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A Reply to Bahn and Flenley on Easter Island Prehistory

Robert Langdon

In the latest issue of the *Journal of Pacific History* (vol.28:284-6), there is a review of the Paul Bahn/John Flenley book *Easter Island, Earth Island* (Thames and Hudson, London 1992) which has prompted me to say a few things about the book myself. The reviewer, Rapa Nui specialist Grant McCall, says that Thor Heyerdahl is 'unlikely to be pleased' at the book's 'consistent dismissal' of his work and that my research into Easter Island's prehistory is cited three times and refuted.

The *Shorter Oxford English Dictionary* defines 'refute' as: to prove a person in error, confute, disprove, overthrow by argument, prove to be false, or demonstrate an error. As Bahn and Flenley have done none of those things in relation to myself as far as I can see, I think a reply from me in RNJ on what they do say would be salutary as Heyerdahl's position is also involved.

The first point on which Bahn and Flenley allegedly refute me is on p.12 of their book. It relates to the discovery in 1971 that 18 Easter Islanders with no known non-Easter Island ancestors were carriers of certain genes that are peculiar to Europeans and especially common among Basques. The genes, called A29, B12, are a linked pair or haplotype in the Human Leucocyte Antigen (HLA) system. This system, which was only discovered in the 1950s, is of critical importance in the transplantation of human organs. If the HLA genes of a donor and patient are incompatible, a transplantation cannot succeed.

Although the HLA genes occur in millions of different combinations, geneticists soon found that there were 'preferential associations' among people of different races, which made them 'a marvellous new marker for anthropological research' (Teresaki 1973). In 1970-72, teams of geneticists undertook a world-wide project to map the distribution of the HLA genes through serological tests among communities long isolated by geographical, religious and other barriers. It was through these tests that 18 out of 49 reputedly pure-blooded Easter Islanders were found to be carriers of the same genes as 7.9 per cent of the people in the remote Basque village of Macaye in the French Pyrenees (Thorsby and others 1973, Dausset and others 1973).

By a coincidence, Professor Jean Dausset, of Paris, a co-discoverer of the HLA system and a Nobel Prizewinner for Medicine and Physiology in 1980, was involved in both the Easter Island and the Macaye tests. He and his colleagues went to considerable pains to ensure that their Easter Island subjects were of pure Easter Island descent. They were therefore extremely surprised by their findings and concluded, lamely, that

as the A29, B12 haplotype was 'characteristic of European Caucasoids', the possibility of 'recent admixture by foreign visitors (prior to about 1870)' could not be excluded.

The results for Easter Island of the world-wide serological tests (Dausset and Colombani 1973) came to my attention in 1974 as I was writing the last chapters of my book *The Lost Caravel* (Langdon 1975). As I had already deduced from other evidence that some Easter Islanders of Captain Cook's time were partially descended from the crew of a long-forgotten Spanish ship of 450 years earlier, I was able to propose an explanation for the findings of Dausset and his colleagues that they themselves could never have dreamed of. The islanders with the Basque genes, I wrote, were descendants of one of the Basque seamen of the caravel *San Lesmes* that had disappeared in the eastern South Pacific in 1526 on a voyage to the East Indies.

In the earlier chapters of *The Lost Caravel*, I had used various kinds of evidence to reconstruct the fate of the *San Lesmes* and its crew. Beginning with the discovery of four ancient iron cannon on the reef of Amanu Atoll, 800 km east of Tahiti, I argued that the caravel had run aground there by night, that the crew had saved their ship by pushing their heavy guns overboard, and that they had then pursued a westerly course until reaching the island of Ra'iatea, 200 km northwest of Tahiti. There they had either repaired their ship or built another, after which many of the men set out to try to return to Spain by sailing southwestward for the Cape of Good Hope. This course, however, took them only as far as the North Island of New Zealand where for some reason they settled.

Meanwhile, some of the men who had remained behind at Ra'iatea married into chiefly families and so established Hispano-Polynesian dynasties that lasted down to Captain Cook's time. During that 250-year interval, some islanders of part-Spanish descent were evidently drifted southward to Ra'ivavae in the Austral Group, whence some of their descendants eventually reached Easter Island. Among the evidence advanced for the latter part of my reconstruction were descriptions by the European explorers of Cook's time of light-featured, European-looking people at Ra'iatea, Ra'ivavae and Easter Island as well as traditions from those islands about a man called Hiro who, on genealogical grounds, was estimated to have lived in the 16th century.

