it sure looks like paradigm change (Lertzman 2009).

Third, resilience theory is not wedded to non-equilibrium ecology, as Paul Nadasdy should know. In fact, much of the theory development in resilience was based on systems exhibiting two or many equilibrium points, such as Marten Scheffer's Dutch lakes (Scheffer 2009). The critical point here is not lack of equilibrium but rather the existence of threshold effects and abrupt transitions under *apparent* conditions of stability and equilibrium. These ideas are being applied to problems such as climate change adaptation, and are receiving attention from

diverse areas (Marten Scheffer won the 2009 NWO/Spinoza Prize for his resilience work, the highest science award in the Netherlands). Is this relevant to fisheries social science? The answer is yes, especially if you are looking at some of the systemic problems of coastal management. It is worth repeating my favourite example. The 2004 Asian tsunami was a natural disaster, but its impacts were amplified by increased vulnerability due to poor environmental management. The expansion of shrimp aquaculture for global markets was partially responsible for the reduction of the coastal vegetation that acted as a natural buffer (Adger *et al.* 2005). Other applications have included studies of fisher community resilience to shocks and stresses (Marschke and Berkes 2006), and analyses of variability in marine ecosystems and the response of fisher livelihood systems to such variability (Perry and Sumaila 2007). These applications are of interest to social scientists and are not easily dismissed.

Douglas Wilson offers much food for thought and I am going to comment on two of his points. He argues that 'human ecology needs a clear conceptual separation between society and nature, something that their deep interconnection makes problematic'. There are two sides to this argument. On the one hand, we need to seek a unity of people and environment, nature and culture, as I have argued following Gregory Bateson. That would require reconnecting natural science, social science, and humanities perspectives on people and the sea. Rather than seeing the biophysical and the social as separate and distinct systems, the two could be considered as coupled and interdependent. On the other hand, there are serious barriers to such an approach. It would require reconciling disciplines with very different scholarly traditions, assumptions and methodological approaches. Thus, just on the basis of our disciplinary educational systems alone, much of the work on people and the sea will still be carried out by researchers pursuing their disciplinary traditions. However, Doug Wilson's point goes further: 'the first step to understanding the interactions and mutual adaptations among systems is to clearly define their boundaries'. This poses a paradox. If social-ecological systems are indeed co-evolving, it would be very difficult to 'freeze' them in space and time to define their boundaries, only to find that they have moved on.

Douglas Wilson's other important point is about the need for a systemic critical theory of social resilience. He considers that the Frankfurt School of Critical Theory can make a particularly important contribution to resilience thinking. Part of the argument here is that resilience needs systematic linkages between kinds of rationality, science, knowledge, ideology and power, as in Jürgen Habermas' communicative systems theory. It would be fascinating indeed to develop a theory of communicative rationality for alternative fisheries management, Habermas-style (with or without resilience in the mix), as opposed to the conventional rationalist tradition in fisheries management. I am thinking of objectives of efficiency, calculability, predictability, and control in conventional fisheries management, augmented by privatized fishing rights and fully transferable ITQs, European Union style. What we have here is a veritable McDonald's of rationalist tradition, reducing the fisher to part of the assembly line by which the machinery of the fishery runs itself. There is minimal scope for individual decision, or options to switch species or gears, or 'communicative action' among fishers and

between fishers and managers, or incentives for resource stewardship. It is this vision of the 'McDonaldization' of industrialized world fisheries that reminds me why I would rather work with developing country fishing communities, and I thank Douglas Wilson for triggering the insight.

My main argument in the paper was for a rethinking and redefining of the notions of 'natural resources' and 'management'. I am pleased that the main argument seems to have survived the sharp scrutiny of the three respondents. There are no doubt a number of ways to get there. I proposed using resilience as an organizing idea. But we can also consider the contributions of critical theory (Douglas Wilson) and political ecology (Paul Nadasdy), or using interactive governance (Kooiman *et al*. 2005, masterfully critiqued by Symes 2005) as alternative ways to get there. The choice of any of these paths involves interdisciplinarity, increasingly necessary in a globalized world in which the impacts of diverse drivers can emerge independent of the place where they were produced. Engaging with such global issues will require collaborating internationally with other disciplines to interpret causes, deal with consequences, and design alternative policies.

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