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FURTHER OBSERVATIONS ON THE KUUMALA: BASED ON SOME SCEPTICS' PERCEPTIONS AS FEEDBACK

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In his book on the Burgess Shale, the late Stephen J. Gould (1989) taught many that in open biological systems historical contingency set things off along one trajectory, and this precluded a re-run of history along other possible paths. He further developed the theme in his authoritative *The Structure of Evolutionary Theory* (2002). This is also the case for open cultural historical systems as well (Kirch and Green 2001: 8), so that unexpected contingencies often set things off on a path that seems counter-intuitive at first sight. This position, of course, is in direct conflict with the universalist law-and-order school of the 'new archaeology' and its advocacy of mid-range generalisations providing a uniformly predictable range of repeated outcomes (Kirch and Green 2001: 3). In the following observations, as the result of feedback and commentary from colleagues on my essay "Sweet potato transfers in Polynesian prehistory" (Green 2005), I attempt to illustrate the differences in these two approaches.

At the onset I introduce what appears a rather delightful strategy, one it is my hope might become widespread not only in the field of *Evolution in Four Dimensions*, but also in Pacific archaeology.

Each chapter ends with a "dialogue," and the whole of chapter 10 takes this form. We use these dialogues as a device to enable us to reiterate some of the tricky points in our arguments, and to highlight areas of uncertainty and issues that are contentious. The participants in the dialogues are M.E. (who represents the authors, Marion Lamb and Eva Jablonka) and someone who could have been called the devil's advocate, but who, in order to avoid the negative connotations of that term, we have chosen to call Ifcha Mistabra (I.M. for short). *Ifcha Mistabra* is Aramaic for "the opposite conjecture." It is a term that embodies the

argumentative dialogue style used in the Talmud, in which arguments are countered and contradicted, and through this dialectic a better understanding of the subject is reached. The book can be read without the dialogues, but we think that readers may find them interesting and helpful, because they reflect many of the questions and concerns that our students and others have raised when we have spoken about our evolutionary views (Jablonka and Lamb 2005: 2–3).

William R. Dickinson, known to most Pacific archaeologists as Bill, has been involved in temper studies and other geomorphological problems they encounter, for decades. However, he is also a world-class geologist at the very top of his field. Hence, when out of the blue, he sends the following email on 2 November 2005, it started the thinking process – no, I did not really cover those issues as thoroughly as I might have. Better have another look at them. This was his message:

Well, I got myself the Ethnology Sweet Potato Monograph, and perused it with fascination... Had no previous appreciation of the camote-batata-kumara circuitry... Some thoughts.

First off, it being rare for me to find occasions to express full appreciation of your science, let me say that your article is by far the best. It has the hallmarks of combined balance and vigor, rigor and freethinking, review and prospectus that make it vintage Green. Marvelously stimulating and fully authoritative.

Second, I was struck by the apparently sound appraisal (and not just by you) that Polynesians made it to South America and back again (and prior to AD 1000 if the Mangaia data is sound). If they could do that (hearking back to my argument about pottery-makers in the Marquesas), it would seem relatively trivial to get out to the Marquesas before settling islands farther west.

Third (and on the other hand), the southern Cook story makes me nervous. Can it really be that people had sweet potato on Mangaia hundreds of years before any got to Rarotonga? That is a mental stretch for me (and cannot see how the makateas could be the answer, as no one tends to garden them even today). Maybe sweet potato came to the Cooks and then died out for a while (but that would be odd, given its utility). Looked back at Hather and Kirch, and no way I have the expertise to really evaluate what they say, but it seems to me (following the soundest of scientific principles) that the story of really old sweet potato in the Cooks badly needs replication (the whole time frame of the kumara circuit rests heavily on that one article, now 15 years old with no independent confirmation).

I did note, however, that only one of their three charred sweet potato occurrences can be associated with an AD 1000 date (and that sample is just said to be above the AD 1000 horizon). The other two are just bracketed at the top by about AD 1400. Maybe this opens the door to envisioning that a lot of touring around to Hawaii, Easter, South America, and New Zealand (including acquisition and dispersal of kumara) happened in the same time frame only a couple-three hundred years before European contact. Not so good for my Marquesan speculations (which you doubt anyway), but might ease the Cook sweet potato conundrum. Current conventional wisdom would seem to require that Polynesians got to South America and back to Mangaia some hundreds of years before going to Hawaii and New Zealand. Not sure what Geoff Irwin would think of that scenario, but it grates a bit on my nerves.

