

Environmental thresholds and the empirical reality of state collapse: a response to Erickson (1999)

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We are grateful for the opportunity to respond to Erickson's (1999) critique of our articles on human–environment interactions in the Lake Titicaca basin of Bolivia (Ortloff & Kolata 1993; Abbott *et al.* 1997; Binford *et al.* 1997). His decision to publish this critique in *ANTIQUITY*, rather than in the journals in which our articles appeared, permits us to reach a broader audience. Erickson labels our interpretations a form of 'neo-environmental determinism', but his rejection of our conclusions stems from serious misunderstandings and is misleading to readers who have not examined our original data. He (p. 634) claims:

- 1 our research represents 'simplistic reductionist thinking' that treats humans as 'passive pawns' of environmental change;
- 2 our dating of the chronic drought in the Andean altiplano after AD 1150 is imprecise and not correlated with the 12th-century disintegration of the Tiwanaku state; and
- 3 the drought did not affect intensive agricultural production.

We address these claims in reverse order.

Erickson confuses the distinction between short-term and long-term impacts of precipitation change when invoking modern drought records. He states (p. 637) that 'many of these historic droughts and floods are on a par with' the profound climate change that we documented between the 12th and 15th centuries. However, these were short-term dry spells (2–5 years) with lake-level fluctuations of 2–3 m around the 20th-century mean. The cited 17th- & 18th-century droughts were only 1–3 dec-

ades in duration. Erickson fails to grasp the order-of-magnitude difference in duration and ecological impacts between these short-term events and the centuries-long drought we documented. He then uses comparisons of cultural response to modern droughts to argue that 12th-century populations would have readily coped with protracted drought, but his ethnographic analogies are invalid. Contemporary agricultural infrastructures and technologies are significantly different from those of the Tiwanaku period: modern populations do not depend on water intensive, raised-field agriculture. Demographic conditions and socio-economic structures of the pre-Hispanic and modern periods are incommensurate. Erickson's analysis assumes that the political economies of these periods are comparable. Structural disjunctions between 12th- and 20th-century rural society far exceed any assumed continuity.

According to Erickson (p. 638), the 'severe drought of 1983–84 provided an excellent test of raised-field agriculture'. In reality, this brief dry spell constitutes no test at all. The level of Lake Titicaca remained near the 20th-century mean throughout this 'severe drought'. Lake level did not fall 15–20 m as it did during the 12th–15th-century drought (Binford & Kolata 1996; Abbott *et al.* 1997; Binford *et al.* 1997: 239–40, figure 7). Although raised fields mitigate the impacts of short-term dry spells, they cannot sustain production under chronic aridity.

Erickson asserts (pp. 637–8) that raised fields could be cultivated on lands exposed by the receding lake during drought conditions. His

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comments reveal a deep misunderstanding of water budgets and their agricultural consequences. Water in a drainage basin is finite. Each year, some water enters the system through rainfall and shallow groundwater; some is lost to evapotranspiration, seepage to deep aquifers, and outflow; and some remains in the basin. When input decreases but output remains the same for several years, water volume necessarily decreases and lake level declines. Decreased infiltration to shallow groundwater causes a longer-term reduction in spring and river flow, the major sources of water for raised fields. The area of land that seems suitable for raised-field cultivation does increase with lowered lake level, but water available to fill canals significantly decreases. Fresh water is the fundamental condition for the ecological functioning of the raised fields (Biesboer *et al.* 1999; Carney *et al.* 1993; Orloff & Kolata 1993; Kolata & Orloff 1996). We demonstrated that during persistent droughts, raised fields on such newly exposed land would have been subject to decreased soil moisture, increased salinity and increased susceptibility to frost damage (Binford *et al.* 1997: 243–5).

Erickson further (p. 640) remarks that periods of drought 'are *always* followed by periods of heavy rainfall' (his *italics*). In effect he argues that protracted drought is not a problem because there will be high-rainfall events during the episode. This is like saying, 'don't worry about long-term starvation, because you'll have a feast every now and then'. Even if the climate was so predictable, our water budget models demonstrate that short periods of heavy precipitation do not mitigate centuries-scale drought.

With respect to the onset of climate change in the 12th century, Erickson (p. 635) erroneously claims that the 'chronology of the drought is far from precise' because the 'AMS samples used to date the drought were freshwater snails and marsh reed seeds, possibly not the best class of organic material to date a drought'. We used both organic C (achinete) and inorganic C (snail) samples for sediment dating, tested the magnitude of the 'reservoir effect' using ^{210}Pb , and adjusted the dates accordingly (Abbott *et al.* 1997: 172). The 60 AMS dates from the lake-sediment cores are well ordered stratigraphically (Abbott *et al.* 1997: figure 3) and agree with previous studies on lake sedimentation (Wirmann & Oliveira Almeida 1987). Erickson

mistakenly conflates five different sediment hiatuses indicative of low lake stands during the past 3500 years to conclude that the 12th-century onset of drought was poorly dated (Abbott *et al.* 1997: figure 3). His related statement (p. 635) that the collapse of Tiwanaku's urban centres occurred '100–200 years before the supposed onset of the drought' is a selective reading of our data. Initially, we estimated terminal dates for domestic occupation in Tiwanaku urban centres between c. AD 900 and 1100 (Orloff & Kolata 1993: 196). Subsequently, we refined our estimate of terminal Tiwanaku occupation in rural and urban settings to c. AD 1100–1200 with numerous new radiocarbon dates from lake cores and more extensive archaeological excavations (Binford *et al.* 1997). Drought-induced settlement abandonment occurred at some point after these terminal dates.

Within the precision of AMS techniques, our data demonstrate a strong temporal correlation between social, agroecological and climate changes in the Andean altiplano. The best hypothesis implicates the AD 12th–15th-century drought in the disintegration of the Tiwanaku state. Erickson's (p. 641) claim that raised-field agriculture expanded rather than contracted in the post-Tiwanaku period ignores our extensive empirical evidence which is fundamentally at odds with interpretations in his Ph.D dissertation (Erickson 1988).

Finally, Erickson (p. 634) labels our work 'simplistic', 'reductionist' and an 'extreme incarnation of neo-environmental determinism'. Contrary to Erickson's misrepresentation of our work, we believe human communities actively respond to and cause environmental change. We never proposed that the 12th-century disintegration of the Tiwanaku state represented a 'total cultural collapse, plunging the Lake Titicaca basin into a post-Tiwanaku "Dark Ages"' (p. 635). We simply maintain that in some instances extreme environmental conditions reach thresholds beyond which complex forms of social organization (states, urban centres) cannot persevere. One human response to such extreme conditions is to reduce social complexity by abandoning unsustainable urban centres and reorganizing systems of production.

The intemperate language in Erickson's critique inhibits rational discourse. Erickson's notion that an environmental component to the explanation of cultural change is 'neither ad-

equate nor satisfactory for anthropology' is an invitation to intentional ignorance. A substantive critique of our results will require new palaeoenvironmental and archaeological data

that contests our empirical evidence rather than simple assertions drawn from a pastiche of ethnographic analogy and currently fashionable theory.

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