

On Langdon's (1989) East Polynesian Plant Study

Jeff Marck¹
Australian National University

Introduction

Robert Langdon (1989) has recently presented a discussion of Eastern Polynesian (EP) plant names suggesting a number of distributions purported to be at variance with the genetic relations attributed to the languages of the area by linguists. He concludes that:

If the evidence above is now looked at in its entirety, it will be seen that it offers no support whatever for the theory that Eastern Polynesia was settled from the west by a single group of immigrants whose language became the proto-language of the region. On the contrary, the evidence suggests at least three separate migrations from the west as well as a series of migrations within the region in which the Society Islands played a dominant role linguistically and in other ways. The three main migrations were: from Tonga to the Marquesas Islands, from Samoa to the Society Islands, and from either Wallis Islands or Futuna to Easter Island. Because of the great distances, each of the main migrations may well have involved at least one intermediate island. Ra'ivavae in the Austral Groups has already been suggested in the case of Easter Island. (Langdon 1989:324)

Langdon's complaint that someone has proposed a "theory that Eastern Polynesia was settled from the west by a single group of immigrants" is unmotivated. If he is talking about recent work by archaeologists and linguists, no one has proposed such a theory. Why he has assumed the theories of one or the other discipline call for such a scenario is never made clear. All the linguists have ever claimed is that a unified dialect or language, which developed somewhere in Eastern Polynesia itself, is ancestral to all modern Eastern Polynesian languages other than Pukapukan. The claim that this language, Proto Eastern Polynesian, developed predominantly out of a Samoan rather than Tongan type of speech is a separate claim. This second claim does not preclude demographic or linguistic input from Tonga, Fiji or other areas in the west. It only asserts that a Samoan (Nuclear Polynesian) type language predominated rather than a Tongan (Tongic) type language. In fact, it now seems that it was specifically Samoa or the Ellicean (cf. Howard 1981) speaking atolls which dominated the development of the early speech

¹ This work was done while on an Australian National University Ph.D. scholarship. Andrew Pawley offered substantial comments on earlier versions.

in Eastern Polynesia, at least in the use of pronouns (Wilson 1985) and in the sense of certain odd (sporadically innovative) pronunciations (Marck in press).

At the time of Langdon's (1989) publication, no linguist working on Polynesian problems had ever suggested that a particular phylogeny of linguistics precluded demographic, linguistic or other cultural input from groups other than a group's closest linguistic relatives. His erroneous assumption of such an assertion in conventional Polynesian linguistic theory is not unique. A linguist has more recently made just such an assumption regarding the demographic and linguistic development of Rapanui. After some otherwise trenchant and valuable linguistic analysis Fischer concludes that:

...one courageous canoeful of East Polynesians were the first and only ones to arrive on Rapanui until their descendants' historical encounter with Europeans in 1722, we must necessarily infer perhaps as long as 1,700 years of total isolation... (Fischer 1992:187)

Such inferences are not necessary, as Fischer (1992) actually claimed² or as Langdon believed linguists in general assumed. There could easily have been many kinds of cultural contact that simply did not result in any linguistic borrowing that we can detect today. Yet Langdon writes an entire paper based upon the assumption that linguists have been making this assertion. The work is superficially provocative and there are two or three useful original observations but little of the rest withstands close scrutiny.

Langdon (1989:305) opens by claiming that the concept of a proto language is vague. He suggests that proto languages "have only a limited degree of realism" unless attested by other forms of evidence. This trivialises the work of comparative linguistics. While the thoughtful linguist is ever in search of non-linguistic evidence to give a broader view of prehistoric communities, we do not require non-linguistic evidence to confirm the reality of a proto language community as Langdon implies, supporting his assertion with quotes and comment based on Pulgram (1961).

Langdon's first paragraph reads thus:

About 27 years ago, the linguist Ernst Pulgram (1961) published a thought-provoking paper entitled 'The nature and use of proto-languages.' While it was true, he said, that languages related through common descent were derived from a common ancestor language, this notion had only a limited degree of realism if unattested by other forms of evidence. For this reason,

² Fischer (1994) backs away from this position in later work, attributing it to Kirch (1984:266-8) and others (Fischer 1994:footnote 1) but this is not my reading of Kirch (1984:266-8) who seems to make no such assertion or assumption.

On Langdon's (1989) East Polynesian Plant Study

he suggested that proto-languages should only be considered creations for the convenience of investigating an otherwise inaccessible linguistic past. No claim should be made for their being real languages in any sense of the word unless and until sufficient non-linguistic evidence was available to fix them in time and place, and to associate them with an anthropologically, archaeologically and historically identifiable society. The reverse process, the creation of a society to go with an unattested, reconstructed language, was altogether improper, Pulgram said. Langdon (1989:305)

Langdon misinterprets Pulgram (1961) in some respects. Pulgram's narrow and idiosyncratic definitions of "reality" versus "realism" are easy to confuse and Langdon appears to have done so. Langdon also ignores the tepid reaction of other linguists to Pulgram's assertions. Priestly (1973:319, Footnote 12), for instance, associates it with an extreme view that never gained much acceptance. Pulgram was speaking of a problem concerning Proto Romance involving the dialect situation in Italy at the time of the Roman Empire (and the emergence of Romance onto other parts of Europe).

Three factors distinguish the Proto Eastern Polynesian (PEP) situation from that of Proto Romance:

1) There is no generally recognised interstage (reconstructable intermediate language) between Proto Romance and Proto Indo-European (other than Classical Latin) while there are at least seven interstages identified and reconstructed between PEP and Proto Austronesian, the common ancestor of all the Austronesian languages.

2) The first two interstages above PEP (Proto Nuclear Polynesian (PNP) and Proto Polynesian (PPN)) were fairly similar to PEP and not terribly remote from it in time.

3) Characterising the internal differentiation of Romance has been problematic while most of the subgroups of EP have been clear for some years: Easter and Pre-Central Eastern Polynesian (CE) first diverged from each other. Then Marquesic and Tahitic became distinct, Hawaiian emerging out of Marquesic and Maori out of Tahitic (Green 1966). This, combined with 1), allows for a more definitive characterisation of the phonology and vocabulary of Proto Eastern Polynesian than is possible for Proto Romance. Classical Latin, as it developed into Romance, was a dialect continuum for about 1,000 years, with no major geographical boundaries, whereas Eastern Polynesian split mostly into geographically remote daughters, allowing for a fair degree of independent development.

Langdon (1989:305) claims that such scholars as Green (1966), Pawley (1966), Biggs (1971, 1972, 1978), Bellwood (1978:127-128), Clark (1979) and Kirch (1985:63, 1986) "pay remarkably little attention to the question of who the Proto-Eastern Polynesians were, where they came from, how and when they could have reached Eastern Polynesia, and where, precisely, they first settled." Langdon simply misrepresents the facts in this matter. The works Langdon mentions are often concerned with precisely the issues he mentions (Biggs 1972:146-150, Green 1966:33-35) or with

establishing the subgrouping evidence that would allow comment on such issues (Biggs 1978, Pawley 1966). But these people have simply used careful language when speaking of matters that are presently indeterminate *within certain limits*.

Langdon's fuzziness on the proto language issue does not mean that he has failed to produce interesting evidence about the distribution of plant names in Polynesia. But some of the most interesting distributions he presents are simply in error and in some of those cases when his data are more complete, few linguists could agree with his interpretation of their significance.

The plants and plant names Langdon considers are breadfruit, sugarcane, paper mulberry, coconut, bitter taro, arrowroot and the fehi banana.

Breadfruit

Table 1 contrasts Langdon's (1989:309-311) impression of Polynesian breadfruit terms and their distributions with data from Biggs (1990). As can be seen, where Langdon believed **kulu* and **mei* reflexes were entirely in complementary distribution, they are not and both are reconstructed for Proto Polynesian by Biggs. With his incomplete information Langdon concludes that the distribution:

*suggests that the people of the Marquesas and Mangareva Islands must have obtained their breadfruit from one of the Western Polynesian islands where mei is the usual term for it, and that those in the islands of Tahitic speech got theirs from Samoa, considering that Samoa is the nearest sizeable island where a reflex of *kulu is used. Hawaii, in its turn, apparently got its breadfruit from one of the islands of Tahitic speech., (Langdon 1989:309)*

Langdon (1989:309-311) treats the **kulu* term as if it did not exist in PPN because he did not find it in Tongan. But a good potential cognate *does* exist in Tongan and in his source for Tongan (Churchward 1959: *kulu* 'k. of tree'). The external evidence is quite clear. The **kulu* term is reconstructed back to Proto MalayoPolynesian meaning "breadfruit" and is widely reflected through all subgroups of Polynesian. The **mei* term is found in all the Polynesian subgroups except East Polynesian outside of Marquesic.

Clearly **kulu* and **mei* both existed in PPN, Proto Nuclear Polynesian (PNP) and PEP meaning "breadfruit" or something intimately related to the tree and its fruit. It is not clear what, precisely, the **kulu* and **mei* terms meant in PPN or later interstages. It does not presently appear, for instance, that one meant "the tree" and the other "the fruit" nor is there any indication that **kulu* referred to a particular variety while **mei* referred to another. The external evidence concerning **kulu* and its persistence as a generic term in many PN languages points strongly to it having been the generic term in PPN. Why **mei* replaced it in some languages is not clear. Such uncertainties are a part of life in comparative linguistics.

