

5 *Proto Melanesian plant names reconsidered*

ANN CHOWNING

1 Introduction

In 1961 I was invited to contribute a paper on Melanesian plant names to a symposium, entitled 'Plants and the migrations of Pacific peoples', to be held at the Tenth Pacific Science Congress. Two years later, under the editorship of the convenor, Jacques Barrau, that paper was published, together with the others given at the symposium, as Chowning (1963). At the time I wrote, I had done fieldwork in two Austronesian-speaking societies in what is now Papua New Guinea: Lakalai (West Nakanai) in New Britain and Molima on Fergusson Island in the D'Entrecasteaux. In Lakalai I was a member of a team led by Ward Goodenough, who not only taught me to recognise many of the common cultivated plants but also had invited an ethnobotanist from the Department of Forests to join us. The ethnobotanist's report (Floyd 1954) was produced after I left the field, but was the source of the botanical identifications I used in my paper. For the comparative data, I relied on word lists from Lakalai and Molima together with the published vocabularies of Melanesian languages that I owned or could find in the library of Columbia University. Because French-Wright (1983:163) was under the impression that my research was "based on New Britain", I should note that Lakalai was the only New Britain language that I used. I have misplaced my original notes, and so I cannot list all the languages of my original sources. They include at least one that I have not been able to find here, judging from the fact that I cannot locate the required third witness (apart from Lakalai and Bauan) that led me to reconstruct **lautolu*¹ ('three-leaf') *Evodia* sp.'

Because my present procedure differs somewhat from that used in 1961, the latter is worth repeating. First, I required three witnesses for each reconstruction, from languages that were widely separated in space and that did not seem to show other signs of close ties. I excluded data from Polynesia and Micronesia, as well as from Indonesia, although I consulted Indonesian material in considering my conclusions. Because all my material came from within Melanesia, it is misleading to say as Tryon (1994:481) does that my Proto

¹ Despite a misleading statement by Blust (1983–84:123), who says that I cited reflexes for a protoform he is discussing, in fact I did not list in the 1963 article any of the data on which I based my reconstructions.

Melanesian “was presumably intended to cover the same languages as the Oceanic subgroup”. In retrospect, I think that my only error was to assume a particularly close relationship between Molima and certain languages in the south-east Solomons; I cannot now imagine why I thought that they could not be treated as separate witnesses.

My use of ‘Melanesian’ to designate a presumed linguistic grouping of Austronesian languages spoken within Melanesia now looks very odd, but it was common at the time. See particularly Capell (1962), on which I relied for deciding which languages were Austronesian.

It is probably safe to say that although the article has been cited frequently, particularly as regards general points that I made, the actual linguistic reconstructions have been almost totally ignored. Milke (1968) mentioned several of my protoforms that fitted in with his reconstructions. Otherwise, as far as I know, the only linguists who have actually made use of my protoforms are French-Wright (1983) in his unpublished MA thesis, and Tom Dutton (1977), but both were concerned only with food plants. (Presumably the situation would have been different if I had been able to locate my data when George Grace asked me for them, in which case they might have gone into his various collections of Proto Oceanic (POc) reconstructions.) A noteworthy example of neglect of my reconstructions applies to the word for putty-nut, *Parinarium*, which I then reconstructed as **iita*. In 1971 Pawley and K. Green actually quoted what I said about the stability of the term for this tree. But in Pawley and Pawley (1994:343), the discussion of a possible term for ‘caulk’ and putty-nut mentions only a term with a much more limited distribution.²

In the years that followed, I have carried out additional fieldwork in two other societies in New Britain, Sengseng and Kove, as well as making return trips to Lakalai and Molima. In Lakalai I spent considerable time checking Floyd’s data, including his more unlikely spellings. I have also, in the course of these trips, gained much more experience about the use of plants that I had in 1961. At the same time I must emphasise that I have no special knowledge of botany. Partly because of my extreme sensitivity to contact with poisonous plants, which abound in Melanesia, I have never collected specimens myself. For identification I have had to rely on a variety of published sources, such as Massal and Barrau (1956).

