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Ancient DNA and its contribution to understanding the human history of the Pacific Islands

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ABSTRACT

Two recent papers, by Lipson et al. *and Posth* et al.*, have challenged current interpretations of the initial settlement of Remote Oceania. We invited Stuart Bedford, who is an author on both papers, to outline their importance, and a number of scholars in various disciplines to comment on their findings.*

Keywords: aDNA, colonisation, Remote Oceania

RÉSUMÉ

Deux articles récents, par Lipson et al. *et Posth* et al.*, ont contesté les interprétations actuelles de la colonisation initiale de l'Océanie lointaine. Nous avons invité Stuart Bedford, qui a contribué aux deux publications, à discuter de leur importance, et un nombre de savants de diverses disciplines à commenter ces découvertes.*

Mots-clés: aDNA, colonisation, Océanie lointain

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STUART BEDFORD

The physical and cultural make-up of the inhabitants of the Pacific has been pondered at least since the first European ships sailed into its waters, and quite probably well before that, as different groups encountered each other over a 50000-year history. Since 1961, when Golson first recognised that Lapita pottery spanned the Polynesian/Melanesian divide, how this could be explained has been vigorously debated. Was there an Asian input or was it primarily an indigenous Papuan development; were there major migratory processes involved, and were they rapid or slow and incremental; and what happened after Lapita across its distribution?

Two recently published papers (Lipson *et al.* 2018; Posth *et al.* 2018), along with an earlier one (Skoglund *et al.* 2016), suggest that ancient DNA results have finally begun to make substantial contributions to this old-age debate. Both of the more recent papers generate genome-wide ancient DNA (aDNA), primarily from Vanuatu samples, that encompasses the 3000 years of human occupation, Lipson *et al.* from Central Vanuatu (Efate and the Shepherds) and Posth *et al.* from Malakula in the north and Futuna and Tanna in the south. Modern samples were also collected from Vanuatu as part of the Posth *et al.* data generation. All were compared with hundreds of present-day Oceanian and East Asian samples. Both papers arrive at a similar conclusion, that the initial settlement of Remote Oceania was primarily by populations of East Asian descent and that from c.2500–2300 BP there was increasing Papuan input (deriving from New Britain in the Bismarcks), to the point at which it dominated as it does in the population today. However, they diverge in terms of their explanation of the processes involved: Lipson *et al.*

talk in terms of a "second wave of migrants" from New Britain that, by 2300 years ago, had nearly completely replaced the original inhabitants; Posth *et al.* suggest that rather than one large-scale event, or second wave, "the process was incremental and complex, with repeated migrations and sex-biased admixture with peoples from the Bismarck Archipelago".

Both may be right, of course; different things may have been happening in different parts of the Vanuatu archipelago, with its 82 inhabited islands that stretch over 1000 km. But our results are only as robust as the data at hand. Pacific archaeological skeletal material is generally rare due to a whole host of reasons, including sometimes local sensitivity, and the increasing probability of loss with age. The "second wave" at 2300 BP as proposed in the Lipson *et al.* paper is based on a single sample (TAP 1). The closest samples to either side of this are the initial Lapita inhabitants c.500 years earlier and individuals from Epi who date to 1000 years later. Six of the 11 new samples from the same paper date to the past 150 years. Similarly, the single individual from Tanna (TAN002) in the Posth *et al.* paper does not tell the full human history of the first 1000 years in southern Vanuatu. We can always do with more samples.

The concept of waves of migration fits nicely with the archaeological record in relation to the Lapita story, which is of a rapid, extensive, largely homogeneous (certainly in terms of material culture) colonising migration that had its origins in ISEA and that moved rapidly into uninhabited territory beyond the main Solomons. However, a migration concept does not fit the post-Lapita story. Archaeological evidence for the region does not support any sort of "major wave" that even remotely resembles the Lapita one. If, as these two papers indicate in the case of Vanuatu, there is contact and biological input and admixture from the west (New Britain), then I would argue the archaeological record suggests that it is more likely to have occurred incrementally and unevenly over the past 2500 or more years rather than abruptly. These papers provide a platform for future research, which, with more sampling, will most likely demonstrate that things are much more complex than initially outlined. As Jim Specht has noted, "The results of archaeological research should always be treated as provisional and conditional on future work that may contradict, confuse or clarify existing knowledge and understanding" (Specht 2012: 22).

