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Fashion or Proof? A Challenge for Pacific Anthropology

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FASHION OR PROOF?

A CHALLENGE FOR PACIFIC ANTHROPOLOGY

A. Keith Thompson

Abstract: This article is a call to Pacific anthropologists to write the story of the origin of mankind in the Pacific a bit larger and perhaps to look scientifically for additional explanations. Is it possible that the early diffusionists may have gotten some things right, albeit for the wrong reasons?

At its heart, the human obsession with metaphysical questions such as “Who am I?” and “Where did I come from?” funds anthropology departments at universities. The hope is that the tools of modern science and technology will provide more satisfying answers to these questions than have come from the study of religion and theology. Kerry Howe’s title to one recent book about anthropology in the Pacific points to humanity’s search for meaning through origins. He named it simply *The Quest for Origins*,¹ but in many respects, contemporary Pacific anthropology does disservice to the scientific quest and the gnawing obsession that motivates it. For example, it focuses to the seeming extinction of all else, on the question “Who came first?” The contemporary anthropologist’s vocational need for academic credibility stifles exploration and opinion that digress from the mainstream. However, increased thinking outside the box has the potential to flesh out the answers we seek.

1 K. R. Howe, *The Quest for Origins* (Penguin Books: Auckland, 2003).

I will further demonstrate this with an analogy from genealogy—not a science, perhaps, but a discipline that proximity vests with much greater certainty when it provides proof. If I were to credit only my convict great-great-grandfather, Charles Talbot, as my ancestor in the Pacific (convicted at the Cambridge Quarter Sessions July 30, 1827; transported to Tasmania on May 2, 1828; and arriving August 25, 1828, on the second convict sailing of the “Woodford”)² because he was first, I would miss the contribution made to my character and gene pool by the Mackintoshes, who came from Auldearn near Inverness in Scotland to Oamaru in 1879; the Norrises, who also came to Auckland in the 1880s from England; the Kerkins, who came from Cornwall to Auckland in 1906; and the Hulses, who lived on the Isle of Man in the 1850s and who intermarried with the Kenworthys and the Thompsons from Northhamptonshire in Manchester before the Thompsons came to Wellington in two installments in 1918 and 1919. In addition, my Thompson name comes down the patriarchal side of my family tree even though they were the last to come Down Under. Indeed, even if pure math tells the whole story, Charles Talbot accounts for only 1/32nd, or slightly more than 3% of my genes, but he was first, though I do not carry his name and apparently don’t look much like him.³

2 Meryl Yost, “Tasmanian Convict Ships List – ‘W’ Ships,” accessed November 29, 2007, http://www.rootsweb.com/~austashs/convicts/conships_w.htm.

3 An extract from one record of his penal servitude in Tasmania gives the following precise physical description of Charles Talbot: “5 foot 5½ inches tall, dark fresh complexion, small oval head, large bushy whiskers, square upward shaped visage, medium forehead flat in front, dark black eyebrows, gray eyes, long straight downward pointing nose, large mouth, long indent in chin at point with a slightly purple scar on left eyebrow, a mark of the king’s evil under each side of the jaw, and large slightly ruptured scar on the back of the right hand” (Mitchell Library, Sydney, Talbot, Charles, Cambridge Quarter Sessions 30 July 1827 14 years M.L.Ref. A10593 p.396 Ship Woodford (2) Arrived 25 August 1828 Con 18/21).

Another slice of information provides context before I set out my thesis. Contemporary anthropology posits that we do not need to look outside the Pacific for an explanation of the physiological differences that characterize her diverse peoples.⁴ These differences can all be explained by internal adaptations. But, as one leading anthropologist friend quipped to me, such logic implies that, “Evolution can occur on a boat ride!”⁵ Such humor, of course, does disservice to the notion of a funnel in genetics—meaning that if only the big, fat people survived a seminal canoe voyage, only big, fat people passed on their genes to later generations. The humor is not completely unjustified, since even that simplification ignores any skinny, small genes that the big, fat survivors carried.

To have a meaningful understanding of who the Polynesians are and where they came from, anthropologically speaking, we need to search out more of the story and open our minds to the nuances that do and must exist in the story of the colonization of the Pacific in pre-European times. Understanding a little about evolution, I find very difficult to accept that my native friends in Tarawa, Majuro, Honiara, Lae, Port Vila, Noumea, Salelologa, Vavau, Rotuma, Niue, Aitutaki, and Moorea all come from precisely the same gene stock originating fewer than 5,000 years ago because they all look so different. Given the short time involved, I believe the discredited wave and diffusion theory must tell part of the story, and I have been pleased to discover recently that leading Pacific anthropologists are open to such a possibility, though they have not written much on the subject.⁶ However, when

4 For example, see Howe, *Origins*, 51–52, 61–62.

5 Ben Finney, private conversation, Honolulu, August 23, 2007.

6 Geoff Irwin, personal discussion, March 22, 2007; Kerry Howe, personal discussion, Massey University at Albany, Auckland, May 3, 2007; Ben Finney, personal discussion, Honolulu, August 23, 2007; and Patrick Kirch, personal discussion, Berkeley, California, February 7, 2008. Against the current, John Sorenson has written extensively on this subject with his magnum opus. John

I write of diffusion theory, I do not mean the idea that the whole world was populated from some Aryan headquarters in Europe. When I write of diffusion theory, I mean the peoples of the Pacific did not have just one gene source; the Pacific was colonized by people from diverse places and gene pools. While one source may appear to predominate when we consider only part of the evidence, that source is still not the only source nor necessarily the most interesting source.

My thesis is that there must have been waves of colonization and significant diffusion. In writing that, I realize I might have chosen less loaded labels than *waves* and *diffusion*, as these words and their baggage may close minds that would otherwise have read further, but I think it both honest and useful to admit I am revisiting some old chestnuts, at least in part. Indeed I assert that whereas no one can yet prove beyond reasonable doubt the exact detail of the waves of immigration that the self-contained evolution theorists posit,⁷ simple, honest armchair deduction alone makes the case for wave theory, undisputable for the truly objective.