I envy those colleagues who collect local terms for plants that I would not recognise if I bumped into them. This said, I have kept up an interest in the question of Proto Oceanic plant names, and welcomed an opportunity to re-examine the material, especially now that so many more data are available. I should add, however, that I have not been able to consult the Peekel volume recommended by Ross (1996:163). Of course I also lack access to the vast files held at The Australian National University and by Blust. In my favour, perhaps, is both ethnographic experience – which has made me wary about certain generalisations, such as about medical uses of particular plants, and a speaking knowledge of Lakalai and Molima – which enables me sometimes to correct mistakes and oversights made by others who have

² Using the evidence available to me later, including *atita* in Kove and all the coastal languages extending west to Kilege (Goulden 1996) and Banoni *darsita* (Grace-Lincoln 1979), I would have suggested the addition of an optional first syllable even before Blust (1984:199) published POc **kantita* and derived Mussau *arita* from it, but I note that *tita* alone is recorded for Lakalai, Tolai, Mono, and Gela. For his south-east Solomon languages, Lichtenberk (1988:59) reconstructed a protoform with medial *q*, but tells me that he did not do so only to account for a glottal stop that Ivens wrote inconsistently in Sa’a. It is probably safe to assume that Kwara’ae *saia* also reflects **tita*. (I cannot, however, account for the first part of Bauan *makita* – Capell 1957:155 – if it is cognate.)

consulted typed vocabularies. Examples of errors that I detected only because I speak the language are to be found in two of the terms cited for Nakanai by Ross (1996). One of these, *galagala*, is used to support the reconstruction of a term for 'taro leaves'. But this is a general word for leaves; by itself, it has no specific reference to taro. Taro greens are called *ilili* or, more rarely, *pehe*.

Another mistake seems to represent someone's copying error. All the plant names in Johnston's Nakanai dictionary, Ross's source, seem to come from a typescript of Lakalai I gave Johnston. The term *oio*, used by Ross as one of only two witnesses to reconstruct a term for *Gnetum gnemon*, was typed just above the actual plant name, *ola* (which does not support the reconstruction).

I had assumed that an oversight accounted for the failure of Geraghty and Ross, both of whom have used my Lakalai data, to cite Lakalai *ka-liva* '*Alocasia macrorrhiza*' in support of the protoforms they list. The oversight could easily be explained by the presence of the *ka*-prefix (see Ross 1996:170) and metathesis in the following syllables. Ross, however, tells me (pers. comm.) that he and his team simply considered the Lakalai term "too questionable". I disagree, and still think I was right to use it for reconstructing **vila* (now **piRaq*) as the name for the plant.

2 Wild plants

When I wrote my earlier paper, I paid as much attention to wild plants as to cultivated ones, insofar as my data allowed. In recent years it seems that those dealing with the reconstruction of plant names in western Oceania have concentrated on cultivated plants to the neglect of wild ones. Wild plants abound among the reconstructions proposed for Indonesia, as by Blust, and for eastern Oceania, as by Clark, Geraghty, and Biggs, so the neglect is not general, not is it total (see, for example, a number of the plants mentioned in Ross 1996). But for reconstruction of POc or Lapita culture, the probable importance of wild plants seems to me to have been neglected.

Ross tells me (pers. comm.) that further work on names of wild plants has only been delayed, and will be carried out.

An emphasis on cultivated plants appeared as early as Pawley and R. Green's (1973:29) list of "lexical reconstructions connected with agriculture". The reader is not told why all the trees on this list, which include *Terminalia*, casuarina, and *Barringtonia*, are thought to be cultivated. The assumption that they were, however, has continued, particularly in the recent work of Kirch. He states flatly that fruit and nut trees would have been cultivated by Lapita people in "permanent 'orchard gardens', very likely in close proximity to settlements", and adds that: "Throughout island Melanesia today, this kind of village aborigiculture is highly typical, and has probably been practiced since Lapita times" (Kirch 1997:208). His assumption reflects his experience of seeing such arboriculture in two places with acute land shortages, Tikopia and islets off Mussau, but I would dispute the assumption that they are typical. Indeed, I have never seen an orchard garden, though reportedly one once existed on a Kove island before it was cut down so that coconuts could be planted for copra.