Table 1: Reflexes of Proto Polynesian *kulu and *mei in selected Polynesian languages (Source: condensed from Biggs (1990) and Langdon (1989)).

PPN	Biggs		Langdon
	*kulu	*mei	-
Tongic:			
Tongan	kulu	mei	mei
Niuean	-	mei	-
Nuclear Polynesian:			
Anutan	-	mei	mei
Kapingamarangi	gulu	-	gulu
Mae	kuro	mei	mei
Rennellese	-	mei	mei
Samoan	'ulu	-	'ulu
Tikopian	-	mei	mei
Eastern Polynesian:			
Hawaiian	'ulu	-	'ulu
Maori	kuru	-	kuru
Marquesan	ku'uvahake	mei	mei
Mangarevan	kuru	mei	mei
Rarotongan	kuru	-	kuru
Tahitian	'uru	-	'uru

External evidence (Biggs 1990): Rotuman 'ulu, Fijian kulu, POC *kuluR, PMP *kulu[r] 'breadfruit', (Bender et al. 1990): PMC *mai 'breadfruit'.

Langdon's (1989:309-311) assertion that Marquesas and Mangareva have borrowed *mei* from Tongan is unnecessary and unmotivated and his implying that PPN had only **mei* and not **kulu* is dismissed on the basis of agreements both internal and external to Polynesian.

Again, in the case of terms for "fermented breadfruit", Langdon failed to realise that both the forms he examined were present in Proto Polynesian and again he lists their distributions (Langdon 1989:310, Figure 2) as being mutually exclusive which they are not. Table 2 shows data from representative languages from Langdon (1989) and Biggs (1990).

Langdon cites forms for the PPN **mara* set in Marquesan and Mangarevan as: *ma* and *maa* where Biggs (1990) has *ma'a* and *mamara*. Biggs has *maa* in both languages for a reflex of a would be PPN **maa*. Biggs (1990) handles this second set of correspondences in Marquesan and Mangarevan by a different reconstruction which has reflexes *only* in

Table 2: Reflexes of Proto Polynesian *mara and *masi in selected Polynesian languages (Source: condensed from Biggs (1990) and Langdon (1989)). Reflexes of Biggs' (1990) questionable PPN *maa reconstruction included.

PPN	Biggs			Langdon
	*mara	*maa	*masi	
Tongic:				
Tongan	maa	maa	mahi	maa
Nuclear Polynesian:				
Anutan		mamaa		maa
Kapingmarangi	mmala		mahi	
Rennellese	maga		masi	masi
Samoaan	mala	masi	masi	
Eastern Polynesian:				
Hawaiian	mala			
Maori	mara		mahimahi	
Marquesan	ma'a	maa	mahi-/komahi	ma
Mangarevan	mamara ³	maa	mahimahi	maa
Rarotongan	mara		ma'i	ma'i
Tahitian	maramara		mahi	mahi

External evidence (Biggs 1990):

PPN *mara 'food fermented to preserve or enhance taste': Rotuman *mata* 'wet', Rotuman I *mara* 'so ripe as to fall off tree', Fijian *madrai* 'fermented breadfruit', Nggela *manda* 'ripe', POC *ma(n)da, Proto Eastern Malayo-Polynesian *mada 'ripe, soft'. (But see Marck (in preparation) for difference in history between PEO *mara 'fermented breadfruit' and *mada 'ripe, soft').

PPN *maa 'fermented food': no external evidence cited.

PPN *masi 'sour, acid, fermented (of vegetable food)', Rotuman *masi* 'salt', Fijian *masimasia* 'wind spoiled breadfruit', POC *masi 'fermented breadfruit', PAN *kam(ng)(c.s)i 'breadfruit, fermented breadfruit', PANI *qasiN 'salt(y)'.

³ Biggs (1990) has maramara in POLLEX and it is undefined in those materials. Maramara is defined as 'firewood' in Tregear (1899) while mamara is defined as "sharp in flavour, acid. Piquant to the taste." Therefore, I have substituted Biggs' maramara with Tregear's mamara in this discussion.

Tongan, Marquesan, Mangarevan and Anutan. Biggs' PPN reconstruction **maa* may be a bookkeeping device for this other distribution rather than demonstrating a conviction on the part of Biggs (1990) that **maa* existed separately of **mara* in PPN. But in a later version of POLLEX, Biggs (1994) continues this reconstruction adding possible cognates from N.Z. Maori and Kapingamarangi (MAO *maa/ii* 'sour, fermented; kinds of fermented food (Wms)', KAP *maa/i* 'ripen, become yellow and soft, of breadfruit only (Lbr)').

This distribution is very curious. Anutan has extensive borrowings from East Uvean and Tongan (cf. Green 1971, Biggs 1980, Ranby 1982, and Feinberg 1989) but the agreement of the other languages with Tongan *maa* is unexpected and the Tongan/Marquesan/Mangarevan distribution is interesting in the sense that Langdon claims: there is reason to wonder if Marquesan and Mangarevan borrowed from Tongan. Tongan regularly loses PPN **r* so PPN **mara* certainly became Tongan *maa*. Langdon did not note that Marquesan and Mangarevan also have regular reflexes of **mara* (*ma'a* and *mamara*) but there are the unexplained irregular doublets (*maa* in both languages). The semantics are somewhat supportive to a loan hypothesis. Reflexes of PPN **masi* in Marquesan and Mangarevan follow the general "sourness" sense of most PN languages. The regular reflexes of **mara* in Marquesan and Mangarevan have to do with rot while the irregular reflexes have to do specifically with preservation of starchy food, as does Tongan *maa*:

TON mahi 'sour, astringent', MQA mahikoha'aha'a 'qui empeste', MQAI komahi 'very ripe, of breadfruit', MGVI mahimahi 'cooked food kept a day to enhance taste', MGVI kokomahi 'a kind of food made from spoiled breadfruit'.

TON *maa* 'plantains or bananas preserved by fermentation', MQA *ma'a* 'rotten', MQA *maa* 'breadfruit preserved by fermentation', MGVI *mamara* 'sharp in flavour; acid', MGVI *maa* 'breadfruit or taro preserved by fermentation'. Biggs (1990)

Whereas Langdon was simply in error on the history of the "breadfruit" terms and no loan hypothesis can be supported through the comparative method, the strength of his loan hypothesis for the "preserved breadfruit" terms was weakened by incomplete development of the available evidence and especially his failure to note the doublets in Marquesan and Mangarevan. The presence of irregular doublets and their phonological and semantic similarity to the Tongan term is striking and suggestive of a loan from Tongan. But then there are the N.Z. Maori *maaii* and Kapingamarangi *maai* forms which reduce the uniqueness of the Marquesan and Mangarevan similarities to Tongan and raise the possibility that a doublet existed at the Proto Nuclear Polynesian level or thereabouts.

At this point I will examine some of Langdon's comments about

languages and how they work. These comments indicate basic differences between tenets of the comparative method, on the one hand, and how Langdon views language prehistory on the other. He states:

Another clue to the prehistory of breadfruit in Eastern Polynesia is that both MQA and MGV reflect a varietal name that is found only in TON and in two other Western Polynesian languages, EUV (the language of Wallis Island) and SAM. The term is puou. It means 'a variety of breadfruit' in all languages except MGV, where it means 'breadfruit flower'. TON, MQA and MGV are the only Polynesian languages that share all three breadfruit terms under discussion [not true, however: J.M.]. It is therefore reasonable to conclude that puou in EUV and SAM was borrowed from TON, as Wallis Island is known to have been heavily influenced from Tonga (Biggs 1980), while SAM is the language of one of Tonga's nearest neighbors. (Langdon 1989:311).

The linguist's obligation is to consider all possibilities, then comment on their relative strengths and weaknesses. Langdon most often considers only one. He says the *maa* form exists only in Tongan, Marquesan and Mangarevan, which is essentially true but for other borrowings from Tongan, and that there is a mutually exclusive distribution of **kulu* and **mei* reflexes, which turns out not to be the case. But neither would be a reason to put forward a theory of origin for a third term with a different distribution. It is just as plausible that Tongan borrowed *puou* from Samoan. It is equally possible that no borrowing occurred at all, i.e., that **puou* existed in PPN and was lost in Outliers and Eastern Polynesian other than Marquesic. Terms for tree varieties are not always stable in Oceanic languages, much less stable than such general terms as "breadfruit", "pandanus" and "coconut" (which also show some replacement). A Western Polynesian loan into Marquesan and Mangarevan is possible but it is only one of a number of potential explanations for the distribution.

Langdon then makes a leap in the interpretation of his distributions, saying:

On the other hand, the puou, maa and mei of MQA and MGV can scarcely be ascribed to borrowing across thousands of kilometres of ocean, and must therefore indicate a genetic relationship with TON. (Langdon 1989:311)

On the basis of these three purported special agreements, Langdon would have us ignore all we know about the shared innovations of MQA, MGV and other Eastern Polynesian languages to Nuclear Polynesian languages in Western Polynesia and the Outliers.