I will begin this argument by discussing what constitutes proof—even in anthropology. Though I could discuss proof in great academic detail,⁸ this essay is not the place to do that, and I will try to keep it simple by presenting the different standards of proof that apply in human experience through

Sorenson and Martin L. Raish, *Pre-Columbian Contact with America across the Oceans: An Annotated Bibliography*, 2 vols., 2nd ed. (Provo, Utah: Research Press, 1996). This work documents more than five thousand discrete evidences of pre-Columbian contact between the American continent and the rest of the world, including Polynesia.

7 Theories of Polynesian origins are said to form a continuum—from Terrell's notion of the *Entangled Bank* to Diamond's *Express Train*. Jonathan S. Friedlaender, et al. "The Genetic Structure of Pacific Islanders," *PLoS Genetics* 4/1 (2008): 173, 186.

8 My PhD thesis focuses on one small aspect of the law of evidence. Anthony Keith Thompson, *Religious Confession Privilege at Common Law: A Historical Analysis*, Murdoch University, West Australia, 2007.

analogies from legal practice and discussion of proof standards generally accepted in scholarship. Even more than in the study of history, the discoveries of anthropology can produce only hypotheses. The reason contemporary anthropology has focused on beginnings rather than diffusion has more to do with context, fashion, and contemporary academic credibility than with finding satisfying answers to the underlying gnawing questions identified above (such as “Who am I?” and “Where did I come from?”). I concede, though, that revisionism can also provide a useful foundation for academic research. Finally, I will conclude that wave and diffusion theory are just as deserving of academic respect and future consideration as the arguably simpler self-contained answers to the question of first origins. Indeed, perhaps wave and diffusion theory deserve more contemporary consideration because they have been ignored for the last fifty years.

What Is Proof?

At its simplest level, proof is the creation of a sense of certainty, but we do not often use the word *proof* in that simplistic way. We recognize that because of human fallibility and deceit, there are many things we cannot know for sure, so we devise probabilistic rules that enable us to work out which facts are most likely to be true.⁹ Perhaps the *proof* art is most developed in mathematics and in law. So familiar are the proof vocabularies of mathematics and law that we use them out of their home contexts. For example, we routinely identify the margin for error in public opinion polls in mathematical terms, and we

9 Alex Stein suggests that traditional evidence law rules are founded upon probabilistic theory, which allows judges to apportion risk as they deem fit and should be set aside in favor of more mathematical principles, which can yield more trustworthy results. Alex Stein, *Foundations of Evidence Law* (Oxford: Oxford University Press, 2005).

are satisfied beyond doubt of many things in our everyday lives before we take some new direction.

There are essentially three standards of proof in law, which are most easily understood when they are translated into approximate mathematical statements. For instance, an accused person will not normally be convicted unless the court is, say, 99% satisfied of guilt—*beyond reasonable doubt* is the standard legal phrase. In civil cases, a lower standard of proof has been deemed sufficient. The plaintiff must only satisfy the court that the case has been made out *on the balance of probabilities*, which mathematically would constitute 51% proof. To say that a *prima facie* case has been made simply means that a judge has accepted, after a preliminary review of the facts and law, that the criminal charge outlined *could* sustain a guilty verdict once *all* the evidence has been heard. Literally, the Latin phrase *prima facie* means *at first appearance* or *on the face of it* or in other words that on a limited review of the evidence provided by one side of the debate, it is arguable that there is a case to be decided. If the prosecution cannot satisfy this very preliminary standard, they cannot advance the matter. Thus, it is more difficult to suggest what percentage of proof the court has accepted if it decides that a *prima facie* case has been made out.

The following example demonstrates the tentative nature of a *prima facie* case finding. If only 10% of the material likely to be aired at trial were heard during the preliminary review, there could not have been more than 10% proof—perhaps less if that evidence were not tested by cross-examination. Thus, it is fair to state that finding a *prima facie* case against someone is finding no proof at all—regardless of how that result may be portrayed in the popular press. The weighing of the evidence in such preliminary reviews has not really begun and awaits subsequent detailed briefing. In a historical or anthropological context, a *prima facie* case might be translated to mean that

people educated in the field consider that a new suggestion appears to have validity, but they have yet to be convinced.

What standards of proof apply in other contexts? While we do not always think of proof in non-legal areas in such precise statistical ways, normally we can identify the legal paradigm that has become accepted as applicable. For example, historians can establish some facts with absolute precision as the focus of the headlines in every American newspaper on Monday, December 8, 1941. The causes of World War II, however, are much more difficult to pin down. Much ink has been spilt defining those causes, and in the end, we accept the historian's best guess if all the available evidence has been objectively considered. Of course, if new evidence comes to light, there is room for revision of the previous conclusions. In this sense, all historical conclusions are provisional. Historical scholarship is generally satisfied if a matter is proven on the balance of probabilities—in which event we might claim that we are 51% sure.

What standard of proof applies in anthropology? Some will say that because it is a science and increasingly uses the tools of technology, we can state some findings with much more certainty than 51%, and that is true. Despite the occasional criticism of the reliability of radiocarbon dating technology,¹⁰ it is generally accepted that we can determine exactly the age of a given item or a shard of Lapita pottery. But since the contents of the said Lapita pot were likely organic and have vanished, different issues of proof attach to the deductions we make about the person(s) who made the pot, where they lived, how

10 Atholl Anderson has suggested that many of the earliest dates yielded by radiocarbon dating in the Pacific must be culled in the interests of intellectual rigor, and his findings have been confirmed by the reworking of samples measured at the University of Waikato Radiocarbon Dating Laboratory between 1975 and 1995. Howe, *Origins*, 176. Others have pointed out that Anderson's methodology is itself flawed since his convenient exclusion of the earliest samples yields the later dates of human habitation for which he has always argued.

the pot got to where it was abandoned, whether it was ever traded, how the pot was broken, whether such pots were ever repaired, why the pot was abandoned, and about the artistic inspiration of its crafter and the genetic makeup of all the actors who ever handled it. Obviously it would take a very long bow to answer any of these questions and many others with a degree of certainty approaching even 30%. Perhaps the best we can do in such an anthropological case, even bolstered with archaeological evidence, is make a *prima facie* case.