There are three points to be considered: typical use of wild foods, whether in daily life or in periods of crop failure, the definition of 'famine foods'; and the use made of the bush for purposes other than foods. To deal with the third one first, it strikes me as wholly unlikely that Lapita people would have resorted to the bush only for such materials as "lianas and

rattan, bamboo, and other materials essential for manufacturing fishing nets, baskets, and other artifacts” (Kirch 1997:203). I would argue that the forest was equally necessary for the materials needed for houses, canoes, weapons, perhaps clothing, and a wide range of other artifacts. See such surveys as Powell’s (1976, 1982).

As regards foods, anyone who has observed them in a Melanesian society has been struck by the importance of wild foods in the diet. Powell (1982:210) points out how many fruit and nut trees grow wild in lowland areas. It is true that after European contact the importance of some of these diminished, for three reasons. First, starch staples were introduced that were more often resistant to drought, disease, and other hazards than traditional staples; second, many new fruits, such as papayas and pineapple, were also introduced (and cultivated); and third, the government inspired or compelled planting of coconuts for the production of copra. But earlier observers agree that very few trees were to be found in settlements or even gardens (see, for example, Guppy 1887; Hees 1915–16; Panoff 1972). At most, people cleared around the trunks of trees that grew wild – a very mild form of ‘arboriculture’. Furthermore, they often sought out a wide variety of wild foods to supplement those they raised. The Molima do so to this day as regards greens, and the Sengseng, by my reckoning, got about half their calories from wild foods. What some people regard as famine foods (e.g. wild yams or sago), others regard as valued parts of their daily diet.

I could expand on this point, but what interests me here is asserting that the early speakers of Oceanic languages, like their ancestors to the west (see Zorc 1994), had every reason to enter and explore the forests and to become familiar with what it held. Not only should we grant that many of the trees and other plants on which they relied were growing wild, but we should be alert to the probability that, within the limits afforded by local conditions, they continued to seek out and recognise a variety of old friends as they travelled east. In view of all that has been done in recent years, especially by Ross, in reconstructing names for food plants, it was hardly likely that I could have added to their number, but I think it is significant that the few new reconstructions I propose below all refer to other kinds of wild plants. Since I more or less stumbled on them, I suggest that more thorough investigation – including simply looking for cognates in conservative languages of plants well-identified for others – might be very valuable in increasing our knowledge of the lives and interests of speakers of POc. Without denying their interest in “utilitarian” plants (Tryon 1994:507), I suspect that a variety of other interests could also be reconstructed if we search farther. See, for example, Blust (1983–84:87) on a variety of ficus as the haunt of ghosts, or the terms reconstructed in the west for certain flowering plants. As the example of **nununuk*, **laji/*lasi* (see below), and **salato* show, we can also acknowledge that some plants were named so people would know to avoid them. (I am ignoring the fact that the Lakalai actually used the bark of *Laportea gigas* for building houses.)

3 Present approach

First, since no one now disputes that Austronesian languages entered Melanesia from the west, I do not have to exclude Indonesian material from my data base. In fact, I have specifically searched for cognates in Melanesia of terms previously reconstructed only for Indonesia. (See discussion of the term for nipa palm). I have also used data from Polynesian languages, and am grateful to Ross Clark for supplying me with data from POLLEX on Proto Polynesian, as well as with his reconstructions for Proto North Central Vanuatu (PNCV). (I

have neglected Micronesia simply because I find it more difficult to recognise cognates in Micronesian languages.) In general, I have followed the same guidelines as Pawley (1996:134) in which a term can be assigned to POc if it is found outside Oceania as well as within it, or if it is found in “two or more of...five subgroups or collections of languages”, which are so widely dispersed that borrowing is presumably ruled out. Except where I have corrections to make, I have made no attempt to duplicate the many reconstructions proposed by others in the past few decades. Neither, apart from **tita*, have I bothered to correct or defend my own original list of reconstructions. That I was basically on the right path can be seen by the close resemblances between the protoforms that I postulated and those postulated by others who were usually working from a much better collection of material, as well as having a more sophisticated knowledge of the presumed phonology of POc. (See the forms from Chowning 1963 cited in Tryon 1994, while noting that he omitted two of mine – for kapok and *Laportea gigas* – both of which are similar to the forms he does cite.) Instead, I have confined my attention to postulating a few new terms, extending or modifying others, disputing a few, and trying to understand the implications of the information we now have.