Nuclear Polynesian (of which Marquesan and Mangarevan are a part but Tongan is not) is defined by a number of shared innovations in phonology, pronoun morphology and the like (Pawley 1966, Marck in press). Eastern Polynesian (of which Marquesan and Mangarevan are a part

but Tongan and Samoan are not) is defined by a substantial body of uniquely shared sporadic sound changes, morphological and lexical innovations (Pawley 1966, Green 1966, Marck in preparation). Further evidence for the established Polynesian subgroups is abundant throughout Biggs (1990, 1994) except in the case of "Samoic-Outlier" which I have abandoned elsewhere (Marck in press). Langdon offers no criticism of this kind of evidence. He simply ignores it and proposes an alternate subgroup based on a clouded argument. Borrowing is *precisely* that which is implied by Langdon's partial demonstration of the *maa* situation.

Langdon's section on breadfruit (1989:311) ends with further problems:

But the matter does not simply rest there. Both MQA and MGV have a number of secondary breadfruit terms that also indicate contact with non-Tongan speakers, e.g. MQA ku'uvahake, ku'uvahane 'kinds of breadfruit', MGV kokomahi, kokoma'i 'food made from spoiled breadfruit', kurutara 'breadfruit with roughened skins'.

I do not understand how this indicates contact with anyone or why they would be non-Tongans. How Langdon believes he has established anything in that closing paragraph remains mysterious.

Sugarcane

Langdon (1989:311-313) recognises a potential loan situation where the Proto Eastern Polynesian term for 'sugar cane' appears to be borrowed from Tongan rather than inherited directly from Proto Nuclear Polynesian. Both Biggs (1990) and Langdon (1989:312) recognise mutually exclusive distributions which are given in Table 3.

Biggs reconstructs two forms: PPN **too* and PNP **tolo*. He relates both to POC **topu*. This would assume an irregular loss of the final syllable of POC **topu* and a lengthening of the first vowel in PPN. The vowel lengthening is automatic (PPN morpheme structure rules do not allow short vowels in noun bases (Pawley personal communication)). Then there would be a Nuclear Polynesian innovation adding an **l*. This is my own summary of the suppositions implicit in Biggs' (1990, 1994) organisation of his **too* and **tolo* forms. This seems a bookkeeping device in Biggs' work to separate complementary sets of correspondences. As Biggs (1990, 1994) is work in preparation it is not appropriate to ascribe to him any particular point of view. I simply mention here the logical implications of how the entries currently stand.

Langdon correctly points out that by reconstructing PPN **toro* there is no irregularity at all between Tongic and "Samoic-Outlier" but that Eastern Polynesian languages are then seen to show irregular loss of PPN **r* (PPN **r* normally > TON \emptyset , PNP **l*, PEP **r*). Langdon suggests a Tongan loan into Eastern Polynesia on the basis of this distribution.

Pawley (personal communication) provides supporting evidence for

Table 3: Reflexes of Biggs' Proto Polynesian *too, Proto Nuclear Polynesian *tolo and Langdon's Proto Polynesian *toro in selected Polynesian languages (Source: condensed from Biggs (1990) and Langdon (1989)).

	Biggs		Langdon
	PPN *too	PNP *tolo	
Tongic:			
Niuean	too		too
Tongan	too		too
Nuclear Polynesian:			
Samoan		tolo	tolo
Tokelauan		tolo	tolo
Rennelese		togo	togo
Sikaiana		tolo	tolo
East Futunan		tolo	tolo
Nukuoro		dolo	dolo
Anutan	too		too
East Uvean	too		too
Pukapukan			too
Mae	too		too
Eastern:			
Easter	toa		toa
Hawaiian	koo		koo
Maori	too		too
Marquesan	too		too
Mangarevan	too		too
Rarotongan	too		too
Tahitian	to		too

Langdon's interpretation. Pawley believes that the Polynesian forms do not derive from POC *topu but instead may come from a Proto Central Pacific form *doro or *dolo probably meaning 'stem' or 'stalk'. (Fijian Bauan *tolo* 'trunk (of a body)' and Wayan *dolo* 'stem' as well as Bauan *dorodoro* 'trunk'). There is a problem of whether Proto Central Pacific *l or *r was involved but such problems are not unknown in Polynesian correspondences to Fijian (Marck in preparation). Therefore, the Fijian forms show reasonable phonological and semantic agreements with Langdon's PPN *toro reconstruction.

It is also interesting that some of those "Samoic-Outlier" languages known to be influenced by Tongan (East Uvean, Anutan and Tikopian), one

known to be influenced by Eastern Polynesian (Pukapukan) and a fourth not generally thought to have Tongic or Eastern Polynesian influence (Mae) all show the *too* (Tongic/Eastern Polynesian) rather than *tolo/toro* (non-Eastern Nuclear Polynesian) form.

There are at least two possible explanations for the **too* and **tolo* or **toro* distributions:

1) PPN **toro* regularly became Proto Tongic **too* and PNP **tolo*. In PEP **tolo* irregularly reduced to **too*.

2) PPN **toro* regularly became Proto Tongic **too* and PNP **tolo* but was lost in Pre-Eastern and was borrowed into Proto Eastern from Tongic as **too* after Tongic lost PPN **r*.

Langdon considers only the second possibility:

The fact that reflexes of the Tongic form are universal in Eastern Polynesia demonstrate two things:

- 1) *sugarcane* was carried to the region from a Tongic source, and
- 2) reflexes of PNP **tolo* had been forgotten before any Eastern Polynesians who originated in the non-Tongic islands acquired their first sugarcane.

Langdon (1989:313)

Concerning the question of which Eastern Polynesian region first borrowed Tongic *too*, he says: "it would be reasonable to assume that the Marquesas Islands filled that role because of the evidence already brought forward on breadfruit terms" (Langdon 1989:313). On the present matter we have no linguistic basis to point to the Marquesas and, as we have seen, his evidence on the breadfruit was largely in error.

Neither would we limit the discussion to the possibility of Tongans delivering the species to Eastern Polynesia. It could just as well have been Eastern Polynesians going back to Western Polynesia, acquiring plants and trade goods and then returning to Eastern Polynesia with the names from the Western Polynesian group from which they acquired them.

Langdon (1989:313) then speaks of a Marquesan "counterpart" to Tongan in a purported relationship between Tongan *too ngata hina* and Marquesan *too aniata*. "No parallel exists among the varietal names known in Tahiti ...". There is no reason to believe that Tongan *ngata hina* would have anything to do with Marquesan *aniata*. The correspondences are entirely irregular both in the sense of common inheritance and in the sense of what a loan between the two might look like.

Langdon has shown an interesting pattern in the terms for sugarcane; one that had not been noted previously⁴. His inference that Eastern

⁴ Biggs was making two reconstructions at about the same time and possibly earlier but he has never raised the loan hypothesis as Langdon has.

Polynesian borrowed from Tongan or Tongic is only one of at least two possible interpretations. But it seems to me that his is the most likely as the sporadic loss of PNP *l is otherwise unknown for PEP (Marck in preparation based on Biggs 1992). However, his suggestion that the loan was into Marquesan and his other assertions are unsupportable.

Paper Mulberry

Langdon (1989:313) asserts that:

Names for the paper mulberry tree and the cloth made from it vary considerably in Western Polynesia and in neighboring Fiji and Rotuma. There are more different names in Eastern Polynesia.

He concludes (Langdon 1989:313-314):

The many variants in the west probably reflect the fact that the paper mulberry was not brought into the area until long after it was first settled and the names for it were adopted haphazardly...

His assertion about the diversity of names is erroneous. Two forms are widespread. Proto Polynesian *siapo 'paper-mulberry plant (*Broussonetia* sp.); bark-cloth' is reconstructed on the basis of Tongic, "Samoic-Outlier" and Eastern Polynesian cognates. The common word for the plant in Proto Central Eastern Polynesian appears to have been *aute (< PPN *kaute 'Hibiscus rosa sinensis') (Biggs 1990).

The most significant "fact" he goes on to raise on this matter is simply in error:

evidence for the presence of the paper mulberry in the islands west of Fiji at the time of European contact is almost non-existent.

(Langdon 1989:314 citing Kooijman 1972:445-451)

In fact the distribution of paper mulberry west of Fiji is essentially continuous to New Britain and the coast of New Guinea and Langdon's source (Kooijman 1972:442-456) mentions this quite clearly. Peekel (1984:131-132) provides additional mention of its use in New Britain as a source in bark-cloth manufacture. It is also reported for parts of Micronesia where Oceanic languages are spoken (Kooijman 1972:451-456) and tattoo designs show an affinity to other Oceanic bark cloth designs through an even wider area of Micronesia. Langdon's assertion that the complex is unique to Polynesia and Fiji within Oceania is without foundation.