The *Lost Caravel* won me a research fellowship at the Australian National University to carry out further research into the influence of 16th century Spanish castaways in the Pacific. I later published a revised and expanded version of the book (Langdon 1988) which

included a new chapter on Easter Island. This took account of correspondence I had had with Dausset on my explanation for his Easter Island discovery which he himself had published (Dausset 1982) as well as research into the genealogies of all the Easter Islanders with Basque genes. A feature of the chapter was a family tree showing the inheritance of Basque genes on Easter Island. This revealed that all carriers of such genes are/were descended from a single man, Pakomio Maori, who, in 1886, was estimated by the surgeon of the *USS Mohican* to be 'at least the Biblical three score and ten', or born in about 1816 (Cooke 1899:713). Two photographs of Pakomio taken in 1886 and preserved in the Smithsonian Institution, Washington, were reproduced. These, I said, revealed Pakomio as a man of 'moderate height, with light hair, a white, stubby beard and strikingly Caucasian features'. I also mentioned that in 1977 a noted Easter Islander, the late Victoria Rapahango, then 80, had told me that Pakomio had died when she was nine or 10 years old and that she remembered him as having had red hair and blue eyes. I quoted descriptions from Easter Island literature of descendants of Pakomio with similar features.

What evidence do Bahn and Flenley use to 'refute' my claim that Pakomio and his kin were/are descended from one of the Basque sailors of the *San Lesmes*? They say:

We may never know for sure which, if any [European] vessels called at Easter Island in the 17th century or earlier, but one scholar, Robert Langdon, believes that a lost Spanish caravel, the *San Lesmes*, was shipwrecked on a reef east of Tahiti around 1536 [sic], some of its crew intermarrying with Polynesian women; their descendants eventually reached Easter Island and donated Basque genes, which are still found there. There is some genetic support for Langdon's theory: recent analysis of HLA (Human Leucocyte Antigen) groups, a tissue type system useful in the preparation of medical transplants, have revealed that eighteen people of 'pure' Easter Island stock possess a combination of genes that is frequent among Basques (the Basque haplotype). They can be traced back to one islander of the late 19th century, and prove that at least one Basque passed through. However, there is no chronological dimension to these analyses: that is, one cannot assume that Basque genes arrived via the *San Lesmes* or any other early ship. There were hundreds of whalers in this part of the Pacific during the 19th century, and Basques were usually preeminent in this industry. We know virtually nothing about the routes or landfalls of all these doubtless lusty crews.

Bahn and Flenley misrepresent my case on three important points. First, by saying that Pakomio Maori

was an islander of the 'late 19th century', they conceal the fact that he was born in 1816 or thereabouts and so give unjustified substance to their own claim that his father could have been a Basque sailor of that century. Next, by saying that there is 'no chronological dimension' to analyses of the known genetical facts, they seek to deny that Pakomio's own lifetime had a fixed dimension and that the margin for error in estimating his crucially important date of birth is necessarily small. Finally, Bahn and Flenley misstate the facts when they say that the presence of Basque genes on Easter Island proves that 'at least one Basque passed through'. It doesn't. All it proves is that Pakomio's father or his mother was also a carrier of the Basque haplotype.

As explained elsewhere (Langdon 1988a:200), every person carries two HLA haplotypes, one inherited from his/her father and one from his/her mother. The genes are co-dominant. So everyone has a 50:50 chance of acquiring either one of the two haplotypes of his/her parents. Therefore if Pakomio's mother had been the carrier and transmitter of A29, B12, she, obviously, would have been an Easter Islander. By analogy, if Pakomio's father had been the carrier, he could equally have been an Easter Islander. But in theory, at least, he could also have been a Basque sailor of the early 19th century.

All Basques, however, do not carry the A29, B12 haplotype. As already noted, only 7.9 per cent of Macaye Basques do - and that is the world's highest known figure. So the chances of a male Basque with those genes turning up at Easter Island at any time are never likely to be much better than about two in 25. And because such a man might equally bequeath his non-distinctive genes to any child he might father there, the odds that the child would acquire his A29, B12 haplotype would actually be only about one in 25. Yet it is even less likely in theory that Pakomio Maori could have acquired his distinctive genes from such a visitor. As a woman is fertile for only about 48 hours in her monthly cycle of 28 days, any Easter Island woman who had sexual relations with a fleeting European visitor at any time in the past would have had only a 14 to 1 chance of conceiving a child by him.