One point needs to be stressed. It seems that I am more conservative than some of my colleagues, particularly Ross, in deciding first that two terms are related, and second that a particular identification can be assigned to them. Four examples will suffice – three from Ross, one from my own earlier musings.

In 1973 I cited a Kove counting classifier *saku*, used for pairs of mats, bundles of thatch, and sections of sago (Chowning 1973:217). This struck me at the time as being interestingly similar to sago terms reconstructed by Dempwolff, and it now seems much more persuasive, as possibly indicating cognacy, than the terms he and Dutton cite, including Muyuw *sag* ‘pandanus used for sleeping on’ (Ross 1996:188) and Jabem *saku* ‘spoon for stirring sago or taro puree’ (Dutton 1994:112). But I would still not cite these as supporting evidence for the word for ‘sago’, any more than I would use Lakalai *talo* ‘to mash cooked taro’ as evidence for a reflex of POc **talo(s)*.

A second example is Ross’s reconstruction of POc **m^wamo* as ‘famine foods; wild taro’. I confess that it never struck me that Kove *momo* ‘sago’ (one of Ross’s witnesses) and Sengseng *e-mom* ‘*Dioscorea alata*’ were related; I expect such accidental resemblances to crop up in languages with limited numbers of phonemes. But what bothers me about his reconstruction is that in no case does the supposed cognate form refer to taro, wild or not, and wherever yams are involved they are, as in Sengseng, cultivated varieties. See also Maenge *momo* ‘*Dioscorea alata*’. Note that both the Maenge and the Sengseng eat wild yams, the latter very often, but do not call them by *momo*-like terms. Furthermore, at least in Kove, sago is not a famine food but a major part of everyday diet, prized as highly as taro; they were most indignant at my dislike for it. The reflexes cited do not seem to me to support the way in which the protoform is defined.

The third case concerns me more because it has been so widely accepted, as in Kirch’s (1997:207) list of POc food plants: **(wv)ele* ‘cut nut, *Barringtonia* sp.’. Two of the witnesses are from adjacent languages in West New Britain, Lakalai (Ross’s Nakanai) and Mangseng. In the former the term (*uele*) refers to *Canarium*; in the latter *vere* is simply defined as ‘a tree’. Yet in Lakalai there exists a term *uele te vere* for a different species of *Canarium*. In addition, I pointed out in (Chowning 1996:46) that the Sengseng term for *Canarium*, *e-vel*, is cognate with the Lakalai term. In short, the evidence suggests that the New Britain terms all referred to a tree that was not *Barringtonia*, in contrast to the Solomon terms, and for those the data in Tryon and Hackman (1983:207-210) indicate that a number of languages have a

rather than *e* in both syllables. If the Solomons and New Britain terms are related, the original definition is unclear; if they are not, the shape and distribution of the protoform should be reconsidered.

The example from my own investigations was unpublished because, as noted above, I thought Molima was closely related to some Solomons languages and excluded material from Polynesia. For this reason I did not reconstruct a second term for *Inocarpus edulis* to include Sa'a *mapwe*, Tahitian *mape*, which actually designates a *Terminalia* sp. I have now changed my mind. Because the Eastern Oceanic (EOc) witnesses are so consistent as regards both the botanical identification and the final vowel, I would now exclude from the cognate list both the Molima term and Proto Polynesian (PPn) *mapa*, which designates a *Diospyros* sp., and reconstruct Proto Eastern Oceanic (PEOc) **m(w)ap(w)e*, or something similar, for *Inocarpus*. The witnesses include, along with those cited by Ivens (1918), PNCV **mwabwe* (Clark 1994). If the Molima term either ended in *-e* or designated a Tahitian chestnut, I would admit it and assign the term, perhaps in amended form, to POc. (See also Bwaidoga *mafa* 'a species of tree with nuts like almonds' – Jenness and Ballantyne 1928–29). As it is, I prefer to be cautious.