No offense intended to you or Pat (skepticism is never out of order, so long as kept close to the vest, because it can lead at times to better appraisals).

In the best sense of the concept, Bill is, in my view, playing the role of *Ifcha Mistabra*. What is the response of M.E. as a cultural historian who wrote the essay he is evaluating? Can the dialogue along the lines of M.E./I.M. on the matters raised further improve our understanding?

I.M. You say that current conventional wisdom would seem to require that Polynesians got to South America and back to Mangaia some hundreds of years before going to Hawaii and New Zealand. Not sure what Geoff Irwin would think of that scenario, but it grates a bit on my nerves. M.E. And also most certainly on mine, for instance where I too note that among some 40 ¹⁴C determinations that apply to these cave deposits with some for the deposits above the basal levels with first of the tuber rootlets in them, that would provide dates for these deposits (Green 2003: 58). In short, we both adopt the same stance about these dates – we have to await the publication of the pertinent ¹⁴C determinations. It would seem far more likely the Polynesians who went to South America got back to the Marquesas on a relatively trivial and highly likely return voyage before settling into any islands to the west. The sweet potato did thereafter diffuse through continuous interaction in the core region from the Marquesas through the Society Islands to the Southern Cooks (there is after all hard evidence for other items moving within this core attested in the models of Rolett (1996: 533, Figure 1) and Weisler (1998: 521–532), and the transfer of the sweet potato to the west would be one of the kinds of soft evidence that has to be inferred on that basis rather than direct evidence). And as for what Geoff Irwin may think about these matters, one can shortly read his essay (Irwin in press) about it in a forthcoming volume. It would seem

the main function of the Mangaia physical evidence is to provide us with one ^{14}C based estimate of time depth for the sweet potato in the core region, and as I.M. legitimately indicates, this 15-year-old estimate currently continues to stand alone with no independent confirmation. Fortunately, the news on the network is that Patrick Kirch and David Steadman have plans to finish writing up and publishing their Mangaia investigations this year, and that will assist in refining some of the rather unsatisfactory AD 1000 generalisations often found in the literature that are almost certainly in error.

In the kuumala essay I thought most available sources on missionary transfers and missionary preferences for the sweet potato as a cultivar had been exhausted, in particular to the island of Rarotonga following the December 1831 hurricane that brought on famine, relieved by Buzacott and Williams sailing their little home built ship back to the home mission base in the Society Islands, in which, on their return in October 1832, the sweet potato was included on the long list of plants and animals brought back to relieve the Rarotongian situation. The view was that Rarotongans knew of the sweet potato, but before this incident saw no point in acquiring it for themselves. Thanks to some recent working through the whaling journals of the 1830s to 1850s with Rhys Richards (in prep.), it is now evident I could easily have expanded on instances of the whaling ships of that period also provisioning themselves by exchanges of sweet potato grown by the Rarotongans for goods carried by these vessels. Such provisioning of whaling ships by the locals continued west of the Southern Cooks into Tonga and beyond, and moreover began early in the 19th century AD.

More important, there are two other instances of failure to cover all the historical sources: the first of an even earlier missionary agent acting to transfer the sweet potato to Atiu which previously lacked it, although the people probably knew of the plant, from Borabora in the Leeward Society Islands, in 1828 (Dalton 1990: 119, footnotes 224 and 227). Note that this transfer is an earlier one by four years to that of the second to Rarotonga in 1832. Kuumara were initially only grown inland, around the original missionary settlement it seems. Unlike Mangaia, the plant was not being grown on the outer ring of makatea (Tanga 1984: 8–9), nor on the fertile lowlands used for dry-land crops that also supported swamp taro, but on the less fertile inland volcanic slopes around the central villages (Kautai *et al.* 1984: map on p.xiii; Tanga 1984: 9–10). Today the observation to be made is that sweet potato in Atiu remains a very minor crop (they are not mentioned by Tanga 1984 for instance), and that vegetable crops and kumara are planted at the beginning of the year and mature in only six months (consequently kumara is usually the first crop to be planted after a hurricane).