Anything to do with DNA in the Pacific needs to be handled sensitively and involve the full collaboration of institutions and communities. Both papers involved Vanuatu researchers and extensive discussions with communities. Post-publication, several authors visited co,mmunities on Malakula that had participated in the collection of modern samples. In subsequent fieldwork I have been able to discuss the results with people on Malakula, some of whom had given saliva samples, while others had decided not to. All were keenly interested to debate the results and, perhaps not surprisingly, nobody seemed particularly startled that the people of Vanuatu had strong biological connections with New Guinea. Indeed, overall, the results are not that surprising in themselves; similar scenarios explaining Pacific settlement have been around in archaeological circles for a long time (Golson 1961; Green 1963), but what the data generated by these papers demonstrates is the extraordinary potential of aDNA to contribute, in combination with other disciplines, to the understanding of the complex human history of the Pacific.

ROBERT BLUST

The papers by Lipson *et al.* and Posth *et al.* report the same discovery in slightly different forms. The essential point of both is that the notion of a biologically, culturally and linguistically uniform "Lapita" expansion from Near Oceania to Remote Oceania (Green 1991) is a serious oversimplification of Pacific prehistory. While it has long been assumed that the bearers of the Lapita archaeological culture in the Bismarck Archipelago were "southern Mongoloids" (SM), who spread rapidly from the western edge of the Pacific to both Micronesia and Polynesia, speaking languages that belong to the Oceanic subgroup of Austronesian (Bellwood 1975, 1978; Blust 1976; Pawley 1974), debate has turned on how much they were affected by contact-induced change with the non-Austronesian (NAN) populations that had settled Sahul and the insular Pacific as far east as the central Solomons many millennia before them. Both the "fast train" (Diamond 1988) and "slow boat" (Oppenheimer & Richards 2001) models of this process can be seen almost as caricatures of the Lapita expansion, but they highlight a genuine issue, namely whether the settlement of Remote Oceania was accomplished with little or no contact with the NAN populations of the Western Pacific, or whether the Oceanic-speaking peoples of Remote Oceania represent a fusion of two distinct historical traditions. Blust (2008: 454) described his position in terms of two (or more) migrations by biologically, culturally and probably linguistically distinct populations, called M1 and M2, and he maintained that the major research questions regarding what Spriggs (1997: 159) had called in general terms the "Melanesianised" populations of Vanuatu and New Caledonia, were as follows:

(1) What was the timing of M2 in relation to M1?
(2) What were the relative sizes of the populations in M1 and M2?
(3) Which of the following did M2 involve:
    (a) an AN-speaking population that had been "Melanesianised" in Near Melanesia,
    (b) a Papuan-speaking population that had been "Austrronesianised" in Near Melanesia, or
    (c) a Papuan-speaking population that was "Austronesianised" only after arriving in Remote Melanesia?

Given this problem, Blust (2008: 455–456) had the following to say, which must be quoted *in extenso* because of its relevance to the new evidence from genetics:

> Putting aside the current lack of archaeological support, the idea that large numbers of Papuan speakers who had adopted key elements of Proto-Oceanic culture arrived in Vanuatu shortly after the first wave of SM Austronesians is not inherently implausible. The arrival of Proto-Oceanic speakers on the north coast of New Guinea must have been an epochal event, comparable in some ways to the arrival of Europeans in the Americas. For tens of millennia Papuan speakers had lived in isolation from the outside world, knowing only their neighbouring groups. Suddenly, a new population that was physically and culturally distinct from the indigenous Papuans appeared along the coasts – highly mobile, linguistically much more uniform, at home with the sea, and possessing a range of new technologies that enabled them to expand as far as Fiji and western Polynesia within a few generations. There has been a tendency to think of Papuan speakers as hunkering down and holding their own in this situation. But contact with Proto-Oceanic speakers could have dislodged some Papuan-speaking groups and influenced them culturally before much gene flow had occurred. With a basic knowledge of the newly learned outrigger canoe complex, pottery, and some other elements of material culture, these groups, still speaking Papuan languages, could have left their home territories in the wake of the Austronesians, or together with them. In this way, Remote Melanesia would have been settled simultaneously or in rapid succession by both SM AN speakers and Papuan speakers.

The results obtained by Posth *et al.* and Lipson *et al.* could hardly provide a better match for this scenario. What is new in the genetic evidence is the specific sourcing of M2 in Vanuatu to New Britain (specifically the Papuan-speaking Baining of east New Britain, according to Posth *et al.*), and its dating to about 2300 BP, as against roughly 3000 BP for M1, according to Lipson *et al.* Not surprisingly, these groundbreaking results still leave important questions unanswered. One of these questions is the history of what Lipson *et al.* call a "third wave", which followed M1 into western Polynesia, with the result that the (limited) NAN genetic heritage of modern Tongans appears to show closer connections with the Solomon Islands than it does with Vanuatu. In addition, as noted in Blust (2008: 456), any attempt to portray Remote Melanesia as having two histories cannot ignore Fiji, which is linguistically, culturally and biologically closer to Polynesia than is true of any part of Vanuatu, yet where phenotypic variation ranges from almost Polynesian to something much closer to populations typical of Melanesia. This implies at least two migrations of biologically distinct populations, similar to what is now assumed for Vanuatu, but with much less influence from M2, suggesting that the M2/M1 population ratio was smaller in Fiji than in Vanuatu. In addition, there are important differences between the Vanuatu and Fiji cases in terms of language. Blust (2005, 2008) and Donohue and Denham (2008) gave specific linguistic reasons for assuming that M2 was Papuan-speaking when it reached Vanuatu. However, nothing similar is evident in Fiji, and one wonders whether it might have been settled first by the same population that left descendants in western Polynesia and Rotuma (M1), and a few centuries later by a mixed population from Vanuatu that was already AN-speaking as a result of the adoption of what must have been a widespread post-POC lingua franca. To summarise where research on the prehistory of Remote Oceania should be going from here, the Vanuatu case has shown that what is *a priori* improbable can turn out to be true, and if it is true in Vanuatu there is no reason why it could not also be true in other areas. Genetic research similar to what has already been undertaken successfully in Vanuatu should be extended to the Santa Cruz islands, where a similar double population history seems extremely likely – as Ross and Næss (2007) have shown, all languages of Santa Cruz are AN, but they are very aberrant, and some cultural features are reminiscent of New Guinea. It should also be extended to Fiji, where population relationships are no doubt more complex as a result of the "Austronesianisation" of a mixed (M1 + M2) Vanuatu population before it ventured onto an early Fiji, which was biologically, culturally and linguistically Polynesian when the Fijian M2 arrived. Finally, New Caledonia may hold other secrets of Pacific prehistory that differ in fundamental ways from what has been found in Vanuatu. In particular, although much of the New Caledonian population probably descends from the same M1–M2 fusion found in Vanuatu, various scholars have noted from time to time that it also appears to include an Australoid phenotype (Biasutti 1959: v. 1: 420; Bellwood 1978: 25). George Grace, who conducted linguistic fieldwork for several years on the island, felt that this was particularly noticeable in the north. These observations remain impressionistic, but it is conceivable that a sparse foraging population became established there before the onset of the Lapita revolution and its subsequent "Melanesianisation". The only way we will know for certain is to extend the kind of diachronic DNA sampling that has proven so revealing in Vanuatu to its southern neighbour.