As Oceanic languages and the paper mulberry have essentially identical distributions through Melanesia, Micronesia and Polynesia, we can reasonably conclude that the plant was established in the Pacific through the agencies of the Proto Oceanic community and the dispersal of some of its speakers into the Pacific.

Langdon claims (1989:314):

The facts suggest a more or less direct migration from Southeast Asia into the central Pacific, perhaps within the past 1,000 to 1,500 years.

His "facts" do *not* suggest a direct migration nor would there be any reason to hang such a time frame upon them if they did. An argument for late introduction is, however, consistent with Langdon's agenda for developing evidence for a post-Oceanic migration directly out of Insular Southeast Asia into Polynesia. He expands upon this in Footnote 3:

*Such a migration could also explain why there are many words in the languages of Western Polynesia that are not found in Melanesian islands to the west. It could explain why the Polynesians are physically different from the Melanesians. It could explain differences in culture and social structure. And in the context of this paper, it could explain why there are two words for breadfruit in Western Polynesia - *mei apparently being the original word and *kulu, which is very similar to PAN reconstructions, being an introduced word. The notion that all, or most of the words in Western Polynesian languages can be traced back to a single proto-language, PPN, also looks vulnerable. (Langdon 1989:314)*

On the following counts he ignores the prevailing evidence and theories to account for it:

1) The notion that there is a problem in relating West Polynesian vocabulary to anything other than PPN (and local innovations) is unmotivated and he gives no reason for it.

2) Differences in Melanesian and Polynesian culture, social structure and physical anthropology have eloquent *in situ* explanations that do not require secondary migrations (cf. Pawley 1981, Pawley and Green 1984). It isn't clear why Langdon goes on as if this literature does not exist. There are also massive resemblances in culture and language that indicate common heritage in the Oceanic cultures of Polynesia, Micronesia and Melanesia.

3) It is not necessary to explain the distribution of reflexes of PPN **kulu* by resorting to a late migration when the **kulu* term obviously continued into PPN out of languages immediately ancestral to PPN and from there out of POC itself.

Langdon (1989:314) then discusses similarities between some Polynesian terms and words from Toradja in central Sulawesi:

*Several terms relating to tapa are almost identical in the Toradja language of central Sulawesi and the languages of Polynesia and neighbouring Rotuma (Kennedy 1934:230). One such term, Toradja fuya 'bark cloth', has likely cognates in ROT uha 'bark cloth' and SAM u'a 'paper mulberry tree'. Two others, Toradja ike 'tapa-beating mallet' and totua 'wooden tapa-beating anvil', are almost exactly replicated in PPN *ike 'tapa mallet' and*

**tutua 'tapa anvil' (Biggs 1979). Such close linguistic parallels could scarcely be expected if Toradja and other Southeast Asian languages had been separated from ROT and the Polynesian language for '3000 years or more' as the orthodox theory on the prehistory of Polynesian (Jennings 1979:1) would have us believe. Langdon (1989:314)*

"Cognates" are words that derive from direct inheritance out of a common proto language. Words that transfer from one language to another after the disintegration of the common ancestor are called "loans". Langdon would have us believe that Polynesian is non-Oceanic or that loans occurred from Toradja into Polynesian at some later stage. It is not clear from his use of linguistic terminology which scenario he is proposing, although his earlier comments require a loan hypothesis. SAM *u'a* and ROT *uha* are not regularly cognate nor would a Polynesian ***uka* or ***u'a* normally be realised as *uha* in Rotuman's complex history of borrowing from Polynesian (see Biggs 1965). Neither are likely cognates nor likely loans from a form identical or similar to Toradja *fuya*. In either case there would be an initial fricative in SAM and ROT and a different correspondence would be expected for the second Toradja consonant.

Relating PPN **ike* (FIJ *ike*) and **tutua* (FIJ (LAU) *ndundua*) to the Toradja forms is certainly correct but would not indicate any special relationship to the Toradja area. Toradja was specifically chosen by Kennedy (1934) in his seminal work on Indonesian and Polynesian *tapa* because it was one of the last areas practising what was formerly a widespread Indonesian, Malaysian and Philippine technology. Presumably cognates once existed all over Insular Southeast Asia and probably still do to some extent. It is a question of the technology falling into disuse and marginal late description, not one of original distribution.

Langdon doesn't believe such close linguistic parallels could be expected after so many years of independent development. But they do. There is hardly any difference between hundreds of Austronesian languages in their words for "face, eye" or "five" or "fish". It is simply a matter of the simplicity of the base and what has happened to the sounds involved and the same would be true of a Proto Oceanic **ike* or **(nd,t)u(nd,t)u*.

Langdon (1989:314) goes on to say that:

As isolated Rotuma is the most northerly island in the Fiji/Western Polynesia region, it could well have been the point of entry for Southeast Asian castaways bearing slips of the paper mulberry tree.

It is difficult to imagine how such castaways would make it all the way to Rotuma with viable paper mulberry slips. It is also difficult to imagine how a few castaways arriving in Rotuma could rescue Polynesians from a Melanesian physical appearance centuries after Eastern Polynesia was settled (as Langdon (1989:314, Footnote 3) would have his Southeast Asian voyagers doing).

I shall dismiss much of the rest of Langdon's discussion on paper mulberry on the grounds that it addresses a non-existent problem: the erroneous belief that paper mulberry does not exist in Melanesia west of Fiji. I believe the most reasonable assertion is that paper mulberry came into the Pacific with the arrival of Austronesian speakers. Langdon (1989:314) is content to have **ike* and **tutua* reconstructed for PPN based on phonologically regular reflexes widely distributed through Eastern and Western Polynesia and Fiji. His migrational gymnastics for getting the plant here and there through Eastern Polynesia after Proto Polynesian times (Langdon 1989:314-317) then become internally inconsistent.

Coconut

Langdon (1989:318) believes that:

An examination of Eastern Polynesian terms for the coconut (Cocos nucifera) provides further support for the conclusion that the Marquesas Islands were settled from both Tonga and the Society Islands.

He notes a basic difference between those languages that reflect PPN **niu* 'coconut, coconut palm' (nearly all PN for which there are data) and certain Eastern Polynesian languages that he would have reflecting an innovative form: **hakari*. He sees evidence for **hakari* as follows: "TUA *hakari*, TAH *ha'ari*, MGV *erehi*, MQA *e'ehi* 'coconut' and RAR *'akari* 'mature coconut'" (Langdon 1989:318).

There are two distinct sets of forms here. The Tuamotu, Tahitian and Rarotongan forms reflect Proto Tahitic **hakari* while Marquesan and Mangarevan reflect Proto Marquesic **erehi*. Langdon wishes to derive the **erehi* forms from Tahitian by borrowing. To account for the differences in the sequence of consonants Langdon would have the first consonant lost and the second and third consonants metathesise in Marquesic after borrowing the term from Tahitic. The differences in the vowels would be due to **a* having raised to *e* in Marquesic.

Biggs (1990) reconstructs PNP **sakali* 'flesh of mature coconut' and gives as Eastern Polynesian reflexes the forms Langdon mentions plus a number of others that retain the PNP meaning. This is not a late borrowing around Central Eastern Polynesian, it is a directly inherited form whose meaning had to do with mature coconuts in PNP and PCE. The agreement of meanings in TAH, TUA, MQA, and MVA may be a PCE feature or the result of dialects influencing each other. In any event, Langdon's failure to properly identify the word's history earlier in Polynesian leaves him with an empty argument.

Langdon's (1989:318-319) conclusions surrounding the matter are a case of unwarranted inference and must be dismissed:

It may be inferred that TAH speakers exerted a powerful influence on MQA

and MGV to be able to oust such an ancient word as *niu* 'coconut' from their vocabularies.

By his own characterisation of the evidence, **niu* has been "ousted" from neither MQA nor MGV. And, as mentioned above, there is no reason to assume a direction for borrowing even if we allow that the Tahitic and Marquesic forms have something to do with each other. As for a single loan indicating a generalised "powerful influence" in any borrowing context, that is simply not the kind of expression a linguist would use to characterise such a situation.

Langdon (1989:319) goes on to note that one of the fifty odd varieties of coconuts in the Marquesas shares an adjectival designation with Tongan: MQA *tokave* and TON *ta'okave* both referring to a coconut variety with small nuts. Langdon notes that only TON and MQA share the term and assumes a loan into MQA from TON. *Varieties* of trees and other cultigens have a very unstable pattern of reference throughout Oceanic even when the generic name is relatively durable. Thus distributions such as those Langdon has described are generally thought to be somewhat indeterminate, coming either from loans or a common history in which varietal nomenclature has been unstable and lost in many related languages. With so many varieties it could just be a coincidence where TON and MQA are the only languages which have a more widely distributed form recorded. Langdon cannot, in this instance, claim that a loan is the only possibility nor can he assume a direction in the case that it was a loan.