4 Identifications

In 1963 I reconstructed only terms to which I could, I thought, assign a secure botanical identification. This requirement excluded much of the material available both in published sources and in my own Molima data. I should note at once that except for Lakalai, my own vocabularies are full of the unsatisfactory 'definitions' that frustrate me when reading other ones: 'a tree with edible yellow fruit', for example. Sometimes I do have additional data in field notes that enable me to make an identification (as with Molima and Kove *natu* for *Burckella*) once I can consult other materials, but what I have avoided is reconstructing a protoform if I can only define it vaguely. For example, I am not sure that Kove and Tolai *bama* 'vine with edible root' are the same because the remainder of the descriptions differ so much. The Kove state that the vine contains a potable liquid, whereas the Tolai say that the bark is used in making fishnets. This latter description suggests *Pueraria* (see Tryon 1994:508), but I have no good reason to think that the Kove plant is the same, and so I have not added it to my collection.

On the other hand, in recent years, and purely because of work done by others, I have become persuaded of the usefulness of defining some terms so vaguely that no botanical identification can be given, or stressing that alternatives exist. In the former category I would put Blust's definition of **laji* (Ross's POc **lasi*) as 'tree with poisonous sap', followed by a query as whether it is *Antiaris toxicaria*, more satisfactory than the clear botanical assignment in Tryon. My reason is that the label applies to different trees with poisonous sap, notably *Semecarpus* in several cases. Despite its first syllable for which I cannot account (but see PNCV **walasi* '*Semecarpus*' – Clark 1994) I would add Molima *wenasi* '*Semecarpus*' to the examples cited by Tryon. Another cognate, Lakalai *vorasi/vorarasi*, was identified by Floyd as possibly *Exoecaria*. I suspect that the Lakalai form was affected by confusion with the verb *rasi* 'to damage' and *vovo* 'skin' (often reduced to a monosyllable in compounds).

A more interesting case of apparent false precision is that of POc **ntoRa*, defined by Blust as 'hardwood' (and see Wolff 1994:540) but in Tryon, following Geraghty and Clark, simply as *Intsia bijuga*. The distribution of meanings attributed to the term within Oceanic

makes it clear that this is indeed the correct definition for the west, but not in Polynesia. Geraghty reconstructs Proto Central Polynesian (PCPn) **ya(R)u*, which extends as far as Eastern Fijian (compare POc **yaRu*), for ‘casuarina’, but reflexes of **ntoRa* designate casuarina in Rotuman and Polynesia (Biggs 1965:408), and a new word, PCPn **vesi*, is reflected in the Fijian and PPn terms for *Intsia*. Note that in English both trees are called ‘ironwood’; the hardness continues to be a salient characteristic. However surprising this shift may be, a full definition should include the fact that two different trees are called by the same term (see **viRu* below).

Another case in which I am dubious about the full identification is that of POc **pijo* as an edible *Saccharum* sp. (Ross 1996:218). Too many of the witnesses are not identified as *Saccharum*. Those listed in Tryon do seem to support such an identification, and undoubtedly I am influenced by the fact that the Lakalai cognate *viro* refers to *Phragmites*, but I would still prefer to see the *Saccharum* identification listed with a query, though Ross (pers. comm.) disagrees.

Another identification that made me wonder is Ross’s of POc **m^wwa(r,R)e* as ‘croton, *Codiaeum variegatum*’. (I concur, by the way, with the new formal reconstruction, which is required by Kove *mohe* and is better than my original **male*.) Of course I agree with Ross that “cognate sets span both” croton and cordyline varieties; that was why in 1963 I assigned names for these, along with cycads, to a “croton group” (Chowning 1963:41). But when reflexes of the protoform designate cordyline from Lakalai through Kove to Bariai (Kabana), I cannot be happy about the apparent certainty of Ross’s present definition. In Ross’s list, ‘Nakanai’ *male* is incorrectly said to mean *Codiaeum*.

For some of us, the possibilities of identification have been limited because (as far as I know) few comprehensive collections have been made in places where Oceanic languages are spoken. Within Melanesia, those who have collected, like Panoff, have typically published only a fraction of their material. I wholeheartedly agree with Tryon (1994:481) about the need for “collaborative research by botanists and linguists”, though I would like to add ethnographers to the mix. But it is perhaps also worth noting that the most useful data would come from languages that are relatively conservative (like, for example, Kove rather than Sengse). I was disappointed to find, for example, that the Roviana dictionary, which contains many botanical identifications I have no reason to distrust, also displays very few that have cognates in other Oceanic languages. Without many more very thorough studies, we have to rely far too often on guessing that two plants that look or sound similar are really the same thing. With luck, we may obtain names in some other languages, but my experiences with Tok Pisin in West New Britain have made me wary about how helpful these may be.