However, regardless of whether or not we believe we have made a *prima facie* case or even feel optimistic that we may have established our hypothesis on the balance of probabilities, it is objectively self-evident that we do not prove anything of enduring value in anthropology beyond reasonable doubt. Though we may be able one day to prove the date of a Lapita pot's manufacture as well as its demise, those stark facts alone do not provide any enduring satisfaction to an anthropologist.¹¹ They are altogether too clinical. What we really want to know is what we can reasonably deduce from the clinical facts, and it is the word *reasonably* that identifies the standard of proof accepted in anthropological scholarship. *Reasonable* here is not as in *beyond reasonable doubt*, it is what the reasonably objective person would deduce if these facts were put before him or her. English judges used to capture this sense of what was reasonable by identifying the reasonable man as a fictional man on "the Clapham omnibus."¹² Their idea was to identify an

11 Howe observes that though we "rely on the 'hard facts' of modern science such as radiocarbon dating, genetics, linguistics... [and] archaeology... [y]et how we interpret Pacific prehistory, what aspects of it we emphasise, still reveals a range of cultural values and pre-occupations." See Howe, *Origins*, 24.

12 The quoted phrase was first coined by L. J. Greer in *Hall v. Brooklands Auto-Racing Club* (1933) 1 KB 205. However, perhaps the earliest formulation of the notion of the reasonable man came from B. Alderson in *Blythe v. Birmingham Waterworks Co.* (1856) 11 Exch., 781, 784. He said: "Negligence is the omission to do something which a reasonable man, guided upon those considerations which ordinarily regulate the conduct of human affairs, would do, or doing something

objective, unbiased *everyman*. In coming to judicial conclusions in civil cases, they would try to work out what an everyman would decide and make it the judges' decision. The literature debating whether there can be such an everyman is legend, but the esoteric concept endures in many forms in our 21st century society. I suggest it is what the *everyperson* would decide that dictates a good conclusion from clinical facts in anthropology. If objective, unbiased *everypersons* had all the relevant, currently known facts before them, how would they consider the Pacific was colonized? I want to suspend consideration of that question until I have identified just how hard it is to find such an unbiased every person.

Geoff Irwin demonstrates that the issue of proof is indeed a live and relevant issue in Pacific anthropology when he writes: "While science must keep an open mind about [the possibility the first settlers of New Zealand arrived before 1350 AD], there is a burden of proof on those who propose [such ideas]."¹³ Even though the idea spawned by oral genealogy and taught to generations of New Zealand primary school children has held that the first arrival in New Zealand was at Kupe in 950 AD, Irwin believes that contemporary anthropologists have objectively proven on the balance of probabilities that the first settlement came much later, despite the Kupe tradition. He further states that to reestablish that old idea, the traditionalists must put up some hard evidence.¹⁴

which a prudent and reasonable man would not do." Lord MacMillan elaborated that standard in *Glasgow Corporation v. Muir* [1943] A. C. 448, 457 when he said: "The standard of foresight of the reasonable man... eliminates the personal equations and is independent of the idiosyncracies of the particular person whose conduct is in question."

13 Geoff Irwin, "Voyaging and Settlement," in *Vaka Moana, Voyages of the Ancestors: The Discovery and Settlement of the Pacific*, ed. K. R. Howe (Albany, Auckland: David Bateman, 2006), 89.

14 Irwin, "Voyaging and Settlement," 89. Note also that Michael King effectively makes the same argument. Michael King, *History of New Zealand* (Albany, Auckland: Penguin, 2003), 38–47.

The relevance of the standard of proof can be made in a different way from comments that Kerry Howe has made about Thor Heyerdahl's Pacific colonization theories. He states:

Heyerdahl offered the following broad clusters of evidence for his theory. The *Kon-Tiki* expedition itself proved how it was done. The winds and currents drove sailing vessels relentlessly westwards. There were his claims of similarities between eastern Polynesian words and those of South America. He also claimed that "pure" eastern Polynesian blood groups were similar to those of North and South America. He amassed a whole range of archaeological evidence supposedly showing cultural links with both North and South America—the most notable being the Easter Island stonework. He also argued that certain eastern Polynesian plants, including the sweet potato, originated in South America.

While the public adored Heyerdahl, the scholarly community largely ignored him. Few academics have bothered to spend their time trying to refute his mass of claims and his voluminous evidence. For those aware of the issues, he was so wrong as to be not worth taking too seriously.¹⁵

The outstanding Heyerdahl evidence is summarized below.¹⁶ It is not fair to say that Heyerdahl's mass of evidence was so wrong as to be "not worth taking too seriously,"¹⁷ nor that a reasonable, scholarly posture suggests that there would be no value in reviewing Heyerdahl's evidence more seriously—

15 Howe, *Origins*, 127–128.

16 Howe, *Origins*, 17–18.

17 Note, however, that this is not Kerry Howe's personal position. He simply reports this has been the verdict of the majority of the academic community.

especially since other scholars are now demonstrating that unquestionable links exist between Polynesia and South America.¹⁸ If the rules of evidence used in legal practice were applied objectively, it is also difficult to claim academic anthropology has the moral high ground or that Heyerdahl has, as Geoff Irwin might say, the onus of proof. That is more especially true when Howe clearly admits there has been no effort to address the bulk of the material that Heyerdahl produced as evidence.

Context

Kerry Howe, however, has brilliantly explained how the anthropological theories of the past reflect both the preoccupations and even the religious beliefs of those who proposed them. For example, he points out that the question “Where did the Polynesians come from?” betrays an ancient conceit in the questioner who finds it hard to believe that such a feat of discovery might have been achieved by someone other than the questioner.¹⁹ There are other conceits in the question and the discussion that traditionally surrounded it, which Kerry has explained better than I can. More obvious is the predetermination evident in the anthropological answers offered by 19th century Christian missionaries whose Bible told

18 For example, see Geoff Irwin, “Voyaging and Settlement,” 83; P.J. Matthews, “Plant Trails in Oceania” in *Vaka Moana*, 96; and Ben Finney, “Ocean Sailing Canoes” in *Vaka Moana*, 135. Again, and as mentioned above at footnote 6, John Sorenson at BYU has devoted almost his entire life to the diffusion thesis. While he has not specialized in diffusion to and from Polynesia, most recently with Carl L. Johannessen, a geographer from the University of Oregon, he has demonstrated that a hundred species of plants, many of them cultivars, were present in both the Old and New Worlds before Columbus’s day, and a considerable number of these were shared between the Americas and Polynesia. John L. Sorenson and Carl L. Johannessen, *World Trade and Biological Exchanges before 1492* (New York: iUniverse, 2009). A new, revised edition of this work was published in 2013. The new edition is available from Amazon.com.