In the “Sweet potato transfers in Polynesian prehistory” essay, the absence of the sweet potato by the early European visitors in the 1820s and 1830s AD to Rapa-iti was noted, and its presence in 1864 under the unique term *pata* (borrowing from a term for potato) was documented. However in the whaling journal of G. Dalton edited by Gunson (1990: 118 and footnote 228), he correctly observes “It was common practice for both whalers and missionaries to introduce seeds of exotic fruits and vegetables. Thus the missionary John Davies advised the people of Rapa to sow ‘umara, pineapples ... melon and pawpaw seeds in January 1826 (Davies, 28 January 1826). The implication of this historical observation sounds definitive, indicating that the island of Rapa-iti in the Australs lacked the sweet potato in the form of the early contact plant known to them to have grown in the Society Islands, just as it was judged absent elsewhere in the Austral Island group as documented in the sweet potato essay. This is yet again counter-intuitive to the expectations of some, such as Dickinson. In fact, the local people in the islands of Rarotonga, Atiu and the Australs had all heard about and knew the Tahitian term for *kuumala*, but had declined to affect its physical transfer in prehistory to those islands. Instead the locals left it to the missionaries as agents to carry out that task.

Thus in the M.E./I.M. dialogue that has been constructed here, the Southern Cooks story makes Bill nervous. Can it really be that people had the sweet potato on Mangaia hundreds of years before any came to Rarotonga? This is a mental stretch for him, as he cannot see how the *makatea* could be the answer, as no one tends to garden them even today. The M.E. response is, your views on the Mangaia *makatea* rests on a false perception of its function. Drawing on a the knowledge of a colleague who knows the situation intimately in Mangaia today, this is a view that is fully compatible with the situation reported in the sweet potato essay by a visitor to the *makatea* in 1853 (Green 2005: 49–50; Lamont 1994: 79–80). An email of October 24, 2001 from Richard Walter, with a colour picture attachment showing luxuriant *kuumala* growing on the *makatea* of Mangaia (Figure 1), is persuasive for the M.E. side. To quote Richard: “As you can see, they thrive very well. Contrary to what you might expect, there are extensive pockets of good soil within the Mangaian *matakea* – most within 100 m or so of the inner edge”. Richard Walter’s description of growing sweet potato today on the inner edge of the Mangaian *makatea* has been filled in substantially from much older sources by his Otago University colleague, Michael Reilly, from published and archive sources.

Te Rangi Hiroa refers to it specifically in *Mangaian Society* (1934: 136): “The sweet potato (*Ipomoea batatas*), termed *kuara*... was the chief food of



Figure 1. Kuumala growing on the makatea of Mangaia.

of the conquered in peace times, for it grew in the dry soil of the makatea and the uplands.” And again (1934: 126): “In the makatea are old lanes or channels which have a deposit of finer soil in which kumara could be grown. These better parts are termed *puta ko’atu*, holes in the rocks.” Compare with my notes (more or less the same) from his ‘Mangaia MS Parts’, MS SC Buck Box 4.17 at the Bishop Museum Archives: “Kumara = kuara Mang. was planted on mountain and makatea. There were no special dishes, but it was brought raw to *takurua* (feasts). Planted in *kopu anga kaka’o*, when the cane flowers take form Feb-March. No mention of informants. This was the only reference I noted from the collection of papers.”

I have come to realise that the natural sink holes in the coastal raised coral sections in the Barbers Point sector east of Pearl Harbour, filled over the years with natural and cultural debris, are a perfect, largely self-weeding set of features in which to grow kuumala very successfully. So just as the sink holes of the raised inner edge of makatea in Mangaia are great for growing kumara, so are those of Barbers Point in ‘Oahu, Hawaii.

My interpretative model for Mangaia follows that presented by Kirch (1997) drawing on Te Rangi Hiroa for his information, set out in full in the volume on *Historical Ecology in the Pacific Islands* (Kirch and Hunt 1997). The mytho-praxis of 19th century Mangaian society is the end point of a

long-run of time from circa 1000 AD. “The particular cultural patterns of competition for surplus production from taro lands, the transformation of Rongo – the widespread Polynesian god of agriculture – into a god of war and taro, the transformation of heredity chiefdomship into a more fluid polity favouring achieved power, human sacrifice and cannibalism – all these take on a new significance once they are situated within a dynamic historical context” (Kirch 1997: 164–165). This constituted the dominant mode applying to the major part of the population living in Mangaia.