5 Problems with Tok Pisin

These are of three kinds. The first, and sometimes most difficult to detect, is that one may easily record a Tok Pisin (TP) term mistaking it for one in the local vernacular. To cite a possible example of my own, the Sengse told me that a word of theirs, *meles*, was *malas* in TP. This was not in either of my dictionaries, and it took me a long search to find its meaning, noted in an article on forestry in the *Encyclopaedia of Papua and New Guinea* (Angus 1972:457). The tree is *Homalium foetidum*. Only later did I discover the term in our Lakalai vocabulary, with that meaning (written *malasi*, but pronounced *malas*). Usually

Floyd noted that particular terms he recorded came from TP, but not in this case. The only evidence suggesting that the term is indeed Lakalai is the existence of a binomial *malasi-ililitili*, designating a different variety.

I feel equally uncertain about Lakalai *taliga* 'k.o. fungus'. When I commented on French-Wright's thesis, I had forgotten that this might well derive from TP *taliŋa* and suggested that the comparative evidence, including that from Sa'a, justified attributing **taliŋa* 'k.o. fungus' to POC. I was aware that in many languages the word for 'ear' is the same as the word for 'fungus' even when the former is not derived from **taliŋa*, as is true with Lakalai *gavusa* (designating a different fungus from the one called *taliga*, and see also PNCV **bwero*). As regards the POC reconstruction, I was relieved to see that it was recently proposed by Blust (1983–84:130–131), but I still feel uneasy about the attributing the *taliga* form to Lakalai rather than to TP.

As these examples suggest, a consistent problem for fieldworkers is that informants, especially the younger ones, are so likely to volunteer TP terms. I suspect that this explains Ross's (1996:188) attribution of *saksak* for 'sago' to Bola-Harua. Not only is it phonologically unlikely, but it is contradicted by the terms given in the 1980 Summer Institute of Linguistics' list (Johnston 1980:144).

A different problem that, as far as I know, has not been generally recognised is that of misleading or frankly incorrect definitions in the dictionaries, those of Murphy (1954) and Mihalic (1957) in particular. Mihalic defines TP *galip* as 'Tahitian chestnut', which is actually the English term for TP *aila* '*Inocarpus edulis*'. *Galip* is the term for *Canarium* almond. A considerable number of linguists and anthropologists have repeated this error. Both dictionaries also state that *pitpit* (or *pit*) refers to wild species of *Saccharum*, whereas *Saccharum edule*, called by this term, is always cultivated. Even those who have seen it in the gardens keep repeating that it is "wild" (and see Ross 1996:217).

Mihalic gives *limbum* as the term for 'areca palm', *wail limbum* for *Caryota*, and *wail saksak* for nipa palm. In West New Britain, I have never heard the first of these (the tree is called by the same name as the fruit), *limbum* is used particularly for *Archontophoenix*, *morota* (otherwise the word for 'thatch') is used for nipa, and the Sengseng at least call *Caryota* '*wail saksak*'. I could multiply examples of the problems caused by relying too heavily on the identifications in the dictionaries, particularly if only one is consulted. (In my experience, Murphy is better for New Britain TP, but by no means free of error.) Suffice it to say that some forms elicited by asking for the vernacular equivalent of a TP term such as *limbum* may not mean what the linguist expects.