19 Howe, *Origins*, 8.

them there were lost tribes of Israel somewhere.²⁰ But as Kerry says, it is not quite so easy to see where our own blind spots are.²¹ We are simply too immersed in them to see. Because of my own immersion, I am sure I do not see them all. Nevertheless, I can identify some perhaps because, in a measure, I am an anthropological outsider.

Universities are notoriously political places. To make a career in academic anthropology, one must not only be brilliant and passionate about anthropology, but one must also pay the piper.²² Whereas it is self-evident that universities are houses of new learning, it is proverbial that one cannot afford to be completely original, either. The standard modern academic entry token, the PhD dissertation, is a case in point. While it must be original enough to pass examination, it must also proceed from established reference points and be full of precedential citations of previous authority to have academic credibility. Precedent and originality make strange counterpoint. Surely true originality eschews precedent; the only legitimate reason for a supervisor or examiner to insist on precedent in a thesis is to demonstrate that the candidate adequately understands

20 Howe, *Origins*, 36–41.

21 Howe, *Origins*, 24, 184.

22 Note the transparency in Peter Capelotti's book *Sea Drift, Rafting Adventures in the Wake of Kon-Tiki*. He says: "Heyerdahl was perhaps inevitably disappointed that his experiment in constructing a primitive raft and transiting across an ocean on it did not inspire more scholarly interest. But he should not have been. The unprecedented attention and acclaim earned by the *Kon-Tiki* expedition were almost guaranteed to make the experiment suspect to scholars. Until the very recent advent of public and cable television documentaries, the general public hardly ever witnessed the bitter infighting of academics who either conducted controversial experiments or, likely as not, sat back and criticized those who did. For the critics especially, Heyerdahl was an interloper: a zoologist bearing an anthropological hypothesis into the highly stratified and segregated world of the academy. He seemed to cross too many conflicting lines of evidence from widely separated prehistoric events taken place across millennia. (Peter Capelotti, *Sea Drift, Rafting Adventures in the Wake of Kon-Tiki* (London: Rutgers University Press, 2001, xvii).

the relevant field of knowledge before embarking on a novelty, but that is not the way it works in practice. Doctoral candidate examiners whose own work is discredited by such originality are legendary for issuing fail or rewrite reports, hence the number of doctorates granted in Western universities does not necessarily represent a burgeoning in the body of human knowledge, but they should.

The study of anthropology really began only in the 19th century, so it was natural that it began its life as a science.²³ But in the early years, it was actually an *armchair* science.²⁴ When professors finally began to use the scientific method and look for hard evidence, they were retrospectively embarrassed by the naiveté of their predecessors.²⁵ Rather than sift past work for

23 Until the 18th century (perhaps beginning with Galileo), the world's thinkers were called philosophers—even those who really developed the tools of empiricism. G.C. Gillispie, *The Edge of Objectivity* (Princeton: Princeton University Press, 1960), 7. Gillispie goes on to say: "In its early days, science was distinct from technology, springing rather from thought and philosophy than from craftsmanship. Nowadays, however, and indeed for the last century and more, science has merged more intimately with technology, so arming it with power. ... The answer [to why Europe created science] lies in Greece. Ultimately science derives from the legacy of Greek philosophy. ... Of all the triumphs of the speculative genius of Greece, the most unexpected, the most truly novel, was precisely its rational concept of the cosmos as an orderly whole working by laws discoverable in thought. The Greek transition from myth to knowledge was the origin of science as of philosophy. Indeed, knowledge of nature formed part of philosophy until they parted company in the scientific revolution of the seventeenth century" (Gillispie, *Objectivity*, 8-9).

24 For example, though Edward Tylor, who Kerry Howe calls "a founder of anthropology," (Howe, *Origins*, 43) undertook a field trip to Mexico, he and John George Frazier "derived most of the material for their comparative studies through extensive readings of Classical materials (literature and history of Greece and Rome), the work of the early European folklorists, and reports from missionaries, travelers, and contemporaneous ethnologists." *Wikipedia*, "Anthropology," last modified January 13, 2014, accessed December 15, 2007, <http://en.wikipedia.org/wiki/Anthropology>.

25 "Neither Tylor nor Frazer, however, were particularly interested in fieldwork nor were they interested in examining how the cultural elements and institutions fit together. Towards the turn of the century, a number of anthropologists became dissatisfied with this categorization of cultural

its enduring contribution to scientific advancement, the newly enlightened anthropologists simply started again and threw away the old paradigms completely. Therefore, when Kerry Howe concludes his book about the academic quest for the origins of Pacific peoples, he wonders if “any babies have been thrown out in the diffusionist bathwater?”²⁶ He also suggests in the same paragraph that future anthropologists may well identify contemporary obsession with aboriginal nationalism in the Pacific as a blinker that has obscured objective treatment of the available evidence in the early part of the 21st century. It is certainly academically difficult at present to say anything that suggests the currently dominant strain of aboriginals were not the first here or there because that would dilute their moral claim for various types of compensable wrong.

Now all this is not to say one cannot make an academic career as an anthropological revisionist.²⁷ To prove they are objective, some universities make a point of appointing token professors who swim against mainstream currents. But like personnel managers in modern corporations, their career

elements; historical reconstructions also came to seem increasingly speculative. Under the influence of several younger scholars, a new approach came to predominate among British anthropologists, an approach concerned with analyzing how societies held together in the present (synchronic analysis rather than diachronic or historical analysis) and emphasizing long-term (one to several years) immersion fieldwork. Cambridge University financed a multidisciplinary expedition to the Torres Strait Islands in 1898, organized by Alfred Court Haddon and including a physician-anthropologist, W. H. R. Rivers, as well as a linguist, a botanist, and other specialists. The findings of the expedition set new standards for ethnographic description. A decade and a half later, Polish-born anthropology student Bronisław Malinowski (1884–1942) advocated an approach to fieldwork that became standard in the field: getting ‘the native’s point of view’ through participant observation. Theoretically, he advocated a functionalist interpretation, which examined how social institutions functioned to meet individual needs” (Wikipedia, “Anthropology”).