DAVID V. BURLEY

The phenotypic and genetic distinctiveness of Polynesians and Melanesians has long been documented. At the same time, homogeneity in the Lapita archaeological record for the colonisation of Remote Oceania, including Tonga in western Polynesia, strongly suggests common ancestry. Thus the why and how of Dumont d'Urville's Polynesian/Melanesian divide have been consequential and perplexing questions (d'Urville 1832). The aDNA studies and related analyses of Vanuatu and Tongan skeletal remains and genomic data presented by Posth *et al.* and Lipson *et al.,* as well as slightly earlier contributions of Valentin *et al.* (2016) and Skogland *et al.* (2016), offer potential resolution. The Lapita migration from Near Oceania to western Polynesia was indeed a "fast train". It was undertaken by an Austronesian-speaking group (or groups) that, while originating from the Bismarck Archipelago, had virtually no genetic admixture with their Papuan neighbours. The Lapita peoples of Vanuatu were quickly replaced by a second wave of Papuan colonisers.

That this migratory wave occurred elsewhere in the Reef/Santa Cruz Islands, New Caledonia and presumably Fiji, but not Tonga, can be implied. The implications are consequential and substantive for Oceanic archaeology and our understanding of Polynesian origins. Polynesians are the ancestral vestige of Lapita peoples.

This narrative, as it has been penned, is not exactly a new one. Indeed, over 50 years ago Green (1963: 251) defined the initial phase of Fijian prehistory as the "Early Proto-Polynesian Period" that, ultimately, was replaced by plain and incised ceramic wares of Melanesian culture. In similar fashion, Spriggs (1997: 100) prophetically pondered another post-Lapita phase of migration through Island Melanesia that would have "swamped the 'pre-Polynesian' genotypes". Yet since the 1980s, most Oceanic archaeologists have been loath to consider migration as an explanatory mechanism. The emphasis, rather, has been on long-term *in situ* continuity, adaptive response to transitions in island ecology or sea-level change as well as other regionalised processes. Even Spriggs seemingly has taken this position. With Bedford (Bedford & Spriggs 2008: 113), for example, he was able to find nothing in the archaeological record of northern Vanuatu to support "secondary waves of migration" or even "high levels of interaction". Bedford and Clark (2001) similarly deconstruct the notion of an Incised and Applied Relief Ceramic Tradition, one proposed originally to demarcate migratory events in the "Melanesianisation" of Remote Oceania. With each of these individuals accepting authorship on either or both (Bedford) of the aDNA papers, we must assume reconsideration of earlier claims. And in this, it is noteworthy that Posth *et al.* seemingly reincarnate the Incised and Applied Relief Ceramic Tradition, this time through the 2550 BP Erueti ceramic complex of Vanuatu. This complex, as stated, "parallels a contemporaneous stylistic shift across Island Melanesia post-Lapita, including both New Caledonia and the Bismarck Archipelago".

My intention above is not to criticise my colleagues for past interpretations. What I do want to underscore is the critical significance of these aDNA results for reassessment of our earlier conclusions and for future understanding of the Oceanic past. I, too, am not without past failings. In a 2013 *Current Anthropology* paper, I sought to understand Fijian polygenesis, its different trajectory from Tonga and address the question of the Melanesian/Polynesian divide (Burley 2013). I assumed a slow boat model, where early Lapita communities in Near Oceania incorporated differential admixture with Papuan peoples, leading to a degree of biological variation. If separate founder events in Fiji and Tonga were drawn from biologically diverse populations, then the founder effect strengthened by isolation in Tonga would provide a beginning point for Polynesian/Melanesian differentiation. Clearly, this is now in error (Valentin *et al.* 2016: 296). Alternatively, the same paper's concern for Fijian polygenesis through integration within a western (Melanesian) interaction sphere as well as post–1500 BP immigration seems strengthened. The final verdict awaits expansion of aDNA studies into Fiji by Posth, Lipson or others.

