Bemoaning the "lack of domesticated rice dating to 4,000–3,000 years ago," the authors fail to note the lack of searching with appropriate techniques. Little phytolith or starch research has yet been done in ISEA, and the archaeology is dominated by caves, reluctant locales for food production. Coastal Neolithic village sites in ISEA are likely to be buried beneath many meters of redeposited sediment (Bellwood et al. 2008). Lack of evidence in circumstances lacking suitable recovery techniques cannot be proof of absence.

The Austronesian cognate vocabulary for food production and increasing numbers of reports of rice in pottery and as phytoliths, even when identifiable charcoal macroremains are absent, leave little room for any nonfarming argument for early MP-speaking colonists. Suggestions that bananas, sugarcane, greater yam, and sago might have been domesticated in Melanesia, especially New Guinea, are not here disputed, but the only "archaeological evidence" presented for their early domesticated occurrence in ISEA consists of a controversial claim for banana phytoliths in a site in Uganda.

The paper finishes by denigrating the ability of the farming/language dispersal hypothesis to account for the distribution of genes, languages, and material culture across ISEA over the past 4,000 years. But this hypothesis remains the best explanation for the observed situation (Bellwood 2009). The MP language spread occurred mainly as a result of population movement (Ross 2008), not elite dominance.

Murray P. Cox and J. Stephen Lansing
Institute of Molecular BioSciences, Massey University, Palmerston North 4442, New Zealand (m.p.cox@massey.ac.nz)/Department of Anthropology, University of Arizona, Tucson, Arizona 85721, U.S.A. (lansing@santafe.edu). 28 V 09

As the influential statistician George Box once noted, "all models are wrong, but some are useful" (Box 1979:202). This statement is not mere semantics: models are by necessity simplifications of the real world; they can be considered useful only if they tell us something new. While Donohue and Denham rightly warn about the dangers of entrenched views, their own model of Indo-Pacific prehistory is neither new nor simple, nor, more importantly, is it particularly useful.

Despite claiming differences of opinion, Donohue and Denham's idea that "material culture [and language and genes] dispersed through ISEA from multiple sources along a mosaic of regional networks" is remarkably similar to earlier themes. As far back as 1988, John Terrell advocated a related outlook: that Indo-Pacific prehistory revolved around "interlocking, expanding, sometimes contracting and ever-changing set of social, political, and economic subfields" (Terrell 1988: 647). Such general concepts have understandable appeal: they capture the complexity of human society in a way that simple models do not. Unfortunately, such generalities are not necessarily right, and, more importantly, they are not often very useful.

A good model should be simple (i.e., reduce the complexity of the world), explicit (i.e., use stable, well-defined concepts), and, most importantly, testable (i.e., have clearly defined outcomes). These ideals are not readily apparent in Donohue and Denham's work, primarily because concepts like a "mosaic of regional networks" are not sufficiently concrete. Did Indo-Pacific populations interact equally across geography and time? (Greenhill and Gray [2005:34] would call this a "maximally interconnected network.") Or, as we suspect Donohue and Denham intend instead, did Indo-Pacific populations interact in very specific ways at very specific times in very specific places? If so, when exactly were those times, and what exactly were those places?

Herein lies the rub. The out-of-Taiwan model is so easy to criticize precisely because it is relatively simple, explicit, and testable. It has clear expectations: we can distinguish when data fit the model and when they do not. Conversely, Donohue and Denham's model is sufficiently imprecise that it cannot readily be evaluated. The charge leveled against Terrell's earlier incarnation applies: such an "anything goes" model risks being "vague to the point of uselessness" (Lum's comment in Terrell, Kelly, and Rainbird 2001:116).

By way of example, the out-of-Taiwan model is commonly criticized for its insistence on correspondences between interdisciplinary data sets (such as genetics and language). Amusingly, the initial stimulus behind this now-lengthy debate (Terrell and Fagan 1975) was the finding of just such a statistical association between γ-immunoglobulin diversity and language affiliation in New Guinea (Giles, Ogan, and Steinberg 1965). However, this discovery was no mere fluke: over the past 40 years, similar associations have been found on geographical scales both large (e.g., Cavalli-Sforza et al. 1988) and small (e.g., Lansing et al. 2007).

The latter case is particularly informative: the study examined genetic and linguistic diversity in 532 men from eight small communities on Sumba, an island in east Indonesia less than 200 km across. Under the out-of-Taiwan model, we might expect a rapid Austronesian expansion to have left specific signals in modern communities: they should carry some Asian contribution and have a mid-Holocene origin, and we might also observe remnants of shared linguistic/genetic heritage. This is exactly what we find. Around a quarter of Sumbanese men carry Asian Y chromosome lineages; Bayesian coalescent inference, a particularly robust form of molecular dating, indicates that their villages are less than 5,000 years old; and the proportion of male Asian ancestry correlates significantly with villages' retention of proto-Austronesian cognates. Expectations under Donohue and Denham's model are much less clear, but it seems unlikely that long-standing regional networks could produce a combination of patterns quite like these.

For those who dislike the tenacity of the out-of-Taiwan model, the reason seems obvious: there is no meaningful al-

ternative. Clearly, some early ideas were wrong (any expansion did not result in complete replacement), but equally clearly, some ideas were right (several genetic markers *do* trace back to Taiwan). Rather than yet another personal take on Pacific prehistory, what we need now are good alternative models: simple, explicit, and testable. What expectations do these produce for Sumba? How do expectations differ between models? No matter if these new models turn out to be wrong; in the meantime, it would at least be very helpful if they were useful.