²⁶ Howe, *Origins*, 184.

²⁷ Atholl Anderson at the Australian National University is the anthropologist who currently seems to demonstrate this point best. Howe, *Origins*, 176 and Finney, “Ocean Sailing Canoes,” 132.

paths are normally tangential to the real world at the university concerned. Revisionism may sell a few books to the ladies from Vaucluse, Toorak, and Remuera (and if those ladies are really wealthy, it may endow the occasional chair), but as tokenism, it does not advance the world's general scientific understanding and does not shift the status quo of contemporary academic opinion. That happens only when a few of the most respected mainstreamers take a leap of faith and credit a previously disreputable theory.

Now the sequitur—were the armchair anthropologists of the past completely wrong, or does something remain in their theorizing about diffusion that begs for scientific treatment? Though I have traveled the Pacific as extensively as anyone in the last twenty years, I am not an anthropological fieldworker experienced with brush and trowel. To that extent I am doubtless as naive as my armchair predecessors. But still, it is remarkable what one can learn if one takes the time to simply ponder the old logic alongside the most recently published evidence.

Waves and Diffusion

Heyerdahl was the most famous diffusion theorist, but he was not the first. Until the 1960s, contemporary anthropology held that the genesis of the Polynesians discovered by Cook and other European explorers lay outside the Pacific, probably somewhere to the west. J.R. Forster, who was Joseph Banks's replacement on Cook's second voyage, was the first to posit that Polynesia had inhabitants before those they found in the late 18th century.²⁸ However, he did not believe they adapted or evolved in situ. Rather, on the basis of primitive comparative physiological and linguistic analyses, he believed they originated in some part of Asia rather than in either America or Australia.

28 Howe, *Origins*, 29–31.

Christian missionaries thereafter posited that the Polynesians had Semitic antecedents,²⁹ and this idea was superseded by post-Darwin scholarship that groped for less religious but conceptually similar Aryan or Caucasian origins.³⁰ Margaret Mead's *Coming of Age in Samoa*, though flawed, symbolized the next shift in the anthropological academic mainstream.³¹ Her belief that differences in these peoples could be explained environmentally brought evolutionary theory fully into the Pacific anthropological equation and dispensed with the need for any waves of inbound migration. Kerry Howe summarizes the "broad orthodoxy"³² pervasive until the mid-twentieth century as holding that the initial settlement of the western fringes of Oceania was achieved by dark-skinned, Southeast Asian people, but they were later recolonized by lighter skinned people from the same area who proceeded much farther into the area now commonly known as Polynesia. These ideas "reflect a range of Western cultural assumptions, fears and aspirations."³³

Current thinking holds there was no "Polynesian migration into the Pacific because there were no Polynesians when humans began moving into Oceania. There was, instead, an initial, generalized Austronesian culture that emerged from the Southeast Asian region... [which] experienced a wide range of adaptations... over thousands of years."³⁴ The idea that the remotest parts of Polynesia could have been populated only by chance drift voyages, most controversially promoted by

29 Howe, *Origins*, 36–41.

30 Howe, *Origins*, 41–51.

31 Margaret Mead, *Coming of Age in Samoa. The Making and Unmaking of an Anthropological Myth* (Canberra, Australia: National University Press, 1983) as cited in Howe, *Origins*, 51.

32 Howe, *Origins*, 59.

33 Howe, *Origins*, 61.

34 Howe, *Origins*, 61.

Andrew Sharp³⁵ against Sir Peter Buck's more popular idea that the Polynesians were the "Vikings of the Sunrise,"³⁶ has been discredited³⁷ in particular by Geoff Irwin and Ben Finney, who demonstrated more convincingly (and popularly) that the colonization was more likely the result of a "deliberate strategy of exploration and settlement."³⁸

Philip Houghton's quasi-medical contribution to the environmental argument³⁹ does not serve that cause particularly well. His suggestion that Polynesians evolved large muscular frames to survive cold voyages of exploration is difficult to accept, implying as it does that the evolution involved occurred within a period of a thousand years at most.⁴⁰ Indeed, so implausible does that argument seem that

35 Andrew Sharp, *Ancient Voyagers in the Pacific* (Auckland: Longman Paul, 1963).

36 Peter H. Buck, *Vikings of the Sunrise* (Christchurch: Whitcombe and Tombs, 1975).

37 There is more about Sharp's work that should have been discredited. For example, he wrote: "On the issue of whether the Polynesians were distributed from Western Polynesia or Eastern Polynesia, in the first place, the records of accidental voyages can throw no light, since some occurred in both directions. The answer is established beyond reasonable doubt by the linguistic research of Dr SH Elbert, who has shown that Western Polynesia was the ancestral speech area of the Eastern Polynesian tongues, and that the Hawaiian and Maori are derived from one or other of the latter" (Sharp, *Vikings*, 72–73). But when one reads the article referred to (Samuel H. Elbert, "Internal Relationships of Polynesian Languages and Dialects," *Southwestern Journal of Anthropology* 9: 147–173), one finds that Elbert assumed the fashionable belief in Polynesian origins in "the Asiatic homeland" was correct (158, 163) and sought to explain all his research against that assumptive background without considering other possibilities. However, Elbert did conclude his article with the rather stark factual observation: Percentages of vocabulary agreement are so low that at least three Polynesian languages must be said to exist: West Polynesian, Kapingamarangi, and East Polynesian (Elbert, "Dialects," 170). Sharp ignored this honesty completely, and the omission suggests that Sharp, as many others, had an agenda.

38 Elbert, "Dialects," 62.

39 Phillip Houghton, *People of the Great Ocean: Aspects of Human Biology of the Early Pacific* (Cambridge: Cambridge University Press, 1996).