We are taught that genetics do not lie. Yet with Posth *et al.* and Lipson *et al.*, there seems a more involved story to be told. The two papers share the bottom line and their approach, but it is in the linguistic implications identified in the Posth *et al.* paper where complications are brought to the fore. Despite "aberrant" Papuan features, Vanuatu languages are Austronesian, with an ancestral linkage to proto-Oceanic, the presumed language of Lapita peoples. A complete replacement by a second wave of Papuan peoples, thus presents a problem: incoming Papuans would have to adopt the language of the peoples being replaced. This type of occurrence, as recognised by Posth *et al.*, would be "extremely rare" and possibly "without precedent" in human history. We might even extend this type of observation to cultural integration/appropriation, should the appearance of continuity in the archaeological record be as robust as Bedford and Spriggs (2008) have made it out to be. Posth *et al.* consequently abandon the idea of a single dispersal event with replacement, proposing genetic replacement, where Papuans groups became incrementally absorbed into extant populations over an extended period. And to facilitate integration, they suggest "an undifferentiated proto-Oceanic operating as a lingua franca for linguistically diverse Papuan migrant groups". Whether incremental migration is able to substantially erase a Lapita genome within the two-century period beginning at 2500 BP (Posth *et al.*) and ending at 2300 BP (Lipson *et al.*) needs critical assessment and convincing elucidation. One must also go beyond a lingua franca and query the demographic landscape in Vanuatu as well as social behaviours facilitating Papuan dominance.

Finally, I am uneasy with the implications of these studies for descendent communities across Remote Oceania. Archaeologists have long validated their Lapita ancestry, we have worked closely with community members and we have empowered them with our assurances that they had come first. In Vanuatu, public promotion of this ancestry has been extraordinary through the media, through training workshops, in Cultural Centre exhibits and, in 2005, in a Lapita stamp series with Lapita peoples intentionally cast in ni Vanuatu likeness (Bedford *et al.* 2011). In New Caledonia in 2000, we called home the Lapita peoples of Near and Remote Oceania to celebrate the 50th anniversary of the first Lapita site excavations and to witness the reintegration of Lapita tribes at a traditional Kanak ceremony at Koné. One can only feel a collective sense of betrayal in all of this, regardless of the scientific significance and insights that Post *et al.* and Lipson *et al.* have generated.

MURRAY COX

Two wonderful studies by Lipson *et al.* and Posth *et al.* give us our first transect of genetic change through time for any region in the Pacific. Their data, hundreds of thousands of DNA polymorphisms screened across the human genome in

dozens of ancient and hundreds of modern samples, create a new standard of temporal and geographical genetic resolution. Together, these two studies represent a *tour de force* and, like all good science, their findings answer old questions and raise new ones.

Both papers convey a surprisingly similar story. The first settlers in Remote Oceania reached Vanuatu ~3000 BP (Skoglund *et al.* 2016). Without exception, the Lapita individuals have genomes the variants of which trace back almost entirely to Asia. Given their distinctive Lapita culture (Petchey *et al.* 2014), this community was almost certainly connected with the broader regional expansion of agricultural populations and associated Neolithic lifeways (Bellwood 2017).

However, by 2500 BP, the Remote Oceanic world had changed. Genomes with predominantly Papuan variants prevailed among the peoples of Vanuatu. The extent of Papuan ancestry was surprisingly heterogeneous; with nearly 100% Papuan ancestry, one man may have been a migrant "just off the boat", while a woman with a 50:50 mix of Asian and Papuan ancestry was conceivably a first-generation child. By 1500 BP, Papuan ancestry was much higher and far less variable, approaching modern levels sometime within the past 1000 years.

Ancient and modern DNA from Vanuatu are consistent in their support for a restricted origin of these Papuan settlers. Patterns of genetic diversity trace clearly to the Bismarck Archipelago, notably to the Baining of New Britain, with further linkages to mainland New Guinea and the Solomon Islands. Patterns of shared genetic variants also clearly reveal the path that these travellers took: people carrying Papuan DNA probably arrived in Vanuatu directly from the Bismarcks, leapfrogging the main Solomon Island chain, just like the Lapita colonists before them (Pugach *et al.* 2018; Walter & Sheppard 2017).