For me, a more serious problem has been the existence of many TP terms that are not in my dictionaries. I mentioned *malas* earlier. In Sengseng it took me a long time to discover that *solomon* was the TP term for *Pangium edule*; both dictionaries define it only as 'ceremonial rattles', Womersley (1972:231), writing about the plant without giving its TP name, first describes how the poison is removed and then adds that the "seed coats...are extensively used for dance rattles". I am confident that I identified the plant correctly, but not with the aid of dictionaries. Other terms that I have eventually tracked down or figured out are *bitum* (*Vitex cofassus*), *botol* (*Endospermum foetidum*), *piduk* (*Melanolepis moluccana*), *valagur* (*Polyscias* sp.?) and *lapuat* (*Parartocarpus*). This last resembles Ross's recent reconstruction of Proto Western Oceanic (PWOC) **lapuka* for the same tree. The vernacular names in Lakalai, Sengseng and Kove do not resemble *lapuat* or *lapuka* (see Chowning 1996:46–47). (I owe some of these identifications to Floyd.) There are others that

I have not been able to identify, such as *gulai* (for Kove *asi*) and *mala, baira* for a tree with red wood (Kove *mahara*).

The Kove told me that the tree they use for commercial carving is called both *karasin* (as it is in the Solomons) and *kanau*. I traced its identification through the Solomons term, but noted with interest that *kanau*, for *Cordia subcordata*, is also the Tolai term. Clearly *kanua* should be attributed to POc (see Wolff 1994:524).

6 New and expanded protoforms

POc **(q)asa* ‘*Lygodium* sp., climbing fern’

Lakalai	<i>hara</i>	‘ <i>Lygodium circinnatum</i> , plant used for wrapping of coiled baskets’
Kwara’ae	<i>sata</i>	‘ <i>Lygodium microphyllum</i> ’
Tolo	<i>asa</i>	‘a type of vine used to bind canoes and weave baskets’

Lichtenberk (1988:58) records a number of cognates in Solomons languages, and in Lau the reference is specifically to a climbing fern. Both Lakalai and Cristobal-Malaitan languages have developed a prothetic consonant before POc **a-*, and in both the medial consonant reflects more than one POc consonant, so that my reconstruction may need modification once more cognates are discovered.

POc **iguRa* ‘*Ficus* sp., k.o. sandpaper fig’, leaves used for polishing wood

Lakalai	<i>igura</i>	‘ <i>Ficus</i> sp. with sandpapery leaves’
Motu	<i>igulara</i>	‘ <i>ficus</i> sp.’ (Lister-Turner & Clark 1954. The final syllable is unexplained; Ross (pers. comm.) suggests that it is a loan from a Collingwood Bay language.)
Kwaio	<i>igula</i>	‘a tree, leaves of which are used to polish wood’
Gela	<i>ngula</i>	‘tree with rough leaves’; <i>ingula</i> ‘its leaves for smoothing’

Paul Geraghty first pointed to the resemblance between the Lakalai and Gela terms when he was reconstructing a PCPn word **quRa* ‘rub, scrub’, which he thought was reflected in both languages. But in Lakalai **q* is always reflected as *h*; if a verb reflects **quRa*, it would be *hura* ‘to clean a coconut shell for use as a water bottle’. Not surprisingly, Geraghty was misled by the Gela forms. See also Bwaidoga *aikula* ‘banyan’ (Jenness & Ballentyne 1928–29) and perhaps Molima *aigula* ‘k.o. tree’, though it should be noted that the Bwaidoga use the leaves of a different tree, called *fanavivi*, as sandpaper (Jenness & Ballentyne 1920:184).

POc **kara* ‘irritating plant, perhaps variety of *Laportea*’

Kove	<i>gala</i>	‘a stinging plant related to <i>lato</i> ? (<i>Laportea gigas</i>)’
Tolai	<i>kara</i>	‘sp. of small stinging nettle, <i>Urtica enderalis</i> ’
PNCV	<i>*kara</i>	‘stinging plant’

I had been struck by the resemblance between the Kove and PNCV terms, and in looking for further cognates found the Tolai example. Because I would expect **k* to be reflected in Kove as *k*, I suspect that *gala*, though still cognate, is a loan from another West New Britain language.

POc **nipa* 'Nipa frutescans'

PAn	<i>*nipaq</i>	(Wolff 1994:532-533)
Kwara'ae	<i>niva</i>	'Nipa frutescans'
Gela	<i>niva</i>	'a species of sago palm smaller than <i>sao</i> '

As I noted earlier, I found this correspondence by deliberately looking for cognates of terms that are widespread in languages spoken to the west of Melanesia.