Other theoretical problems with the Bahn/Flenley hypothesis relate to Pakomio's red hair and blue eyes. As those traits derive from recessive genes, both of Pakomio's parents must also have been carriers of 50 per cent or more red hair and blue eye genes for those features to have been manifested in him. About 19.5 per cent of Basques in the Spanish province of Guipuzcoa do have blue, grey or green eyes and about 15.5 per cent have red hair (Langdon 1975:51). So here is a good chance that any lusty Basque on a visit to Easter Island will carry the genes for them. But, of course, Pakomio's mother could not have carried such genes unless she was already of part-European descent. And if she was, then that would destroy the Bahn/Flenley argument.

But it is scarcely necessary to pursue all these theoretical questions because islanders who were 'quite white' and others who were 'brown like the Spaniards' were reported on Easter Island by one of the first European visitors of 1722 (Dalrymple 1771:94). Also, European-looking people with chestnut, red and cinnamon-colored hair were noted in 1770 (Corney 1908:96,127). Yet again, 'hundreds of whalers' did not operate in the vicinity of Easter Island in the 19th century because these waters were largely devoid of whales. This is demonstrated by Lt M. F. Maury's 'whale chart' of the world, published in 1851, which was based on a study of numerous American whalers' logbooks (Starbuck 1964). It is also demonstrated in a volume that I myself edited which lists the Pacific ports and islands visited by more than 2,100 American whaling ships in the 19th century (Langdon 1984). That volume was also based on the whalers' logbooks. It shows that only 43 of the 2100 ships in our survey put into Easter Island between 1807 and 1907, and that only 29 others sighted it. The *Thomas*, the first caller, was there on 17 March 1821, or about five years after Pakomio Maori is thought to have been born. The second, the *Lydia*, arrived in February 1827.

Basques, in any case, were never 'pre-eminent' in the Pacific whaling industry. Except in the period from 1789 to 1812 when a good number of British whalers were active, the Americans were always dominant (Stackpole 1953, Starbuck 1964). Their ships operated out of such New England ports as Nantucket, New Bedford, Salem, Boston, Stonington, Hartford and New Haven - unfruitful recruiting grounds, one would think, for Basque crews. No Spanish whalers went to the Pacific. The first French whalers to do so - one from Havre and three from Nantes - did not appear until 1818-22 (Faivre 1953:229).

In Langdon 1988 are the names of three Basques who sailed in the *San Lesmes* together with their places of domicile, parents' names, etc. The same details are given for several others who are likely to have joined the caravel in South America. My claim that the genes of at least one such Basque reached Easter Island some time before Roggeveen in 1722 is necessarily based on circumstantial evidence. But it is on such evidence that murderers are convicted. Bahn and Flenley, by contrast, offer only wishful thinking to support their claim that Easter Island's Basque genes can be attributed to some unknown Basque from some unknown ship of the 19th century.

The second study of mine that Bahn and Flenley refute, according to McCall, is a comparative study of the Easter Island language that I made in collaboration with the linguist Darrell Tryon (Langdon and Tryon 1983). In summarizing its findings, Bahn and Flenley begin by asserting that, like Heyerdahl, Tryon and I try 'to turn the old Rapa Nui tongue into a link between Polynesia and South America'. They go on:

... Langdon and Tryon claim that, at the time of contact, Rapa Nui's language was made up of three elements, one of west Polynesian origin, one from east Polynesia, and the third of unidentified origin. The first two elements were allegedly fused on the island of Ra'ivavae, 500 km (311 miles) south of Tahiti, and this language was then carried to Easter Island no earlier than the 16th century. The third element, comprising words unknown in other Polynesian languages, was the remnant of a non-Polynesian tongue which could only have come from the east in ancient times. However, other specialists such as Roger Green have shown that there is only weak and selective evidence of a pre-Polynesian language on Easter Island. Langdon and Tryon conjure up a very complex picture, with influences going back and forth across the ocean, and with repeated borrowings which are unmarked and undetected in Easter Island linguistics. The standard, orthodox view is far more straightforward and accounts for the evidence more economically and quite satisfactorily: this is... [that] the language of Easter Island is a member of an eastern Polynesian subgroup.

The foregoing exaggerates the complexity of the Langdon-Tryon findings; it does not explain that, in our view, many of the 'lexical innovations' in the eastern Polynesian languages were actually borrowings from a pre-Polynesian Easter Island language; and it gives the impression that Green (1988) differs substantially from us on how the Easter Island language should be classified. In fact, Green's conclusions and ours on the classification question are much the same. We said that, from a 'purely linguistic point of view', Rapa Nui's basic make-up could be explained as either (1) an eastern Polynesian language that had broken away from an ancestral language of that area at an early stage, thereby retaining many features that were lost in other eastern languages, or (2) a language of Futunic origin that had borrowed heavily from an eastern Polynesian source. We said we preferred the second hypothesis for non-linguistic reasons. Green said the linguistic evidence provided 'a reasonable basis' for accepting the first hypothesis and that non-linguistic evidence was not in conflict with it.