40 While Houghton does not say a thousand years, all the evidence he relies on for Polynesian inhabitation of Remote Oceania anticipates dates little earlier

one might indelicately suggest that the argument would be more convincing if Houghton sided with Heyerdahl and found the ancestors of Polynesian sailors in the high, cold Andes whence Heyerdahl might have been happy to have some of them come.⁴¹ In fact, Houghton even seems to discredit much of the linguistic evidence generally taken to support the environmental thesis when he states:

An immediate example of the fragility of the link between language and people, is given by Oliver, recording in a Bougainville community the almost complete replacement within one generation of one language by another. (Here it happens to record the demise of an Austronesian language.)⁴²

Houghton thus seems guilty of the danger Kerry Howe exposes with his implicit charge that good anthropologists must be careful not to let the result they want color their

than 300 AD, and since the Maori were probably separate and established with large, muscular frames by 1000 years later, this intrepid voyaging, which most distinguishes Polynesians from other Pacific Islanders in Houghton's hypothesis, and the cold of those voyages, which is his evolution mechanism, must have happened within 1000 years if he is correct. Suggs suggested in 1960 that all the differences in Polynesian features could be explained by microevolution or short-term evolution (Robert C. Suggs, *The Island Civilizations of Polynesia* (New York: Mentor Books, 1960), 35–37, 88, 233), but he does not explain how this microevolution could be so time-compressed, save to say that all the right conditions existed for it to happen in Polynesia, namely: "isolation by natural geographical or social boundaries and environmental differences" (Suggs, *Island*, 35).

41 Heyerdahl theorizes that the Polynesian differences from the Melanesians and the Micronesians elsewhere in the Pacific can be accounted for by two different waves of migration. The first of tall, fair-skinned Aryan people, who came from the mountains and coastal areas of what is now Latin America, somewhere between say 100 BC and 300 AD, and another wave of North American Indian people, who came from the Pacific Northwest around 800 AD. See Thor Heyerdahl, *American Indians in the Pacific, The Theory behind the Kon Tiki Expedition* (London: George Allen & Unwin, 1952).

42 Houghton, *People of the Great Ocean*, 135–136.

objective interpretation of the evidence they find and analyze. From Houghton it seems fair to conclude that the physiological changes he labors hard to explain would be more easily dealt with under some kind of diffusion hypothesis.

Houghton is not the only recent anthropologist to say things that ought to breathe life into a reconsideration of a diffusion element in Pacific colonization. Consider the following statements from several other writers featured in Kerry Howe's magnificent text *Vaka Moana, Voyages of the Ancestors: The Discovery and Settlement of the Pacific*.⁴³ Geoffrey Irwin states:

It is now generally accepted that no one group of people travelled all the way from Asia to their new Pacific Island home. As they moved they changed, interacted with others, and eventually produced the diverse peoples, biological types, cultures and the many hundreds of languages known throughout the wider Pacific region today. However, there is less agreement about whether Pacific boat technology and navigational methods developed within the region or were imported from outside.⁴⁴

In the first sentence, Irwin may be said to have restated the internal evolutionary theory albeit using diffusion language. Nevertheless, his concession that the maritime technology alone might have been *imported* seems odd in that context. Though his comments are guarded, P. J. Matthews, writing of "Plant Trails in Oceania," says:

Long before the arrival of Europeans, the sweet potato was carried from the Pacific coast of South America to eastern Polynesia. This transfer is believed to have depended on the voyaging abilities of early Polynesians.

⁴³ Howe, *Discovery and Settlement*.

⁴⁴ Irwin, "Voyaging and Settlement," 56.

Regardless of how the plant was carried, the fact that the Polynesian name *kumara* is based on an American name for the plant is proof that Polynesian and American people had face-to-face contact. The full extent of contact and travel between the two regions remains unknown.⁴⁵

Similarly, David Penny and Anna Meyer effectively admit that diffusion theory will need to be reexamined in the future when they write:

Most of the evidence in this area comes from the maternally inherited mitochondrial DNA, which shows a close match between Polynesians and the indigenous people of Taiwan, the Formosans. ... Interestingly, Polynesian Y chromosomes DNA does not show such a definite answer. The reasons for this are not entirely clear yet, but one idea is that there could have been later waves of migration, with differences in the way in which males and females moved about.⁴⁶

It may be that Kerry Howe's observations about context are again in evidence here. The only reason Jose Miguel Ramirez-Aliaga has found pre-Hispanic chicken bones in Southern Chile⁴⁷ is that he lives in Latin America and was looking for some such evidence of American contact with the Pacific. Penny and Meyer and everyone else respond to the contextual stimuli which hold their interest.

45 Matthews, "Plant Trails in Oceania," 96.

46 David Penny and Anna Meyer, "DNA and the Settlement of Polynesia" in *Vaka Moana*, 98.

47 E-mail correspondence from Jose Miguel Ramirez-Aliaga to Geoff Irwin, August 8, 2006. See also Proceedings of the National Academy of Sciences of the United States of America, June 19, 2007, 104/25, accessed December 1, 2007, <https://www.pnas.org/cgi/content/full/104/25/10335>.

Regardless of Heyerdahl's motivation, he is generally dismissed academically as little more than a curiosity, whereas his famous *Kon-Tiki* expedition in 1947 may be seen to have laid the conceptual foundation for later proof that Pacific voyaging and settlements were not accidental.⁴⁸ He has no real scientific credibility because he is discounted as a European racist resurrecting the old missionary ideas of Aryan origins for Polynesian people. In particular, Heyerdahl's insistence that there was an early colonization by civilized, fair-skinned people with advanced technology who were killed off later by brown-skinned invaders is dismissed with demonstrations of anachronism and selective science.⁴⁹ And there is little doubt that in his later work, Heyerdahl was writing for an audience, but so were some of those who strove mightily to discredit him.

Sitting in my armchair, I admit that I, too, have been entranced by Heyerdahl's adventures, but after recently rereading *American Indians in the Pacific, The Theory behind the Kon-Tiki Expedition*,⁵⁰ I doubt that academia has been completely fair to Heyerdahl. Certainly his ego did not require allies, but when one catalogs his evidence for some American connections with the Pacific and Polynesia in particular, a lot remains that has not been answered. Suggs pointed out in the early 1960s that Heyerdahl's treatment of the Easter Island colonization was highly anachronistic.⁵¹ In a very colorful paragraph, he says:

48 Howe, *Origins*, 112. While Kerry Howe does not credit Heyerdahl in exactly this way, the debate which began when Andrew Sharp sought to rebut Peter Buck's view of the Polynesians as "Vikings of the Sunrise" became much more focused after Heyerdahl seized the public relations high ground following his *Kon-Tiki* expedition in 1947. Howe, *Origins*, 60, 122.