It neglects, however, an important minor mode of the people growing kuumara crops on the inner edge of the matakea and living in the superb cave shelters aligned along the inner base of the matakea in which the deposits accumulated for 700 to 800 years containing the specific biological remains of kuumara identified by Hather and Kirch (1991). Some might see them as the “losers” in the political polity rivalry stakes and wars of territorial conquest continuously played out in Mangaia; I interpret them instead as a long term and important component within the whole ecological-economic system. They fished the deep waters off the outer coastal terrace, managed forest and gardens on raised makatea ring-like barriers, fed their pigs early in the sequence on surplus sweet potato, cooked the Pacific rat, cooked and ate humans, and cooked and ate native birds to extinction or extirpation, but imported and raised the Pacific Jungle Fowl as additional items of diet (Kirch 1997: 157–159). Early in the sequence the pearl shell for making their fishhooks was imported from elsewhere in the southern Cook Islands: “After 1300 AD this importation of pearl shell ceased, and hooks were manufactured of the locally occurring *Turbo setous* shell, an inferior material” (Kirch 1997: 155). Finally, the more substantial of these caverns contain habitation platforms, cyst graves and burials, tupe disc pitching courts, cooking areas and numerous midden deposits. It was where a minor but important component of the Mangaian population acted in the role of providers of protein – marine fish and shellfish, meat in various forms – for themselves and for the wider population with whom they exchanged goods for taro. In an M.E./I.M. dialogue on the topic of matakea, the evidence for the M.E. stands up best.

On the appraisal “that the Polynesians made it to South America and back again... prior to AD 1000, if the Mangaia date is sound ... it would seem relatively trivial to get out to the Marquesas before settling islands farther west”, the M.E./I.M. dialogue yields answers that contain elements of both yes and no. The prior to AD 1000 argument based on potsherds found in the Marquesas (Dickinson and Shulter 2000: 245) constitutes a definite no, which you, Bill, acknowledge I doubt anyway.

M.E.: Let us be clear about the sherds of pottery found in the Marquesas and subjected over the years to a variety of interpretations by M.E. and others (Kirch 2000: 347, footnote 48). Their quartz-free basaltic temper sands could either represent local Marquesas manufacture, or imports from the islands of Samoa, especially American Samoa, where pottery was still being manufactured as late as Clark (1996: 451) suggests, becoming rare between AD 1300 and AD 1600, and abandoned throughout the Samoan group thereafter. More recent investigations under a volcanic ash deposit on Tutuila Island indicate the sherds of these deposits together with a few cultural features such as a large pit, digging stick holes and possible taro molds (Addison *et al.* in press) are at least 1500 years old. The basaltic temper sherds in fact all derive from the Ha'atuatua site (AD 1100–1200) in the Marquesas (Rolett and Conte 1995), or even later sites on Hiva Oa and Ua Huka. And the few Ha'atuatua potsherds from the Rewa Delta of Viti Levu, Fiji could easily have made their way to the Marquesas via exchange transfers through Samoa and thence to Ha'atuatua.

It is the very recently reported, directly dated, sweet potato fragments from Kohala on the island of Hawai'i that demonstrate the sweet potato is likely to have been introduced to the Hawaiian group as early as the late 13th century, and certainly by the 15th century AD. This early end of the estimate is within a century of the later end of the AD 1100–1200 AD estimate by Green (2005: 52) for the secondary introduction of the sweet potato to the Hawaiian Islands group (the archipelago having been colonised by AD 800 (Athens *et al.* 2002: footnote 1; Carson 2005)). While archaeologists are still unable to say with certainty that sweet potato did not arrive with the original colonists of Hawai'i, all available evidence suggests it was a later introduction. The 13th century is within the estimated duration of periodic long-distance voyaging between the islands of East Polynesia (Cachola-Abad 1993).

I.M.: Thus you have scored a major concession in support of the Marquesas as the most likely point of entry for the transfer of sweet potato from South America to the Eastern Polynesia core ellipse region as well as a fairly robust indicative date for this event. Moreover, there is yet another to be added, as Eric Pearthree (2003) informed me in an e-mail that he has never identified in the prehistoric Rapa Nui charcoal samples he has minutely examined, any of the easily identifiable rinds of the *Lagenaria siceraria* or bottle gourd so commonly found in sites of the central ellipse region (Leach 1984: 23, 25, 31, 42 and 61).