Bitter taro

Langdon's (1989:319-320) discussion of bitter taro (*Alocasia macrorrhiza*) centers on an improbable assertion. Although PPN **kape* '*Alocasia macrorrhiza*' is regularly reflected in almost every PN language, he claims that we need to look to Tonga for the origin of the plant in Eastern Polynesian because of his interpretation of words relating to a string figure made from its leaves! Biggs (1990, 1994) makes no reconstruction for the string figure meaning but based on Langdon's data one would reconstruct:

*PPN *lau-kape* 'string doll from bitter taro leaf fibers': TON *loukape*, EAS (hihi) *rau[']ape*, MQA *koukape*, PUK *laukape*. Notes: EAS may be a borrowing from Tahitic, MQA sometimes realises PNP **l* as *k* (Tryon 1987).

Langdon (1989:320) makes two claims on the basis of this distribution: 1) for EAS "the figure and its name have obviously been borrowed from Tahiti" and that 2) "TON, MQA and MGV are the only Polynesian languages that have dual reflexes of PPN **lau* 'leaf'" (and that, therefore, Marquesic has borrowed from TON).

The matter of EAS borrowing from TAH seems possible, even likely. On the second matter, it is true that TON, MQA and MQV have dual reflexes of **lau*, one ending in *-au* and the other in *-ou*. But these are general facts

about **lau* reflexes with nothing at all to do with the particular history of the lexeme **lau-kape*. Tongan, Marquesan and, to some extent, Mangarevan have general "height and backness" assimilations of the low vowel which resemble each other but those in Marquesan and Mangarevan have occurred independently (Marck in preparation). Doublets are not commonly involved in any of these three languages but do occur in cases where there is compounding, the free forms and compounded forms having different pronunciations.

It is extravagant to suggest that we need to account for the distribution of bitter taro (which has regular reflexes of PPN **kape* in nearly every PN language) on the basis of the string figure terms. And upon examination of Langdon's data and arguments concerning the string figure itself we find that his arguments can be dismissed on standard comparative grounds.

Arrowroot

Langdon (1989:320-321) notes the distributions of two terms having to do with arrowroot in a manner essentially consistent with the data from Biggs (1990). Langdon correctly realises that Biggs' PPN **maaso'a* 'Polynesian arrowroot' is reflected only in Western Polynesia and languages which have borrowed from Tongic while Biggs' PPN **pia* 'arrowroot, starch' is reflected in Western Polynesian, the Outliers and Tongic Niue.

As with other words, Langdon does not look to external evidence or misunderstands its significance when he does. In this instance he makes numerous errors in interpretation:

*It will be evident that the Eastern Polynesians could not have obtained their arrowroot or their word pia from anywhere in Western Polynesia where the sole term for it is a reflex of PPN *maaso'a. So it follows that Samoa, Nanumanga and Niue are its only possible sources, on present day evidence. However, as NIU is a Tongic language, its word pia is almost certainly a borrowing from either SAM or some Eastern Polynesian language, while Nanumanga seems an unlikely homeland for any prehistoric settlers of Eastern Polynesia. By a process of elimination, Samoa therefore emerges as the only likely source.*

As it has previously been argued that Samoans were the settlers of the Society Islands, those islands, almost certainly, were the centre of dispersal for arrowroot, or merely its name, throughout Eastern Polynesia.

(Langdon 1989:321).

The three main problems with Langdon's discussion of arrowroot terms are:

1) He does not entirely ignore external evidence but he misinterprets its significance and fails to consider the full range of possible origins for a PPN term.

2) He does not understand that Tongan once had the *pia* word, and

would have Niue *pia* come from a loan, not considering the possibility of direct inheritance from Proto Tongic.

3) He derives Eastern Polynesian terms (and the plant) from Samoa (which trained comparativists would also normally do) but for reasons that are not consistent with application of the comparative method. Also, he specifically has the plant entering Eastern Polynesia by way of Tahiti when linguists would only go so far as to say the term entered Proto Eastern Polynesian regularly out of Proto Nuclear Polynesian and would place the Proto Eastern Polynesian homeland in a linguistically indeterminate locality.

Biggs (1990) reconstructs two forms for PPN: **pia* and **maasoa'a*, with slightly different meanings. The first is thought to have been a more general term in PPN (referring to starch, paste and arrowroot itself in the daughters) while the second referred just to the plant. Biggs relates **pia* to POC **(m)piRa* and PAN **biRaq*, both meaning "Alocasia, Cytosperma" and to FIJ *yabia* which means "arrowroot, starch" as do the cognates in much of Polynesia. The **maasoa'a* form names the plant in most Western Polynesian languages, has no known external cognates and is not known in Eastern Polynesia or most of the Outliers where **pia* reflexes name both the plant and its starch in most languages.

A few paragraphs before those quoted above Langdon wants to relate Polynesian *pia* to FIJ *yabia* 'arrowroot' and "PAN and POC **rumpia* 'sago' where Biggs (1990) recognises the FIJ cognate but relates it to POC and PAN words for taro. Dutton (1994) considers the history of some of these forms and notes the general confusion surrounding some terms for "sago", "arrowroot" and others.

The difference is of no significance to the present discussion. Langdon, Biggs and myself accept external forms as cognate to PPN **pia* and it therefore follows that **pia* once existed in TON (through its descent from PPN) but was lost. Langdon's implying that it never existed in TON is simply illogical and ad hoc. Tongic broke off directly at the PPN level and thus Pre-Tongic had the **pia* form. Langdon does not acknowledge that the NIU form could be directly inherited from Proto Tongic. Eastern Polynesian *pia* could have its origin in prehistoric Tongan or Tongic as a loan. But the less complex possibility is that it was directly inherited from the Samoa area at the time of the other events that brought a language based in ancient Samoan/Ellicean Outlier to Eastern Polynesia.

His naming the Societies as the likely area of introduction (Langdon 1989:321) is based on arguments and "evidence" previously disputed in the present work and has nothing to do with the nature of the arrowroot data.

Fehi banana

Langdon (1989:321-323) begins by pointing out the difference between the banana known to Westerners, *Musa paradisiaca*, and the fehi banana, *Musa fehi*, which has erect clusters of fruit that are inedible unless cooked. He then discusses a number of etymologies but errs frequently in properly

identifying their distributions. Therefore, before turning to Langdon's claims, it is useful to discuss some of these terms and what Biggs' (1990) shows for their distribution and level of reconstruction. Biggs reconstructs PPN *futi 'banana', PPN *soaka 'banana sp.', PNP *maika 'banana sp.', PCE *fekii 'a tree fern' and PPN *fua 'fruit' on the basis of the following abbreviated lists of agreements (in Biggs' notation "<...>" brackets enclose forms not counted as cognate):

PPN *futi 'banana': POC *punti, PMP *pun[t]i['] (Dpf), PAN *pun(tt)i 'banana (Musa)' (Ply. 1973), ANU puti 'banana (Musa sp.) (Yen)', ECE futi, FIJ vudi 'banana', MAE futi 'banana (Cpl)', MAO huti/huti 'a sweet-potato variety (Bgs)', NIU futi 'banana, plantain (generic term) (McE)', PUK wuti 'banana (Bge)', REN huti 'banana or plantain (Ebt)', SAM futi 'banana', TON fusi 'banana'.

PPN *soaka 'banana sp.': PEO *soanga 'banana sp.', FIJ soaqa banana sp. (Musa fehi)', MFA- <soaga 'banana variety B.(Clk)>', SAM soa`a 'indigenous banana (Musa sp.) characterised by an erect bunch of orange-red fruit', TIK soaka 'variety of banana (Fth)'.

PNP *maika 'banana sp.': EAS maika 'banana (Fts) (Mtx)', ECE maika 'banana (Musa sp.) (Rby) (Nks)', HAW mai`a 'banana (Musa sp.) (Pki)', MAO maaika/ika '(Orthoceras strictum) and other plants (Wms)', MAO1 ika/ika '(Orthoceras strictum) a terrestrial orchid (Wms)', MQA meika 'banana (Dln)', MGV meika 'banana (Tgr)', RAR meika 'banana (Sve)', TAH mei`a 'banana, (Musa sapientum) (Mte)', TUA maaika 'banana (Stn)'.

PCE *fekii 'a tree fern': HAW- <he`i 'banana variety also called mai`a polapola (Pki.) B.>', HAW1 hee`ii 'papaya; a variety of sweet-potato (Pki)', HAW2 heii 'a tree fern (Cibotum menziesii) (Bvr)', MAO whekii 'a tree fern sp. (Dicksonia squarrosa)', MQA fekke 'kind of fern called Cabbage Tree by voyagers (Crk)', RAR `eki (Mka) 'a fern tree (Smith 1898:175)', RAR1 <vee`ii 'plantain (Mauke) B. (Mka)>', TAH fee`ii 'banane de montagne (Musa fehi) (Mte)'.