49 Suggs, *Island Civilizations*, 212–224. Suggs also severely criticizes Heyerdahl's methodology when he collected the blood samples which underlie the conclusions he made about the origin of Polynesian blood types (215–216).

50 Heyerdahl, *Kon-Tiki Expedition*.

51 Suggs, *Island Civilizations*, 224.

Heyerdahl's Peruvians must have availed themselves of that classical device of science fiction, the time machine, for they showed up off Easter Island in A.D. 380, led by a post-A.D. 750 Incan god-hero, with an A.D. 750 Tiahuanco material culture featuring A.D. 1500 Incan walls, and not one thing characteristic of the Tiahuanaco period in Peru and Bolivia. This is equivalent to saying that America was discovered in the last days of the Roman Empire by King Henry the Eighth, who brought the Ford Falcon to the benighted aborigines.

Though some of Heyerdahl's evidence has been discredited, and very little of it has the durability of the subsequent Lapita discoveries, there is still much in his catalog that raises legitimate questions about balanced assessment by later critics. Consider for example:

- Why Polynesians look more like Madagascans and Northwest American Indians than they do Micronesians and Melanesians. Appearance similarities include stature, nose structure, skin color, beards, and hair color and type.⁵²
- Why Polynesians don't use shell money, yet both Micronesians and Melanesians do.⁵³
- Why there is no betel nut in Polynesia.⁵⁴
- The spiral design of the Maori and their challenging custom of the extended tongue, which has connections with the Northwest American native but not other peoples of the Pacific.⁵⁵

52 Heyerdahl, *Kon-Tiki Expedition*, 21–28, 83–91.

53 Heyerdahl, *Kon-Tiki Expedition*, 47.

54 Heyerdahl, *Kon-Tiki Expedition*, 49.

55 Heyerdahl, *Kon-Tiki Expedition*, 116, 126.

- Polynesians do not use kites in fishing, which is common elsewhere in the Pacific.⁵⁶
- Hair topknots and particularly reddened hair topknots appear in Polynesia and America but not elsewhere in the Pacific.⁵⁷
- Cutting off a finger as a sign of mourning is a commonality between American and Polynesian natives, but it is not found elsewhere in the Pacific.⁵⁸
- The cannibalistic practices of Maori and Northwest Indians are similar.⁵⁹
- Maori and Northwest Indian traditions have many similarities including culture heroes,⁶⁰ sun-binding myths,⁶¹ departed spirit voyages and direction,⁶² and ancestral voyages from frozen climes.⁶³ They also use many virtually identical place names.⁶⁴
- The sweet potato, which is very popular in Polynesia, came from America and has the same name in both places.⁶⁵ The same is true of the American hibiscus flower.⁶⁶
- The cotton that is found in Polynesia has American, not Asian antecedents.⁶⁷
- The American bottle gourd, or calabash, is found in Hawaii but not elsewhere in the Pacific.⁶⁸

56 Heyerdahl, *Kon-Tiki Expedition*, 138–139.

57 Heyerdahl, *Kon-Tiki Expedition*, 131.

58 Heyerdahl, *Kon-Tiki Expedition*, 140.

59 Heyerdahl, *Kon-Tiki Expedition*, 144.

60 Heyerdahl, *Kon-Tiki Expedition*, 151.

61 Heyerdahl, *Kon-Tiki Expedition*, 152.

62 Heyerdahl, *Kon-Tiki Expedition*, 152

63 Heyerdahl, *Kon-Tiki Expedition*, 152–153.

64 Heyerdahl, *Kon-Tiki Expedition*, 757–763.

65 Heyerdahl, *Kon-Tiki Expedition*, 367, 389, 429–439.

66 Heyerdahl, *Kon-Tiki Expedition*, 485.

67 Heyerdahl, *Kon-Tiki Expedition*, 446–453.

68 Heyerdahl, *Kon-Tiki Expedition*, 439–446.

- There are other Andean plants in Hawaii which predate European discovery and are not found elsewhere in the Pacific.⁶⁹
- Yam beans from South America appeared in Tonga and Fiji before European discovery.⁷⁰
- If Polynesian origins are all Asian, why did the Polynesians not use rice in pre-European times? (Note that Heyerdahl speculates as to the reason why maize, too, does not exist in the Pacific.)⁷¹
- Cane and reed rafts appear in both America and Polynesia (but not elsewhere in the Pacific).⁷²
- Both American and Polynesian traditions feature large, navigable freight rafts maneuvered dexterously with centerboards.⁷³ While double-hulled canoes appear elsewhere in the Pacific, the rafts do not.
- The Maori word *totara* is the same word used by Peruvians to describe the most buoyant wood for watercraft.⁷⁴
- Both the Polynesian and American calendars focus on the *Pleiades*.⁷⁵
- The same flutes and gourd whistles are used in Polynesia as in Peru.⁷⁶
- Maori-Polynesian fighting methods, like the Northwest American Indian peoples feature slings and striking weapons rather than the bow and arrow more familiar in Asia and elsewhere in the Pacific.⁷⁷

69 Heyerdahl, *Kon-Tiki Expedition*, 469–473.

70 Heyerdahl, *Kon-Tiki Expedition*, 475.

71 Heyerdahl, *Kon-Tiki Expedition*, 488–496.

72 Heyerdahl, *Kon-Tiki Expedition*, 516–620.

73 Heyerdahl, *Kon-Tiki Expedition*, 538–553.

74 Heyerdahl, *Kon-Tiki Expedition*, 582.

75 Heyerdahl, *Kon-Tiki Expedition*, 644–649.

76 Heyerdahl, *Kon-Tiki Expedition*, 670–680.

77 Heyerdahl, *Kon-Tiki Expedition*, 695–697.

- The fishhook types used by the Northwest Americans are more similar to those used in Polynesia than are the fishhooks used elsewhere in the Pacific, which have more Asian affinities.⁷⁸

Most scientists do not give Heyerdahl a fair hearing. Surely some material here bears further scientific investigation. Suggs's treatment of these findings is a good example of the customary unfairness attributed to Heyerdahl's research. While his denigration of Heyerdahl on grounds of anachronism cited above is a good read, it is much more disdainful than it needs to be and endorses the thought that Suggs was writing to an agenda. When that excessive mockery is coupled with the further fact that Suggs disdainfully denies the significance Heyerdahl placed upon the South American origins of the sweet potato or *kumara* (which has been vindicated by later scholarship), we have cause to set Suggs's views to one side as lacking desirable scholarly objectivity.