Waruno Mahdi
Fritz Haber Institute of the Max Planck Society, Faradayweg 4-6, D-14195 Berlin, Germany (mahdi@fhi-berlin.mpg.de). 9 VI 09

Donohue and Denham present a challenging reappraisal of the Malayo-Polynesian dispersal, also reexamining the role of non-Austronesians in ISEA culture history and reinspecting the farming/language dispersal hypothesis. The Malayo-Polynesian dispersal hypothesis adapted repeatedly to newer insights: a successively developing "out-of-Indochina" model (Heine-Geldern 1932; Kern 1889; Schmidt 1906) gave way to the "out-of-Taiwan" (Chang, Grace, and Solheim 1964) model currently under discussion. The dispersal is a baffling subject, having been linear in neither its linguistic, genetic, nor cultural aspect. However, if the manifestations and consequences of that nonlinearity previously remained inadequately appreciated, the present treatment perhaps oversimplifies them.

Modern humans inhabited ISEA long before the Holocene, but the rising sea (14,000–10,000 BP; Dunn 1970) caused migrations from inundated lowlands. Besides increasing population diversity and density (encouraging plant domestication) at both ends, Indochina and New Guinea, retreating populations would often be trapped on temporary islands; only those who learned to cross the sea survived (Mahdi 1988). The authors thus rightly stress early to mid-Holocene development of maritime interaction and the resulting diffusion of cultural artifacts and irregular distribution of genetic markers. Equatorial populations of inundated regions also reached Taiwan, implying that the later Malayo-Polynesian dispersal included their descendants. Whether genetic traces of this southward movement are discernible before the background of their earlier northward migration is unclear.

In terms of former racial concepts, ethnologists distinguished darker-skinned "Proto-Malays" and lighter "Deutero-Malays," as having, respectively, a greater and a lesser equatorial admixture (cf. Bylmer 1943). A similar distinction was made by the Malayo-Polynesians, with two distinct protoforms for "person" (Mahdi 1994*b*). Intensive contacts emanating from western ISEA since 2000 BP could explain why distribution of genetic markers linking Polynesia with Taiwan "is not prominent in ISEA." Nevertheless, indigenous Taiwanese are reported to be genetically closer to the population of ISEA and Oceania than to that of the mainland (Lin et al. 2005).

Language subgrouping based on exclusively shared innovations requires that the considered cognate sets include only inherited forms (not borrowings). But Malayo-Polynesians are notorious for long-distance contacts, and there was, moreover, a systematic source of noise. Malay-speaking seamen spanned ISEA since 200 BCE–200 CE (Solheim 1980)—transporting spices from Maluku to the Malayan Peninsula, India, and China—and sailed through the Philippines to China before 300 CE (Mahdi 1994*a*, 1999*a*, 1999*b*). This resulted in borrowings across group boundaries and in misleading secondary sound correspondences, with fateful consequences for eliciting a structured phylogeny of Western and Central Malayo-Polynesian. Until this is conclusively resolved, it inevitably creates a misleading impression of "propagation of Malayo-Polynesian languages . . . without direction or hierarchy."

Borrowing also camouflages the separate status of Central-Eastern Malayo-Polynesian: several "almost-exclusive" innovations have cognates only in Sulawesi and the Philippines, but their distribution suggests that they represent a remnant substratum of the former southward movement of Central-Eastern Malayo-Polynesians (Mahdi 1994*b*). Malayo-Polynesian is only one of several highest-level branches of Austronesian, and a "significant break between PAN and PMP" is relevant only if demonstrated to be substantially greater than those between PAN and Atayalic or between PAN and Proto-Tai-Kadai (grouped under PAN; Sagart 2005). So altogether, the apparent inconsistencies in the linguistic picture do not disprove an out-of-Taiwan dispersal.

With regard to the material-culture record, equatorial climatic conditions required significant adaptations. The daily photoperiod was too short for *japonica* rice (Grist 1959), while Zimmermann (1992) concluded that rice-cultivating "Proto-Malays" settled the highlands because of the more suitable soil (Mahdi 1994*b*). Adapting the rice variety to the tropics and littoral immigrants to a highland habitat required time. Malayo-Polynesians of the first wave depended on locally available staples, learning from the indigens they encountered. The authors correctly stress the role of the latter in domestication and distribution of the cultigens but perhaps underestimate the adaptive ability of Malayo-Polynesians.

Farming by Malayo-Polynesians as well as by indigens must have been the decisive factor underlying maritime contact and exchange. Thus, stressing the role of indigens and the importance of contact and exchange does not dismantle the farming/language dispersal hypothesis altogether but modifies it, by appreciating the reciprocal role of indigenous farming.

Watercraft likewise gained central importance, but the outrigger canoe was not the original Malayo-Polynesian watercraft. That was the double canoe (Doran 1981; Mahdi 1999*a*; also Mahdi 1988, 1994*b*). A distribution area, beginning in China (Mahdi 1994*b*, 1999*a*; Needham, Wang, and Lu 1971), stretches over ISEA to Oceania and also to India and Sri Lanka