Against this broad backstory, it is an interesting thought exercise to ponder how such a transition may have played out for the people who lived through it.

Both studies assume that individuals with Papuan genomic ancestry arrived in Vanuatu speaking non-Austronesian languages. Indeed, this is a key argument in the Posth study: gene replacement without language replacement is a global oddity. But how robust is this conclusion?

There is a key time period for which we still lack data. The first Remote Oceanians, with their near-fixed Asian ancestry, lived and died in Vanuatu around 3000 BP. Given extensive associations between the rapid spread of Lapita culture (Bellwood 2013) and the equally rapid spread of Austronesian languages (Gray & Jordan 2000), it is an assumption – but a reasonable one – that these early Lapita individuals probably spoke an Austronesian language. However, 500 years, around 20 generations, elapsed before the next set of sampled individuals were living in Vanuatu – people carrying substantial Papuan variants in their genome at ~2500 BP. A great deal can happen in 500 years.

A question not explicitly asked by either study is this: what was going on in the Bismarcks?

Today, the inhabitants of the Bismarck Archipelago mostly have Papuan genomic ancestry. With minor exceptions such as the Mangseng and Melamela, Asian genomic variants are infrequent (~10%), similar to levels found across Vanuatu. The Baining of north-east New Britain speak a non-Austronesian language, but are an anomaly: the rest of New Britain and New Ireland are, with one exception, dominated by Austronesian speakers (Friedlaender *et al.* 2008). Lapita peoples were living in the Bismarck Archipelago by 3000 BP, hundreds of years before individuals with Papuan genomes travelled from there to Vanuatu. It seems at least plausible that many Papuan groups in New Britain had already transitioned to Austronesian languages by the time these movements to Vanuatu began.

The root causes of this language adoption were likely to be many, but were probably driven by the same reasons proposed for the widespread adoption of Austronesian languages everywhere else – a vehicle to engage with the economic advantages of the growing Austronesian world, with its new technologies, subsistence strategies and trading networks. Natural disasters, common in the Bismarcks, may have also stimulated population movements, cultural innovation and change (Torrence 2016). Papuan communities in the Bismarck Archipelago retained their genes, while adopting new Austronesian languages, a commonplace process that is no oddity at all. These transitions to Austronesian languages, like those elsewhere in the region, presumably brought various Papuan linguistic elements along for the ride (Lansing *et al.* 2007).

An alternative model can therefore be posited for Vanuatu. After 500 years of Austronesian–Papuan contact in the Bismarck Archipelago, it seems reasonable that at least some groups with high Papuan genomic ancestry already spoke Austronesian languages there, probably tinged with Papuan features. Languages (mostly) change slowly and, given the uniformity of culture across wide expanses of geography during the early Lapita period and the extensive sphere of interaction that this implies, the Austronesian languages spoken in the Bismarcks were probably not so different from the Austronesian languages spoken in Vanuatu. A common language may have been one of the draws for people to leave their homes in the Bismarck Archipelago to make a new start in the islands of Remote Oceania. Such a model is even more compelling if, as argued by both studies, Papuan movements to Vanuatu largely involved sporadic immigration of individuals over long periods of time, rather than one or a few population migration events.

Records of cultural change are consistent with such a model. The uniformity of the Lapita assemblage in Vanuatu began breaking down by ~2700 BP (Bedford & Spriggs 2014), suggesting that earlier long-distance contact networks were being at least partially supplanted by a focus on the local. Speculatively, some aspects of the Lapita assemblage, including the artistry of their pottery, almost has the feel of a religion (Bellwood *et al.* 1995). Genomically, Papuan individuals had every reason to adopt

the beneficial aspects of the Austronesian economic system, including its lingua franca, but may well have been less committed to the more ceremonial aspects of the culture.