PPN *fua 'fruit; to bear fruit': PMP *bu`ah 'frucht, baumfrucht' (Dpf. 1938)', EFU fua 'fruit, bear fruit', FIJ vua 'fruit', HAW hua 'fruit; meat of crab or mollusc', MAO hua 'fruit, bear fruit; roe of abalone', MFA fua 'fruit, roe, testicle, kidney (Clk)', MQA hua 'to fruit', MGV `ua 'bear fruit', NIU fua 'fruit, bear fruit', PUK wua 'fruit (Clk)', RAR `ua 'fruit', REN hua 'fruit, nut, seed, bulbil, berry, egg (Ebt)', TON fua 'fruit; roe'

For PPN *soaka I would make the semantic reconstruction 'fehi banana'. Milner's (1966) SAM "soa`a n. Indigenous banana (*Musa* sp.) characterized

by an erect bunch of orange-red fruit. A purple dye can be prepared from its sap" clearly refers to *Musa fehi*. Based on the agreement of SAM to FIJ, it seems reasonable to suggest that the PPN form was **soaka* 'M. fehi'. Langdon (1989:322) states that some Outliers agree in their reflexes of **soaka* meaning "M. fehi" but does not mention his sources.

We can also note the resemblance of SAM *fa'i* 'general name given to banana-plants (*Musa* sp.) and their fruit' to the Biggs PCE **fekii* set as Langdon has in his article. But I would also note the lack of any known cognates from the Polynesian Outliers, the semantic and phonological differences between the Samoan and Central Eastern Polynesian forms and the lack of a known cognate from Easter. The lack of an Easter cognate is, perhaps, the least significant observation as it is but a single, poorly described language. The lack of Outlier cognates is more noteworthy and raises the possibility that the resemblance of SAM to the Central Eastern forms is fortuitous. Even within the limited data sets cited above is the possibly fortuitous resemblance of MAO *hutihuti* sweet potato variety' to Western Polynesian *futi* banana'. Possible chance resemblances are known from other Polynesian cognate sets as well.

We can also note that Biggs' PCE **fekii* reconstruction is problematic. As Langdon (1989:322) has observed, some of the modern forms have two long vowels. Additionally, the consonant agreements around Central Eastern Polynesia are not always regular. Altogether, it would appear to be an old word for some kind of starch food, probably a tree fern' as Biggs has suggested, which was borrowed into Rarotongan and Hawai'ian once it had come to be associated with the fehi banana in Tahitian.

With Biggs' data and the above discussion in mind, I will now turn to Langdon's presentation of data and discussion of its significance. As Langdon (1989:322) notes "TAH *fe'i* is remarkably close to SAM *fa'i*." But, as noted above, the correct representation of the TAH is *fee'ii* and when other Eastern Polynesian languages have words that regularly agree, they all mean "tree fern" or other starch food (HAW *hee'ii*, MAO *whekii*, MQA *fekke*). Only HAW *he'i* and RAR *vee'ii* refer to fehi banana and both are phonologically irregular. The HAW is most probably a TAH loan due to the alternate name *mai'a polapola* ('banana of Borabora') and it is known to have been introduced from TAH in the early part of the last century as Langdon (1989:322) notes. The regular RAR agreement with TAH *fee'ii* would be ***'eekii* so the form appears to be a TAH loan there as well.

Langdon (1989:322-323) goes on to note that:

In MQA, the fehi banana is called huetu... They are said to have been

¹ The roots of tree ferns are edible after proper cooking.

introduced from Tahiti in prehistoric times. In MGV, M. fehi is called huatu or 'uatu. In RAR its name is 'uatu.

For *M. paradisiaca*, Langdon (1989:323) notes the lack of continuity in the Western Polynesian terms which are nearly always reflexes of PPN **futi* and those for Eastern Polynesia which reflect a PEP **maika*. But Langdon was apparently unaware of the ECE cognate *maika* 'banana' and the fact that this form can be reconstructed to Proto Ellicean⁶.

Langdon (1989:323) then makes several unwarranted conclusions about the general history of bananas and their naming in Eastern Polynesia. First he claims that "linguistic evidence provides no clue as to whether the Polynesian settlers of the Society Islands brought the fehi banana with them or whether they found bird-propagated specimens". This is not an unreasonable assertion nor is his next claim which states that "this banana could not have come from ... Tonga" as fehi appears to have first appeared there during the historical period.

However, I will quote the rest of his assertions in their entirety as they tend to be quite extravagant:

2. *Whether the Polynesian settlers of the Society Islands brought M. fehi with them or not, they apparently used their generic term fa'i to describe it from then onwards. This indicates:*

(a) *that they were Samoans,*

(b) *that they did not bring M. paradisiaca with them, and*

(c) *that M. paradisiaca — TAH mei'a or mai'a — was not already present in the Society Islands at that time.*

3. *In the course of time, SAM fa'i became TAH fe'i. (Alternatively, fe'i became fa'i in SAM after the migrant Samoans left home).*

4. *M. fehi was eventually taken from the Society Islands to the Marquesas, as tradition states. But TAH fe'i gave place to the name huatuu, from TAH hua 'fruit' and tuu 'to be erect' (cf. REN togaka above). As the MQA term for fruit is puku or pu'u, this is further evidence of TAH linguistic influence in the Marquesas. Later, after the familiar *a to e vowel change, TAH huatuu became the huetu of MQA dictionaries.*

5. *As MQA huatu, M. fehi was carried to Mangareva and from there to h-less Rarotonga where cognate terms were adopted. Langdon (1989:323)*

Taking these claims in order, the first is that the settlers of the Societies were Samoans, that *M. paradisiaca* was not present, that they did not bring it with them and that they did bring *M. fehi*. I have already raised the question of

6. See Wilson (1985). A group composed of Eastern Polynesian, Ellicean Outliers and possibly Samoan. See also Marck (in press).

whether SAM *fa'i* is actually related to PCE **fe(e)kii*. Also, the placement of the earliest Eastern Polynesian community in the Societies is archaeologically and linguistically unmotivated. It would be good to have an answer to the question of where the first Eastern Polynesians were established, but at present we do not know.

Also, the suggested loss of PPN and PNP **futi* is inconsistent with evidence Langdon cites elsewhere (Langdon and Tryon 1983:41) attributing a **futi* reflex to EAS in the eighteenth century. Actually, it seems the evidence on *paradisiaca* as taken from Langdon (1989) and Langdon and Tryon (1983:41) is more consistent with a scenario in which *paradisiaca* was known by both **futi* and **maika* in PEP and then only by **maika* by PCE times. Even if we discount the report of a **futi* reflex in EAS, the *change* in name of a plant or other cultural item is not evidence for failure of the item to be established early in a community.

Langdon has confused the significance of the fehi and *paradisiaca* evidence. A linguist would normally suggest roughly the opposite of what Langdon has suggested: that *paradisiaca* was certainly present by the time of the disintegration of PEP while fehi was not present at all, or that its naming was not stable by PEP times. It is possible that PEP **feekii* is a loan from SAM *fa'i* but the semantics of the EP forms suggest, as noted earlier, that the PEP form had to do with tree ferns or other sources of starch and that only once it was associated with fehi banana in TAH did it begin to be borrowed around Eastern Polynesia. In any case, these would be post-PEP developments, not something that happened in a PEP community located in the Societies as Langdon suggests.

Langdon's third suggestion is not completely deficient. If TAH borrowed *fa'i* or an earlier ***faki* from SAM it *would* sometimes have become *fe'i* in TAH but this does not explain the long vowels of TAH or other EP languages or the tendency of the other EP words which regularly correspond to TAH to be associated with sources of starchy food other than the banana. Langdon's alternate suggestion, that an earlier *fe'i* became *fa'i* in SAM is unmotivated. SAM is not otherwise known to make such vowel changes and it is simply an *ad hoc* suggestion on Langdon's part.

Langdon's fourth set of suggestions also has problems. There is nothing wrong with citing the Marquesan tradition that fehi came from Tahiti. If simple statements of history are involved we must suspect that some or many are true and such memories have a bearing on the topic at hand. But Langdon fails to note that reflexes of PEP **hua* still mean "fruit" or things having to do with fruit in MQA. Thus it is unnecessary to invoke a TAH loan hypothesis for the MQA term even though the item itself is reasonably suspected of coming from Tahiti. More to the point, there is no candidate for a source of a loan from Tahitian. Another term is used there.

Langdon's fifth set of suggestions again fails to make proper use of the linguistic term "cognate". Langdon would have fehi "carried to Mangareva and from there to h-less Rarotonga where cognate terms were developed". "Cognates" are the result of common inheritance and here

On Langdon's (1989) East Polynesian Plant Study

Langdon is talking about a loan situation. To have the Mangarevan term (and plant) introduced from Marquesas is not unreasonable but Langdon has failed to consider the possibility that MGV and RAR independently innovated the term upon borrowing from another source. The adoption of a descriptive term ("standing up") in both places may have occurred independently. Even when there is linguistic similarity, one is not always free to posit common inheritance or borrowing as the only possibilities.