In sum, in M.E.'s citation of fue as a current reflex found today in Rapa Nui cognate with the meaning, a *Lagenaria* gourd, as it also means in Hawaii, Maori, Marquesas, Mangareva, Rarotonga, Tahiti and the Tuamotu

Atoll Island chain, has proven incorrect. Instead in the Rapanui language it has to be interpreted as a recent and therefore post-European borrowing of the word from the languages of the central ellipse region. Fue with the meaning *Lagenaria* gourd is only a lexical item of Proto Central Polynesian status, just as kuumala is. Both seemingly were transferred together as Burtenshaw (1999) anticipated. Further, molecular genetic backing for the probable transfer of the *Lagenaria* bottle gourd from South America to the central ellipse core region of Eastern Polynesia is currently awaiting publication (Clarke *et al.* in press).

Finally, I.M., there remains still another point favourable to your views. Drawing on M. Allen (2004: 181) and also Matisoo-Smith *et al.* (1998), both the rat evidence and that from voyaging indicate the settlement of Hawaii took place from the Marquesas Island group. The biological evidence also supports that origin for the peopling of Hawaii (Pietrusewsky 1997) as do specific details of Marquesan kin terms that are features of Proto Marquesic, where the simplest explanation for their presence in Hawaiian is that this language is a Proto Marquesic daughter (Marck 2000). Subsequently, this known and favourable sailing passage also provided the transport route for the transfer of the sweet potato to the Hawaiian Islands group at a currently estimated c. AD 1200–1300.

I.M. on all these grounds appears very likely to be correct in thinking the Marquesas Islands group was the entry point from which the sweet potato reached Hawaii. The Marquesas Islands group has also proven to be the entry point from which the sweet potato in the same period reached Mangareva, via the eastern Tuamotus as a way finding stop, c. AD 1100–1300 (Weisler and Green 2001: 415–417), supported by much hard evidence in the remains of the material culture found in Mangareva at the that time.

I.M. in a sceptical mode, though somewhat uncertain of the relevant details, finds that modifications appear to be warranted in the existing M.E. core ellipse model and the transfer pathways for sweet potato to New Zealand and Hawaii, that following Yen, Green (2005: Figures 2 and 3) adopted. The first modification is to delete the arrow from Mangaia to New Zealand. Rarotongan emigrants who came to New Zealand were related to Ngati Porou of the East Coast (Green 1966, Harlow 1994: 108, 113–114). In Marquesan, aniwa is the normal name for the 18th night but is otherwise unknown in Polynesian lists, and in this respect Emory calls attention to the historical significance of its occurrence in New Zealand. It was supplied by a very old ariki at Kaitoke in 1882 (Emory 1946: 186).

In overview, it would seem immigrants from Rarotonga, who in antiquity had not acquired the sweet potato, resided on the East Cape, but were

not responsible for the transfer of the sweet potato to New Zealand. Nor did they have an inclination to return to the Southern Cooks to access it from Mangaia, where a lack of an anchorage made transfers off that island extremely difficult. Neither did they find the prospect of accessing it from the missionary brothers on Atiu a suitable option. Rather, the missionaries on Rarotonga, Buzacott and Williams, went directly back to the Society Islands where a wide range of their wants, including sweet potato, would be fully catered for. Because M.E. focused on the AD 1812 missionary account of how sweet potato were then grown in the Society Islands (Green 2005: 49), this tended to overlook a formal instance well inland of Poverty Bay, Tahiti, of inspecting Tahitian cultivations and finding them “to surpass any idea we had formed of them. The ground was completely cleared of all weeds – the mold broke with as much care as that of our best gardens. The sweet potatoes are set in distinct little mole hills which are ranged in straight lines, in others in quincunx. In one Plott, I observed these hillocks, at their base, surrounded with dried grass” (Beaglehole 1968: 583).

For those who brought the sweet potato to New Zealand, and in particular to its Bay of Plenty tribes of Arawa, Mataatua and other related *iwi* of that part of New Zealand, according to their oral traditions Tahiti would have been recognised as the certain source for securing this plant. Hence the claim by Dunis (2005: 91, Figure 2) that Te Kura Whakaata hits upon Hoaki and Tua Kata. Hoaki sails back to Hawaiki and introduces the *kumara* into New Zealand. Tua Kata is scarified in the storehouse. Rawiri Taonui (2005, cited by permission), in his masterly review of Maori oral tradition, comes to the same conclusion in his Ph.D. draft being prepared for publication.

