REPLY TO ROBERT LANGDON

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Since Mr Langdon's grievance seems to be primarily with Grant McCall's claim that his arguments were "refuted" in our book, his remarks should perhaps more properly have been directed elsewhere. Our book made no such claim; we merely set out some points where we disagreed with Langdon or found his views unconvincing. However, since his article is clearly directed at ourselves, a brief response is required.

The Basque story was mentioned in passing in our book, in a single introductory paragraph (p.12), merely as an entertaining curiosity which had no direct relevance to our main topic, the rise and fall of the island's culture. Professor Jean Dausset, the discoverer of the improperly named "Basque haplotype" on the island, also stressed (1982) that (a) the gene was also common in southern France; (b) he could not be sure that all the "pure-blooded" islanders tested were pure-blooded; and (c) the exact date of the gene's arrival on the island will always be unknown, except that it was pre-1870. Even if one accepts Langdon's arguments in favor of a pre-1816 introduction of the gene, not much progress can be made: there could easily have been an 18th century crew member who was carrying southern French genes, or a visit by some ship that we know nothing about. In any case, Langdon is ignoring the fact that our book clearly set out the likelihood (p.13) that the Dutch of 1722 were not the first European visitors to the island. We are puzzled by Langdon's deduction 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"--this would put Spaniards in the Pacific in the 1320s: some mistake surely?

His speculations about the chances of a Basque with those haplotypes "turning up at Easter Island at any time" are irrelevant, since it seems that one did so. We know that the chances of Easter Island ever being discovered in the first place were virtually nil, yet it was. We also fail to see the relevance of the odds related to a woman's fertility cycle: why should only one woman have been involved?

Whether or not it was a Basque from Langdon's beloved *San Lesmes* who was ultimately responsible for the presence of the gene on the island is not something we propose to lose any sleep over. However, it is worth mentioning that Dausset's most recent campaign (1983) of HLA testing in the South Pacific (reported in *Les Mystères Résolus de l'Ile de Pâques* 1993, p.452)--in a number of areas including the Amanu atoll of the Tuamotus, where Langdon believes the *San Lesmes* ran aground--seems to have found no trace whatsoever of the "Basque gene". It appears to be peculiar to Easter Island, and only to Easter Island.

Where language is concerned, the fundamental point is there is absolutely no linguistic evidence whatsoever for a pre-Polynesian substratum or a so-called Second Wave of Polynesian immigrants (see Fischer 1992). Reviews by a number of specialists of Langdon & Tryon's book have shown that the Futunic factor can be completely discounted. As for what Langdon calls "lexical innovations", they should more accurately be termed morphological innovations, i.e. changes of known Polynesian words into other forms, and these internal alterations of the autonomous language are best and most easily explained by the islanders' long isolation. Unlike Langdon, moreover, we place minimal credence in 19th and 20th century oral traditions about Hiro, Hotu Matu'a or anyone else, for reasons explained in our book. To seek "linguistic clues to the origin of Easter Island's apparent pre-Polynesian inhabitants" is a wholly unwarranted pre-supposition that there *were* any pre-Polynesian inhabitants. We have no evidence at all for them, linguistic or otherwise.

We consider Langdon's references to Corney, using words like "falsified", "mishandling" and "misdeeds" to be not only distasteful and impolite towards a scholar who is dead and cannot defend himself, but also overly melodramatic, as if Corney were part of an insidious conspiracy to conceal evidence of manioc on Easter Island and hence links with South America. At worst, Corney may be guilty of careless work or of mistranslation. But Langdon's entire argument rests on his own chosen interpretation of the Spaniards' use of the word "yuca" in 1770. He blithely assumes that the Spanish mistook taro for achira, which helps him explain why achira *and* yuca were mentioned in Spanish accounts; yet taro and achira are not very similar--the leaf-shape is completely different, being cordate in taro and ovate in achira. Manioc is even more different (having palmate leaves) and much taller, but the pendant nature of its leaf blades is much closer to taro than to achira. In short, anyone capable of confusing taro with achira could certainly have confused taro and manioc. The article by O. Blixen cited by Langdon specifically wonders if the Spanish identification of yuca was correct, and we feel that Blixen's skepticism was fully justified.

The passage about maize, white potatoes, etc, was published by Mellén Blanco in 1986 (pp. 133,228). While the main Spanish party went to Poike, Alberto de Olaondo went to "the interior of the island" where, he said, the natives had these crops. As Mellén Blanco says, "es el único cronista que señala estas especies."

No matter how many accounts of the González expedition turn up, whether by pilots or senior officers, the fact remains that there were no trained natural historians, let alone botanists, involved--de Olaondo was a naval lieutenant and a captain of infantry--and hence little faith can be placed in the accuracy of their identifications. Forster, on the other hand, was a trained botanist, and only four years later he produced an expert and sizable list which recorded taro but not manioc--and certainly not maize or white potatoes! There are two possible explanations for this situation:

1) The Spanish pilots and de Olaondo were hopelessly inaccurate in their attempts at botanical identification. It is most probable that the Spaniards' yuca was Forster's taro, and the maize and potatoes were a figment of the imagination, since the Dutch in 1722 never mentioned them, and nor did anyone else after 1770. No visitor to the island was ever given a corn-cob or a white potato.

2) Manioc, white potatoes and maize were all being grown in some special part of the island (the "Secret Garden"?) in 1770 by a "predominantly American Indian" population, sticking to the good ol' foods from home rather than the new-fangled Polynesian crops. Perhaps this was the refuge of Ororoine, the only survivor of the Poike Ditch conflagration, according to that ever-popular legend: the "Lone Long-ear" or the "Manioc Maniac" as one might now call him. And, according to Langdon, Forster just didn't happen to see or hear of this remarkable collection of crops--an odd fact since, as Mellén Blanco points out (p. 132), the Spaniards mentioned plantations of yuca "en los lugares por ellos visitados y distantes varios kilómetros unos de otros." Can Forster have missed *all* these places?

Needless to say, we prefer the first explanation. The second cannot be "refuted"--it is theoretically possible despite the absence of any evidence for a pre-Polynesian population--but it is rather like saying that even though the few moon-landing sites were on rock one can still cling to the hope that the rest of it is made of green cheese.

Reference

Fischer, S.R. 1992. Homogeneity in old Rapanui. *Oceanic Linguistics* (Honolulu) 31: 181-190.