Langdon's (1989) Conclusions

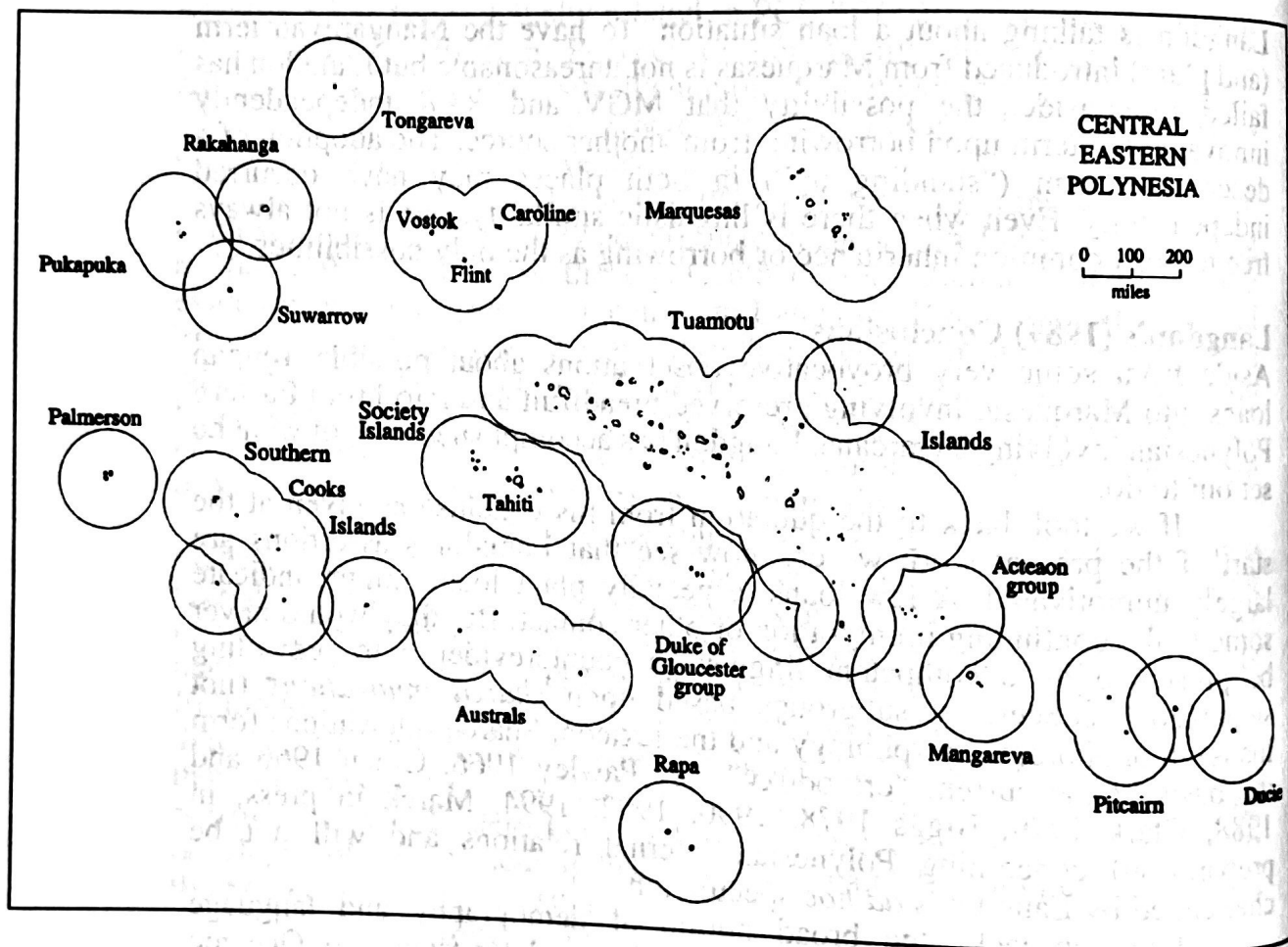
Aside from some very provocative observations about possible Tongan loans into Marquesic involving preserved breadfruit and into Proto Eastern Polynesian involving sugarcane, Langdon has accomplished little of what he set out to do.

If we look back to the quotation from his conclusions given at the start of the present work we can now see that Langdon's assertions are largely unmotivated. A few loans, especially plant loans, might indicate some trade, continuing immigration or other contact. But they would never be presented by a competent linguist as counterevidence to prevailing suggestions concerning subgroups based upon *shared innovations* (not loans) in phonology, morphology and the lexicon. Shared innovations form the basis of the current "orthodoxy" (see Pawley 1966, Green 1966 and 1988, Clark 1979, Biggs 1978, 1990, 1992, 1994, Marck in press, in preparation) concerning Polynesian internal relations and will not be challenged by Langdon's *ad hoc* speculation.

Langdon lacks the broad notion of demography and language community that pervades competent work on Polynesian and Oceanic linguistics in general. Consider, for a moment, Map 1. This is central Eastern Polynesia with radii of one hundred miles extending outward from inhabited islands (and a few uninhabited islands important to certain kinds of voyages). In areas of relatively open ocean, over periods of millennia we know that language level relations tend to break down over distances greater than the hundred mile limit and tend to be maintained (at least at a mutually intelligible dialect level) within areas connected by less than that distance (Marck 1986). But they can diverge very slowly even when well beyond that limit (as seems to be the case with ancient Tongan and Samoan in the first millenium B.C. (Green 1981, Pawley n.d.)) or diverge hardly at all if only slightly beyond that limit (as with the two Marshall Island chains (see Marck 1986:Map 1)).

What this means for central Eastern Polynesia is not entirely clear. The jump from the Societies to the Tuamotus is a voyage of two or three days in traditional craft (see Finney *et al.* 1989) but both present fairly safe targets from the other. The Marquesas are more like a five or six day voyage from the main Tuamotu chain but more like a three or four day voyage from the northernwesternmost Tuamotus.

Generally, prehistorians feel that Tahiti was settled from Marquesas or vice-versa, typically on general geographical and demographic grounds:



a growing population in one would eventually find the other, presumably by way of the Tuamotus. Dialect differentiation would have been slight for many centuries just because languages do not often change rapidly and the process of differentiation could have been slowed by continuing immigration from the founder population, continuing cultural contacts or both. The whole time frame of which Langdon speaks would have been one of mutual intelligibility and easy loan situations. Possibly it was overlaid with continuing cultural and economic contacts with Western Polynesia including importation of plants and animals that were not established in the initial community.

The breakup of Proto Eastern Polynesian seems to have occurred with the settlement of Rapanui but the breakup of Proto Central Eastern Polynesian may have been a much less abrupt event, a process of declining social, cultural and trade contacts. Establishing how and where certain plants came into this web of demographic, social and linguistic relations, as Langdon attempts to do, seems fairly elusive to me.

Langdon's use of the terms "settled" and "migration" lacks a certain vision in this context, especially when he uses individual plant terms to motivate some new "settlement" or "migration". Single canoes or small

On Langdon's (1989) East Polynesian Plant Study

groups of them arrive at settled islands from time to time, carrying ideas or goods. These do not really constitute significant migration or settlement, just the general ebb and flow of people, goods and ideas. We are not free to infer any demographic significance at all, even when we have clear evidence of a linguistic loan. People remember things from past places and they have canoes to try to go out and get them. What would be the prestige that would accrue to the canoe or fleet that successfully went back for plants or animals that were not established in the earliest communities of various islands? We will never know but it seems a necessary element in our modeling. Langdon mentions only the possibility of new waves of Western Polynesians bringing in plants and names for them.

Sometimes Langdon's comments can be pretty meaningless:

Unless it is assumed that Easter Island was settled several times from the west, a 'half-way house' between that island and the rest of Polynesia is an ethnobotanical necessity as well as a necessity on the ground of distance. As has been shown, Easter Island finished up with the paper mulberry tree with a Futunic name, sugarcane with a TON/Marquesic name, bitter taro with a name that could have come from almost anywhere, and the SAM/Tahitic name for arrowroot. Langdon (1989:324)

On logical grounds Rapanui did not need to be settled many times, it only needed some sort of contact which may have involved trade relations or may have simply been its own voyagers returning to the world to the west to bring back plants and animals to a relatively impoverished environment. Few of Langdon's Rapanui loan arguments are very compelling so the question of motivating loans with post-settlement contacts is essentially vacuous.

Langdon (1989:324-325) then discusses American plant species in Rapanui for a few paragraphs before turning to the question of just what, generally, was going on in the transfer of plant species around Eastern Polynesia:

As one voyage seems less than enough to have taken Easter Island its full quota of cultivated plants in prehistoric times, how many voyages would have been needed to stock Hawaii, the Marquesas and the Society Islands? And how many years, or rather centuries, would have been needed for those voyages? The mind boggles at trying to imagine them all. It boggles, too, at trying to reconcile the botanical imperatives with the 35-year-old notion that Eastern Polynesia was settled from some Proto-Eastern Polynesian homeland where everyone was a speaker of Proto-Eastern Polynesian. Quite clearly, Eastern Polynesia's history was much more complex than that. Langdon (1989:325)

In fact the evidence continues to grow (Pawley 1966, Green 1966, Biggs 1990, 1994, Marck in preparation) that there was a largely continuous Proto

Eastern Polynesian speech community sharing subtle innovations of speech. The community may have been large or small. It may have resided on one island or archipelago or it may have resided on more. What it definitely had was social, demographic and transport *mechanisms* for maintaining linguistic continuity through periods of innovation that mark its linguistic daughters as different from Polynesian languages in the west.