A very bold arrow should extend on the Green (2005) Figure 3 of the core ellipse region from the Society Islands to New Zealand’s Bay of Plenty, instead of from Mangaia, indicating it is the most favoured source for the presence of the sweet potato’s transfer to New Zealand.

This alteration, however, I.M. finds, in a usual sceptical stance on these matters, does not exhaust the options. Atholl Anderson has therefore advanced yet another viable option: “Initial discovery of Hawaii and New Zealand may have depended on the prior attainment of settlement sufficiently far east to create broad-reaching angles of attack or greater in relation to prevailing wind systems, with the former settled from the Marquesas” (Anderson 2000: 34) – however Atholl, it turns out will, have to forego his added statement that in the summer easterlies from the Australs one would not in fact be able to obtain *kumara* until the time of European contact.

His observations may have far more substantive evidence behind it than that of which he may be aware. At the onset there is the unique lexical

evidence of Marquesan–New Zealand aniwa noted above, and there is the evidence of Te Rangi Hiroa reported by M.E. that the oldest white fleshed thin multiple tuber sweet potato root variety of New Zealand, te toroa mahoe, is very like the indigenous Mangarevan elongated tuberous root with white skin and white flesh (Green 2005: 50). It is also pertinent that te toroa mahoe is one of Pani’s children (Dunis 2005: 91 and Figure 2, line 11), and therefore the oldest among the white fleshed or *pallida* variety of sweet potato described by Solander (n.d.: 432) during Cook’s first voyage in 1769 as present in New Zealand.

Garry Law (1994) explored the likelihood of multiple settlement in Eastern Polynesia, and he has returned to its exploration more recently as he set out in his 2006 presentation, “Voyaging and settlement in the south west”, to the New Zealand Archaeological Association conference in Waihi Beach, New Zealand. This theme almost certainly requires further exploration.

In further empirical support of connections to New Zealand from this far to the east in Eastern Polynesia, let M.E. bring to the attention of New Zealand archaeological researchers the following items drawn from Weisler and Green (2005: 418, 423, 426 and 427). These include one Type 5 kind of the side-hafted adze in Mangareva, probably originating from Eiao quarry in the Marquesas, and a functionally related but markedly different type from New Zealand (Turner 2005). Janet Davidson has also pointed out to M.E. that Pitcairn (based on the Kenneth Emory paper of 1928 on stone implements) covers the same range of adze types as are found in New Zealand. The octopus lure rig has now been demonstrated through archaeological investigations to be present in the Marquesas and Mangareva, and Leach (1979) has made a case for its early presence in New Zealand settlement period sites. The closest match to Mangarevan pearl shell toggle harpoon head examples are those in pearl shell from the Marquesas. In fact the small sized Tairua (Coromandel) pearl shell lure was first recovered in New Zealand, before Sinoto shortly thereafter found comparable examples of the same small size with a dorsal-ventral hole in the Marquesas (Green 1967). Louise Furey too calls attention to a number of early Eastern Polynesian sites, including the early levels of the Hane site in Ua Huka, where these lures in a variety of materials serve as a chronological marker, Maupiti in the Society Islands and Hanamiai in the southern Marquesas Islands, all of similar age to Early New Zealand sites (Furey 2002: 77). They attest to yet further parallels between New Zealand and the Far Eastern variety of Eastern Polynesia lures. Finally, imitation whale tooth pendants in *Tridacna* shell are attested archaeologically in Mangareva, and although they do not conform to the pendant category found archaeologically in early sites in New Zealand, presuppose some gen-

eral form of relationship within the whale tooth category (Weisler and Green 2005: 426–427). New Zealand archaeologists still have to explore what opens up here as a promising option.