Nevertheless, it is not necessary to give Heyerdahl all the credit he has soaked up because of his fame and his notoriety. Many others have been prepared to concede more nuanced Pacific history and colonization than the current mainstream. Although Robert Heine-Geldern castigates Heyerdahl for all the culture he traces in Polynesia from America and particularly Peru⁷⁹ (Heine-Geldern says a stronger case can be made for those same cultural traits as having come from Asia⁸⁰), he does credit Heyerdahl with resurrecting the Roland Dixon-WJ Thompson theory that the *kumara* was fetched to Polynesia from America by two-way journeys originating in Polynesia.⁸¹ This same point has been given new life by Jose

78 Heyerdahl, *Kon-Tiki Expedition*, 129, 697–700.

79 Robert Heine-Geldern, "Heyerdahl's Hypothesis of Polynesian Origins: A Criticism," *The Geographical Journal*, October–December 1950, 183–192.

80 Heine-Geldern, *Heyerdahl's Hypothesis*, 183–192. In this respect, Heine-Geldern does not deviate at all from mainstream anthropology.

81 Heine-Geldern, *Heyerdahl's Hypothesis*, 190.

Ramirez's recent discovery of pre-Hispanic chicken bones in Southern Chile, which he says evidence some Hawaiian-Chumash connection.⁸² While Robert Langdon was generally seen as a lovable nutter by university anthropologists, most will concede there may indeed be some Spanish DNA in parts of the Pacific for the reasons he states,⁸³ but they could not concede all that Langdon claimed as consequences of that concession.⁸⁴

However, when one weighs together the work of all the Pacific anthropologists I have cited, it is remarkable how few have addressed the work of John Sorenson. I believe future generations will come to regard Professor Sorenson as one of the giants of anthropological scholarship, on whose shoulders others should have stood much sooner. Whereas he has made no claim to have been a Pacific anthropologist,⁸⁵ it is not just his magnum opus referred to above that should have been considered more seriously by the mainstream. He wrote his master's thesis on this very subject in 1952⁸⁶ after serving an LDS Church mission in Rarotonga, where he participated in amateur radio contact with Thor Heyerdahl's *Kon-Tiki* raft en route in 1947.⁸⁷ His monumental two-volume bibliography titled *Pre-Columbian Contact with America across the Oceans: An Annotated Bibliography*, first published in 1990 with Martin L. Raish⁸⁸ (updated and expanded in 1996),⁸⁹ contains abstracts

82 See note 48.

83 Robert Langdon, *The Lost Caravel* (Sydney: Pacific Publications, 1975).

84 For example, see Howe, *Origins*, 130–132, 144.

85 John Sorenson, personal e-mail correspondence, January 4, 2013. Professor Sorenson is better known to Latter-day Saint scholars because he has challenged and vastly extended the boundaries of Book of Mormon scholarship beginning with his seminal book, *An Ancient American Setting for the Book of Mormon* (Salt Lake City, Utah: Deseret Book, 1985).

86 John L. Sorenson, "Evidences of Culture Contacts between Polynesia and the Americas in Pre-Columbian Times," (master's thesis, Brigham Young University, 1952).

87 John L. Sorenson, personal e-mail correspondence, January 4, 2013.

88 Sorenson and Raish, *Pre-Columbian Contact*, 1996.

89 Sorenson and Raish, *Pre-Columbian Contact*, 1996.

of more than 5,000 books and articles both for and against claimed or actual transoceanic voyaging and constitutes virtually the total relevant literature on the question up to the time of publication. Many of Professor Sorenson's abstracts in that work involve Polynesia, and although he turned 89 years old in 2013, he has a new text on diffusion ready for publication⁹⁰ to add to additional work published on this subject matter in 2004,⁹¹ 2006,⁹² and 2009.⁹³

Conclusion

The bottom line is that it is unscientific for contemporary anthropologists to perpetuate an understanding of Pacific anthropology that misleads their students and the public (especially the Pacific Island public) into believing that their story is a completely self-contained one of evolutionary Asian origins. On the balance of probabilities, it must be accepted that the story is much more nuanced than that. It is not only possible but likely that both drift and planned voyages from Latin and North America are a part of that story. It is also likely that Robert Langdon's idea that sailors from some lost European caravels may have contributed some of their DNA to the Pacific gene pool as well. Although species including *Homo sapiens* can adapt quickly to their environments, it is difficult to account for all the physiological differences in the Pacific by simple reference to local environments. People simply have not inhabited the area long enough. Certainly there are inter-island

90 John L. Sorenson, personal e-mail correspondence, January 4, 2013.

91 Department of Asian and Middle Eastern Studies, "Scientific Evidence for Pre-Columbian Transoceanic Voyages to and from the Americas," *Sino-Platonic Papers*, 133, CD-ROM ed. (Philadelphia: University of Pennsylvania). www.sino-platonic.org.

92 Victor H. Mair, ed., *Perspectives on the Global Past*, vol. 2, *Biological Evidence for Pre-Columbian Transoceanic Voyages*, in *Contact and Exchange in the Ancient World* (Honolulu: University of Hawai'i Press, 2006), 238–297.

93 Sorenson and Johannessen, *Exchanges Before 1492*.

environmental differences, but the Houghton suggestion that the Polynesian part of the Pacific Ocean is an essentially cold place, which has biologically required the evolution of some of the largest human bodies in the world's history, does not stack up too convincingly.

This article is thus a call to Pacific anthropologists to write the story a bit larger and perhaps to look scientifically for other possible explanations for the origin of mankind in the region. Would it be so bad if among all they said, they acknowledged that the early diffusionists may have gotten some things right, albeit for the wrong reasons?

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