The Conclusion Here

Langdon's accomplishments in the field of history can not be held out as license for flawed work in linguistics. His development of linguistic data is, at times, acceptable (cf. Langdon 1982) but he commonly fails to consider explanations for distributions beyond his first impressions and the narrow agendas he develops.

The work reviewed here fails to set out the data as it exists in common sources. It fails to consider the full range of explanations for most data distributions. It fails to properly use common linguistic terminology. And it demonstrates a very idiosyncratic view of language in prehistory that is inconsistent with notions of linguistic, demographic and other cultural behaviour grounded in observation rather than disconnected speculation.

References

- Bellwood, Peter S. 1978. *Man's Conquest of the Pacific*. Auckland: Collins.
- Bender, Byron, Elaine Good, et al. 1990(1984). *Proto Micronesian Reconstructions (computer file named NEWMIC4A dated 29/6/90 based on work done to 1984)*. Honolulu: University of Hawai'i at Manoa Department of Linguistics.
- Biggs, Bruce. 1965. 'Direct and indirect inheritance in Rotuman'. *Lingua* 14:383-445.
- Biggs, Bruce. 1971. 'The languages of Polynesia'. *Current Trends in Linguistics*, Vol. 8, T. A. Sebeok, Ed. The Hague: Mouton.
- Biggs, Bruce. 1972. 'Implications of linguistic subgrouping with special reference to Polynesia'. *Studies in Oceanic Culture History*, R. Green and M. Kelly, Ed. 143-152. Honolulu: Department of Anthropology, Bishop Museum.
- Biggs, Bruce. 1978. 'The history of Polynesian phonology'. *Second International Conference on Austronesian Linguistics: Proceedings, Fascicle 2: Eastern Austronesian*, S. A. Wurm and L. Carrington, Ed. 143-152. Canberra: Pacific Linguistics C-61.
- Biggs, Bruce. 1979. *Proto-Polynesian Word List II*. Working Papers in Anthropology, Archaeology, Linguistics and Maori Studies No. 53. Auckland: University of Auckland, Department of Anthropology.
- Biggs, Bruce. 1980. 'The position of East Uvean and Anutan in the Polynesian language family'. *Te Reo* 23:115-134.
- Biggs, Bruce. 1990, 1992, 1994. *POLLEX: Comparative Polynesian Lexicon (a computer data base)*. Auckland: Department of Anthropology, University of Auckland.
- Churchward, C. Maxwell. 1959. *Tongan Dictionary*. London: Oxford University Press.

On Langdon's (1989) East Polynesian Plant Study

- Clark, Ross. 1979. 'Language'. *The Prehistory of Polynesia*, J. D. Jennings, Ed. 249-27. Canberra: Australian National University.
- Clark, Ross. 1983. 'Review article: R. Langdon and D. Tryon, The Language of Easter Island: Its development and Eastern Polynesian relationships'. *Journal of the Polynesian Society* 92:419-425.
- Dutton, Tom. 1994. 'Sago and related items in early AN vocabulary'. *Austronesian Terminologies: Continuity and change*, A. K. Pawley and M. D. Ross, Ed. 101-125. Canberra: Pacific Linguistics C-127.
- Feinberg, Richard. 1989. 'Possible prehistoric contacts between Tonga and Anuta'. *Journal of the Polynesian Society* 98:303-317.
- Finney, Ben, Paul Frost, et al. 1989. 'Wait for the west wind'. *Journal of the Polynesian Society* 98:261-302.
- Fischer, Steven Roger. 1992. 'Homogeneity in Old Rapanui'. *Oceanic Linguistics* 31:181-190.
- Fischer, Steven Roger. 1994. 'Rapanui's Tu'u ko Iho versus Mangareva's 'Atu Motua'. *The Journal of Pacific History* 29:3-18.
- Green, Roger. 1966. 'Linguistic subgrouping within Polynesia: the implications for prehistoric settlement'. *Journal of the Polynesian Society* 75:6-38.
- Green, Roger C. 1971. 'Anuta's position in the subgrouping of the Polynesian languages'. *Journal of the Polynesian Society* 80:355-370.
- Green, Roger C. 1981. 'Location of the Polynesian homeland: A continuing problem'. *Studies in Pacific Languages and Cultures in Honour of Bruce Biggs*, J. Hollyman and A. Pawley, Ed. 133-158. Auckland: Linguistic Society of New Zealand.
- Green, Roger C. 1988. 'Subgrouping of the Rapanui language of Easter Island in Polynesian and its implications for East Polynesian prehistory'. *First International Congress, Easter Island and East Polynesia, Volume 1: Archaeology*, C. Cristino, P. Vargas, R. Izaurieta and R. Budd, Ed. 37-57. Isle de Pascua (Easter Island): Universidad de Chile, Facultad de Arquitectura y Urbanismo, Instituto de Estudios.
- Howard, Irwin. 1981. 'Proto-Ellicean'. *Studies in Pacific Languages and Cultures in Honour of Bruce Biggs*, J. Hollyman and A. Pawley, Ed. 101-118. Auckland: Linguistic Society of New Zealand.
- Jennings, Jesse D., Ed. 1979. *The Prehistory of Polynesia*. Canberra: Australian National University.
- Kennedy, Raymond. 1934. 'Bark-cloth in Indonesia'. *Journal of the Polynesian Society* 43:229-243.
- Kirch, P.V. 1984. *The Evolution of the Polynesian Chiefdoms*. Cambridge: Cambridge University Press.
- Kirch, Patrick V. 1985. *Feathered Gods and Fishhooks: An introduction to Hawaiian archaeology and prehistory*. Honolulu: B.P. Bishop Museum Press.
- Kirch, Patrick V. 1986. 'Rethinking East Polynesian prehistory'. *Journal of the Polynesian Society* 95:9-40.
- Kooijman, Simon. 1972. *Tapa in Polynesia*. B.P. Bishop Museum Bulletin 234, Honolulu: B.P. Bishop Museum Press.
- Langdon, Robert. 1982. 'New World cotton as a clue to the Polynesian past'. *Oceanic Studies: Essays in honour of Aarne A. Koskinen*, J. Siikala, Ed. Helsinki: Suomen Anthropologinen Seura (The Finnish Anthropological Society).
- Langdon, Robert. 1989. 'The significance of cultivated plant names in the settlement of Eastern Polynesia'. *VICAL 1: Oceanic Languages. Paper from the Fifth*

- International Conference on Austronesian Linguistics*, R. Harlow and R. Hooper, Ed. 305-333. Auckland: Linguistic Society of New Zealand.
- Langdon, Robert and Darrell Tryon. 1983. *The Language of Easter Island: Its Development and Eastern Polynesian Relationships*. Institute for Polynesian Studies, Monograph Series No. 4, Laie: Brigham Young University-Hawaii Campus.
- Marck, Jeff. in preparation. 'Polynesian language and culture history'. PhD thesis. Department of Linguistics, Research School of Pacific and Asian Studies, Australian National University.
- Marck, Jeff. in press. 'A revision to the standard theory of Polynesian subgrouping and its culture history implications'. *Proceedings of the World Archaeology Conference, New Delhi, 1994*, R. Blench and M. Spriggs, Ed. New York: Routledge.
- Marck, J.C. 1986. 'Micronesian dialects and the overnight voyage'. *Journal of the Polynesian Society* 95:253-258.
- Milner, G.B. 1966. *Samoan Dictionary*. London: Oxford University Press.
- Pawley, Andrew. 1966. 'Polynesian languages: a subgrouping based on shared innovations in morphology'. *Journal of the Polynesian Society* 75:39-64.
- Pawley, Andrew. 1981. 'Melanesian diversity and Polynesian homogeneity, a unified explanation for language'. *Studies in Pacific Languages and Cultures in Honour of Bruce Biggs*, J. Hollyman and A. Pawley, Ed. 269-309. Auckland: Linguistic Society of Auckland.
- Pawley, Andrew. n.d. 'On the Polynesian subgroup as a problem for the continuous settlement hypothesis'.
- Pawley, Andrew and Roger C. Green. 1984. 'The Proto-Oceanic language community'. *Journal of Pacific History* 19:123-146.
- Peekel, P.G. 1984. *Flora of the Bismarck Archipelago for Naturalists (translated by E.E. Henty)*. Lae, Papua New Guinea: Office of Forests, Division of Botany.
- Priestly, Tom. 1973. 'Subgrouping in comparative reconstruction'. *Anthropological Linguistics* 15:299-323.
- Pulgram, Ernst. 1961. 'The nature and use of proto-languages'. *Lingua* 10:18-37.
- Ranby, Peter. 1982. 'The dual reflexes of Proto-Polynesian *s in Anuta'. *Te Reo* 25:3-11.
- Tregear, Edward. 1899. *A Dictionary of Mangareva*. Wellington: John MacKay, Government Printing Office.
- Tryon, D.T. 1987. 'Le cas du /k/ en Marquisien'. *Bulletin de la Societe des Etudes Oceaniennes* 241:31-37.
- Wilson, William H. 1985. 'Evidence for an Outlier source for the Proto Eastern Polynesian pronominal system'. *Oceanic Linguistics* 24:85-133.