M.E. is looking a bit ‘tashed’ after this exposition. What seemed to you, Bill, a fairly vintage essay by Green, stimulating and fully authoritative, turns out not to be. Your scepticism has proven fully justified. So often it turns out in science, for which the late Stephen J. Gould frequently used American baseball as his example, of how a slow giant curve ball served to challenge a seemingly set position, requires yet further modification of that seemingly set position to be taken under consideration. This is the case here. Other options in fact apply that undermine the M.E. position that was put forward. In *Defending Science – Within Reason*, Susan Haack (2003) finds the puzzle solving strategy it favours always carries the risk that one has made an error and that scientific terms may take on information *or* misinformation, and sometimes (most often in the social sciences but perhaps not only there) evaluative colouration; and so on. The social sciences, of course, insofar as they concern themselves with local and contingent social roles, rules and institutions have a much more historical aspect than the natural sciences. And that the historical contingencies of human societies might be derivable from completely universal laws of nature seems, to put it mildly, much farther fetched than the idea that cosmological events such as the big bang might be. So, though thus far the social sciences seem to lag far behind the natural sciences (Haack 2003: 350), I recommend Haack’s book to *simpatico* colleagues such as Helen Leach, Richard Walter and Peter Sheppard as advocating a reasonable way forward philosophically. And at a recent seminar in the University of Auckland’s Department of Anthropology, Michael Herzfeld, a leading Harvard anthropologist in our field, advocated not simplification but complexity as constituting the way forward in the last half of the 20th century and into the 21st century, in which anthropology uniquely among the social sciences provided the comparative conceptual framework for understanding cultural processes.

A final conundrum relates to the issue of the sailing rafts along the Columbian-Panama coast from Lima north to Acapulco with the Galapagos Island group at its centre (Dunis 2005: Figure 5). Anderson has now explored that group, and found it had not been visited in prehistory as Heyerdahl has claimed it was. This is a setback to Heyerdahl’s claims that having reached the Galapagos in 1100 AD, South Americans set off on sailing rafts for the Marquesas bearing the sweet potato as one of the cultivars they successfully transferred to Eastern Polynesia. Both Green (2000: 71–76) and Bahn and Flenley 2003: 35–36) make a strong case for influence from Polynesia in the

development of ocean-going sailing rafts with triangular sails like those of Mangareva, in Ecuador.

Re-reading Clinton Edwards' (1972) *New World Perspectives on Pre-European Voyaging in the Pacific*, the usual question of sailing rafts along the Peruvian coast south of the Sechura region must be left open, since there is no better evidence in deciding their pre-Spanish use here. One learns of a voyage by Tupac Yupanqui from Manabí where he employed experienced raft builders and engaged the local mariners as pilots. The means of making this voyage are thus associated with Ecuadorians, not Peruvians, and are certainly not identified with any 'Inca' or other Peruvian maritime activities. In all descriptions balsa wood is clearly indicated, also masts, cotton sails, rigging of henequen and a hut as superstructure. They were loaded with a large and valuable cargo of trading goods, some from coastal Ecuador and some from Tumbes. At Spanish contact a well-organised sailing trade extended from northern Manabí, the latter serving as an exchange point for Peruvian goods (Edwards 1972: 854). All evidence indicates a fore-and-aft triangular sail as the aboriginal type, and the first European observers sometimes spotted such rafts 30 to 40 miles from the coast (Edwards 1972: 861). The many comments of competent mariners and modern observations allow the inference that these sailing rafts could sail into the winds well forward of the beam and that their windward capability exceeded that of a square-rigged ship (Edwards 1972: 864). Edwards (1972: 864) claims they would have left the average 16th century Spanish *navio* far astern. In summary, these sailing rafts displayed a very high degree of flexibility as voyaging craft, far greater than any Anderson would hold as part of the usual operation of an ordinary sailing raft in Eastern Polynesia.

The question then becomes, did the people from Tumbes north to Acapulco also make their way back to Lima via the 'on the latitude' sailing route in the period from c. AD 1100–1300 as Dunis (2005: Figure 5) suggests. It seems very likely. At that point Irwin's (1992: 139) Figure 52 of a computer simulation by strategy 4 for ten canoes from Pitcairn to search the ocean north of Easter Island, and one lucky canoe finds Sala y Gomez, its tiny neighbour. Had a computer-instructed turning day of 20 not been imposed by Irwin on the ten canoes, they would have arrived in the 'on the latitude' sailing route and therefore on their way to Lima. It all bears some rather deep thinking about, with no, as yet, obvious answers. Yet it does suggest one way for sailing canoes from Mangareva and Pitcairn to have travelled fairly easily to the northern coasts of South America, made landfalls there, and in AD 1100–1300, stimulated the invention of the sailing rafts with balsa wood platforms that were observed along the Tumbes coast and northward (see Irwin's (in press) essay for his views on these matters).

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