POc **piRu* 'umbrella/fan plam, *Licuala* in the west, *Pritchardia* in the east'

Kove	<i>pilu</i>	' <i>Licuala</i> '
Lakalai	<i>viluvilu</i>	' <i>Licuala</i> sp.'
Gela	<i>vilu</i>	'palm with umbrella-like leaves'
Kwara'ae	(<i>fai</i>) <i>filu</i>	' <i>Licuala</i> '
PEOc/PCPn	<i>*viRu</i>	' <i>Pritchardia pacifica</i> '

Geraghty reconstructed this term in 1990, and Tryon (1994) repeats the identification for PEOc, giving the cognates in various eastern languages, including PPN **piu*. Although Geraghty lists the other Lakalai *viluvilu* term, the word for 'sawfish' (which like the Lakalai themselves he ascribes to the resemblance of the sawtoothed leaves to the fin of the fish), he overlooked the plant name. Earlier French-Wright (1983:208-209), in his discussion of his PCPn **(m)piu*, pointed out that *Pritchardia pacifica* is confined to parts of Polynesia and Fiji. Noting possible cognates in Arosi and Canala, he suggests that the PCPn form "might well be an example of the reapplication of an established plant name to a newly found species". I am sure that he was right, and we should accept that the protoform designated different, thought similar-looking, plants in different parts of the Pacific. Assigning a single Latin binomial can only cause confusion. It would be tedious to describe my own quest for extensions of the *Licuala* term, which began with my recognising that the Kove and Lakalai plants were the same and with re-reading French-Wright.

POc **sabaka* 'Alstonia sp., including *Alstonia scholaris*'

Lakalai	<i>sabaka</i>	' <i>Alstonia</i> sp.'
Maenge	<i>samvaga</i>	' <i>Alstonia scholaris</i> '
Kwaio	<i>taba'a</i>	' <i>Alstonia scholaris</i> '

Cognates are widespread in the Solomons, including Guadalcanal *sambaya*, etc. (Tryon and Hackman 1983:215-217). Despite the similarity in form, these terms are presumably not related to PWMP **sabaqan* (Blust 1983-84:108), which seems consistently to refer to cordyline or something similar.

PEOc **m(w)ap(w)e* '*Inocarpus edulis*', discussed above. The definition seems clear, even though terms for *Inocarpus edulis* derived from PAN **ipil* are attested from Gitua to Tonga. Clark attributed **mwampwe* to PNCV (see data in Tryon 1994:498).

7 Conclusions

I realise that this paper may sound unduly critical of the editors of the volume, but the reason is only that they have done so much work on the topic – more than I could ever hope to do – that inevitably the possibility of carping about certain points arises. I would have liked

to give more attention to particular topics that have intrigued me and others, including Tom Dutton, such as: why terms change for no discernible reason, as with names for casuarina; the discontinuous distribution of certain terms and why a term is applied to one plant in one place and to another elsewhere (see Biggs 1991 on this point); and the existence of multiple terms for the same plant (see Tryon 1994:509). Because north-west New Britain is so widely thought to be something of a homeland for speakers of POc, I would have hoped to be able to contribute something useful on this last point, but can only point to some intriguing distributions. For example, a word for nipa palm, reflected in Lakalai *barema* and Kove *valevalema*, extends from East Nakanai across the Vitiaz Straits to Gitua, uniting languages which, though contiguous, share little in their vocabulary related to plants except for terms that can easily be derived from POc. They have different words for 'sago' and 'yams' (see Johnston 1980 for some of these), and I can find no external cognates for the nipa palm term. This is just a tiny example of the kind of problem that has so interested Tom as he has wrestled with the implications of the distribution of 'cultural vocabulary' in Papua (Dutton 1977). In the same article, along with complimentary comments about my 1963 paper, he also remarked, to my surprise, that "it deals only marginally with Papua New Guinea" (1977:71). I do not consider that on cultural or linguistic grounds Papua New Guinea can be separated from the rest of Melanesia, but I can only hope that my struggles to reconstruct plant names will be of some interest to those who, like Tom and everyone associated with linguistics at RSPAS, are as interested in culture history as in linguistics.

The sources of most of the lexical data should be obvious. Tolai material is taken from Lanyon-Orgill (1960), and Kwara'ae from Whitmore (1966).

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