Green's conclusion was hardly surprising: he was defending a position he had taken 17 years before our study appeared (Green 1966). However, he agreed with us that, 'in the light of wind patterns', the Austral Islands, Mangareva or Pitcairn were the best starting points for voyages from Polynesia to Easter Island, and claimed that Polynesians could have made such a voyage from there as early as the 4th century AD. He admitted, though, that he knew of no archaeological evidence that supported his hypothesis. Moreover, although he did not accept the idea of a pre-Polynesian

language on Easter Island, he conceded that the 'lexical innovations' that separated Rapa Nui and other eastern Polynesian languages from those of the west were 'fairly numerous'.

As the results of my genealogical inquiries about Pakomio Maori and his descendants had not been published when the Langdon/Tryon monograph went to press, we did not use HLA evidence to support our case that Rapa Nui was basically a Futunic language that had been overlain by eastern Polynesian borrowings. However, we did refer to the Ra'iatea, Ra'ivavae and Easter Island traditions about Hiro and the dating of his lifetime to the 16th century. We also pointed out that, according to Easter Island tradition, the island was already inhabited when Hotu Matu'a, its Polynesian 'founding father', arrived there. Moreover, if the predecessors of Hotu Matu'a had not been non-Polynesians, then it was scarcely likely that Rapa Nui would now contain so many words that were unknown in all other Polynesian languages. On the other hand, we did NOT try to 'turn the old Rapa Nui tongue into a link between Polynesia and South America'. We merely said that except for the possibility that *kumara*, the Easter Island word for sweet potato, might be related to *cumar* 'sweet potato' in the Chinchasuyo dialect of the Quechua language of Ecuador, there were, as yet, 'no linguistic clues to the origin of Easter Island's apparent pre-Polynesian inhabitants'. However, we thought the names of several other cultivated plants might one day prove to be important clues. This was because the plants themselves were either of American origin or had been absent in western Polynesia in pre-European times. Also, their names were exclusive to eastern Polynesia and so seemed likely to have reached Easter Island from the Americas.

Green did not discuss the question of the sweet potato in his paper, nor the evidence that Tryon and I brought forward about Hiro. Bahn and Flenley do not breathe a word about Hiro, either. However, while they claim (p.51) that 'all recorded folk tales' from Easter Island 'need to be taken with a large pinch of marine salt', they accept - as Tryon and I do - the 'rich folk memories' of the islanders that 'support the case for colonization from the west' (p.48). On the other hand, because they have an *idée fixe* that Easter Island was settled from Polynesia in the early centuries of the Christian era and not resettled until post-European times, they give short shrift to any evidence that indicates otherwise. Thus, although they say (p.51) that some progress might be made in reconstructing Easter Island's prehistory by 'taking a look at the more tangible botanical information', any suggestion of a link with South America is quickly brushed aside. Their consideration of a third study of mine called 'Manioc, a long concealed key to the enigma of Easter Island' (Langdon 1988b) is typical of their approach.

In Langdon 1988b, I reveal how, at the turn of the

present century, an English scholar, Bolton Glanville Corney (1908), falsified the records relating to the Spanish expedition to Easter Island of 1770 under Captain Felipe Gonzalez when he translated them into English for the Hakluyt Society. In those days, the American origin of the sweet potato had not been established and no Heyerdahl had come forward to argue for prehistoric contact between South America and Polynesia, far less for the settlement of Easter Island by American Indians. Hence, when Corney came across references to the sweet potato in the records of the Gonzalez expedition, he was unperturbed because its presence there at the time of contact was not in conflict with the ethnobotanical views of his time. But when he also found references to manioc, or what the Spaniards called *yuca*, he balked, for manioc, as he well knew, was a plant of American origin, otherwise known as cassava or tapioca.

These days, scholars who come across seemingly out-of-place references to plants and such like in translating texts from one language to another undoubtedly remark on them in scholarly footnotes. Corney did not do this for *yuca*. In one case he silently translated it as taro; in another he left it untranslated and obfuscated its meaning with a misleading footnote; and in others again, he simply omitted, or omitted mention of, documents that referred to it.

