Lansing—Response to Critics

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This article is the latest (but I suspect not the last) word in a debate of long standing with historical origins in two places, one empirical, the other theoretical. The first is the response, both impressed and mystified, of Dutch colonial administrators and scholars when they first encountered Bali's extraordinarily sophisticated and efficient system of water-management for irrigation of rice-fields. The other is Wittfogel's famous thesis, derived from Marx, of "oriental despotism" in which state power is linked to control over irrigation systems. Both debates were conjoined and revived in the 1980s by one of the present authors, Stephen Lansing, with new evidence that supported a new and comprehensive model of a system of water, fields and planting schedules, organising themselves from the bottom up into watershed-wide systems by way of cycles of ritual in hierarchies of temples. Since then Lansing has expanded his research in a series of interdisciplinary collaborations producing an impressive array of additional evidence which has been used to refine and strengthen his model.

Lansing's¹ model immediately attracted worldwide attention and has been widely cited, especially as an example of the ecological wisdom inherent in traditional social/cultural arrangements. His work has however had its critics, many of them specialists on Bali, who have tended to be more sceptical of both his evidence and his conclusions. The core of debate is the question of the extent to which irrigation was historically organised from the bottom up, by a system

of self-coordination of local irrigation groups (*subak*) or conversely, from the top-down, by the ruling elites of pre-colonial times. As such it has implications for thinking both about issues of resource management and about the relationship between centralised states and decentralised democratic formations. This question and indeed the whole debate have been further complicated by the fact that they have been approached from different angles by different writers; ranging from those concerned primarily with matters of ecology, irrigation and resource management, to those concerned more with political organisation. The debate had a thorough airing in *Current Anthropology* (44[3] 2003), but has never been conclusively resolved. It was revived and brought into the pages of *HE* by Henk Schulte-Nordholt's article in 2011 (39: 21–27) in a special issue devoted to *subak*.

The present article is Lansing's response to that and other critiques over the past decade. The critiques it addresses come from opposite ends of the spectrum: one of fine data oriented to the ecological base of the model, the other of interpretation of historical evidence related to the political implications of the model. Lansing's response simply address the critiques at these levels, ultimately dismissing their significance for the model as a whole. But as Thomas Reuter pointed out in the *Current Anthropology* discussion, it is unlikely that any new evidence will resolve the debate and there is "a need to exercise some common sense".

While this debate has undeniable significance both in the small and somewhat rarefied world of Bali studies and the larger domains of social, political and ecological studies to which it also speaks, I think there are limits to the utility of its ever-finer litigation by way of publication and counter-publication. A reliable warning sign of this is an uncomfortable drift close to the borderline of *ad hominem* attack in some of the contributions to the *Current Anthropology* debate.

¹ My use of Lansing's name, in the singular, is somewhat loose, in that much of his work, including this article, is published jointly with his many interdisciplinary collaborators. But it is also deliberate, in that the argument was and has always remained primarily his.

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For many of us with experience in Bali (in my case both from both ends of the issue - the ricefields and the palace), there is clearly solid evidence and merit in both sides of the argument. Lansing's experience in Bali is long and deep, his research systematic and innovative and his analysis ambitious, exciting and generally convincing, especially at the level of its sweeping overview and wide implications. His (perhaps inevitable) lack of attention to (often small and trivial) ethnographic detail, does however remind us of the level at which he is working and that his model is, as he reminds us in this article, just a model, with all the caveats thus implied.

On the other side, the experience of some of his critics (especially Henk Schulte-Nordholt) is equally long and deep, and their evidence and argument equally convincing. There seems little reason to doubt (for example) Schulte-Nordholt's evidence of royal involvement in irrigation from Mengwi, and while this may be evidence of the empirical limits of Lansing's model, it does not appear to me to necessarily discredit it, let alone diminish its usefulness. Likewise Lansing's attempt to discredit Schulte-Nordholt's evidence seems somewhat misplaced. In other cases (e.g. Lansing's debate with Brigitte Hauser-Schaublin) the proponents appear to be simply talking past each other, their attachments to their different theoretical positions and interpretations of evidence preventing them from seeing the merit in each other's arguments.

One thing that the polarised positions of this debate seem to have obscured is the possibility that more or less centralised control of irrigation by (micro-)states and by self-organising systems of temple and *subak* could have co-existed in precolonial times (as indeed they do today in a slightly different way). This could have been by way of different systems in different ecological and/or political environments. It could alternatively have been by way of periodic oscillation or dynamic equilibrium between the two, somewhat in the manner described by Edmund Leach in *Political Systems of Highland Burma*. It could even have been by combinations of the two in one area. This may not be the most satisfying, elegant or exciting resolution of the debate, but given the evidence available, nor does it seem the least likely.

But, to return to Thomas Reuter's advice, let us not forget the presently precarious positions of, on one hand, the global crisis of environment and resource management and on the other, of anthropology, and indeed all the social sciences and humanities, in the contemporary academic economy. I cannot help wondering whether the prodigious talents and energies of our finest scholars could not be better utilised in more cooperative approaches to the problems of the "real" world. This might in turn lead to those who make decisions in that world to take more seriously the value and potential of anthropology. The pages of *Human Ecology* are not a bad place to start this work.