As the original Spanish documents on the Gonzalez expedition had not been published then and remained unpublished until less than a decade ago, Corney's mishandling of the evidence on manioc was unknown both to Heyerdahl and to his opponents through all the sound and fury of the immediate post-Kon-Tiki years. However, when the document containing Corney's one untranslated reference to *yuca* was published in Montevideo in the original Spanish in the 'seventies (Blixen 1977), I immediately became suspicious of it. And I found eight other references to the same plant when most of the Spanish documents then known were later published in Madrid (Mellen 1986). But old ideas die hard. Just as many European astronomers, for 200 years after Copernicus, could not believe that the earth was not the centre of the universe, so Bahn and Flenley could not believe that any new evidence could be found 200 years or so after Gonzalez that might seriously challenge the long-held, but unproven theory that Polynesians were the first and only prehistoric settlers of Easter Island. Accordingly, they had this to say (p.53) about my manioc paper:

It remains to be seen what credence can be placed in Langdon's theory that what the Spaniards described as *yuca* was manioc: for a start, it relies on the testimony of a couple of 18th-century Spanish pilots, neither of whom presumably was very skilled at botanical identification. Secondly, Forster, the first botanist to visit the island, clearly recorded taro

rather than cassava only four years later; and Langdon himself points out that Thomson made no mention of the plant in his careful list [of 1886]. It is hardly a crop that can be missed, for it is not seasonal, and has a large and distinctive top growth; yet it does not appear in any account until the thorough botanical survey of 1911! Langdon is driven to imagine a scenario whereby the final Polynesian settlers who came to the island neglected the plant, which was unfamiliar to them; so the manioc supposedly reported in 1770 became extinct, but the plant was reintroduced before 1911...Clearly, an awful lot of assumptions are being made here.

As may be seen, Bahn and Flenley were not perturbed by Corney's misdeeds as a translator. Their only concern was to defend their belief that American Indians had played no role in prehistoric Easter Island. To do this they assumed that:

(1) The presence of the American sweet potato on Easter Island in 1770 was of no significance and that manioc and other plants of American origin or provenance could not possibly have been there, too.

(2) The Spanish evidence really does rest on the testimony of only two pilots.

(3) Although the Spaniards came from Peru where manioc is a staple crop, not a single one of them would have recognized it, despite its 'distinctive top growth' that can 'hardly be missed'.

(4) What the Spaniards described as *yuca* could only have been taro because Forster recorded taro four years later.

(5) All Easter Island crop plants of the 1770s were cultivated in all parts of the island so that Forster, who saw only a small part of it on foot, would have been just as likely to see manioc as the Spaniards who anchored at a different spot, landed at several widely-separated places in circumnavigating it, closely examined the northeastern sector, and stayed several days longer than he did.

(6) In 1770 the Easter Islanders would not have been divided into two broad camps: one being predominantly Polynesian, cultivating Polynesian foods; the other predominantly American Indian, cultivating American foods.

(7) If the situation envisaged in (6) had existed, the Polynesians would not have neglected to cultivate manioc after the American Indians were killed off, died out or were absorbed into the Polynesian way of life.

Only a few points need to be made about this 'awful lot of assumptions'. First, although the Spaniards undoubtedly did know manioc when they saw it, they are unlikely to have been familiar with taro, then little known in South America (Plucknett 1976). In their ignorance, they evidently described taro as achira (*Canna edulis*), this being an Andean crop plant whose leaves and tuber somewhat resemble those of taro (Gade

1966). As achira and *yuca* appear together in three of the Spanish documents, *yuca*, obviously, could not have been taro if achira was.

Secondly, manioc is referred to in a first-hand account of the Gonzalez expedition in the British Library, London, that was unknown to Corney and obviously written independently of the two pilots whom Bahn and Flenley mention. It was first published in the same year as my manioc paper (Mellen 1988).

Finally, if Corney's misdeeds over manioc were not bad enough, I have now discovered that in his translation of one Spanish document, he actually omitted a 300-word passage in which a senior officer of the Gonzalez expedition reported finding maize, white potatoes and manioc under cultivation, along with bananas, sugarcane and gourds. Of these plants, only sugarcane seems likely NOT to have come from South America in the light of recent studies (Whistler 1990; Langdon 1989, 1992, 1993).

I will publish a paper on Corney's newly-discovered transgression in due course. Meanwhile, scholars such as Bianco 1990, Chapman 1993 and Wallin 1993 have recently presented findings that do not support the Bahn/Flenley view that Heyerdahl's work should be consistently dismissed or that Hispano-Polynesians had no part in Easter Island's late prehistory.

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