Ultimately, the first Papuan migrants to Vanuatu left no direct linguistic record, so we cannot know for certain what languages they spoke. But this, of course, is the point. Posth *et al.*, and to some extent Lipson *et al.*, assume that Papuan genomic ancestry arrived in Vanuatu in the form of people speaking non-Austronesian languages. While they possibly did, this perhaps misses a more thought-provoking point: the extraordinarily complex dynamics that these studies reveal in Vanuatu presumably had related, but earlier, counterparts in the Bismarck Archipelago. Perhaps the reason why Austronesian languages alone are spoken in Vanuatu today is simply because non-Austronesian languages were never spoken there in the first place.

PATRICK V. KIRCH

Some years ago, in an effort to advance a holistic approach to historical anthropology in the Pacific, Roger Green and I advocated the application of what we called a "triangulation method", in which "the subdisciplines of historical linguistics, archaeology, comparative ethnology, and biological anthropology independently contribute their data and assessments to the common objective of historical reconstruction" (Kirch & Green 2001: 42). At the time of our writing nearly two decades ago, biological anthropology had yet to undergo the DNA-sequencing revolution that has recently transformed the field. Consequently, in our own study of Polynesian phylogenetic history, Green and I relied mostly on archaeological and historical linguistic data, giving the biological perspective only passing notice (Kirch & Green 2001: 73-5). Today, major advances in molecular biological anthropology have put it in the forefront of contemporary studies of ancient population movements and demographic histories (Reich 2018). This is nowhere more evident than in the two papers under consideration here (Lipson *et al.* 2018; Posth *et al.* 2018), which have rightly attracted significant attention due to their major implications for revising our understanding of the prehistory of the south-western Pacific.

While the new ancient DNA results are indeed remarkable, I would like to urge caution so as to avoid the pitfall of being so awed by the seeming analytical precision of the new DNA-based models that we allow them to dominate the discourse of Oceanic prehistory. This explains my reference to the triangulation method, and my insistence on the importance of bringing *all* of the varied anthropological perspectives – of archaeology (material culture), historical linguistics, and comparative ethnology – to bear in our efforts to understand the processes that shaped the course of human history across the archipelagos of the Bismarcks, Solomons and Vanuatu during the first millennium BC.

Both Lipson *et al.* (2018) and Posth *et al.* (2018) tell essentially the same story: (1) that the initial population of Lapita colonisers of Remote Oceania (whom Lipson *et al.* call "First Remote Oceanians") had a genetic make-up essentially derived from an East Asian source population with no significant admixture of "Papuan" genes; and (2) that between roughly 2700 BP and 2300 BP, the descendants of this original First Remote Oceanian population in Vanuatu underwent massive genetic admixture with Papuan populations, primarily from the New Britain region. Both research teams see this process of genetic replacement as the outcome of significant movement of people out of the Bismarck Archipelago into Vanuatu.

Lipson *et al.* are apparently content to tell the story in strictly biological terms, while Posth *et al.* make limited efforts to correlate their results with the evidence from historical linguistics, comparative ethnography and archaeology. They point to certain features of Vanuatu languages, such as quinary numeral systems and serial verb construction, as apparently Papuan linguistic features that were borrowed into the Austronesian (Oceanic) languages of the First Remote Oceanians. They also mention certain "ethnographically attested cultural practices" that are shared between Vanuatu and New Britain cultures. Finally, they briefly mention the Erueti ceramics in relation to "Lapita fragmentation", but the argument is left undeveloped. I applaud Posth *et al.* in these efforts to engage with other branches of historical anthropology, with essential aspects of the "triangulation method", for these enrich our understanding of the processes involved, which were surely more complex than just genetic replacement.

What neither Lipson *et al.* nor Posth *et al.* have attempted, however, is to assess the genetic story against the evidence of archaeology, at least not in any detail. It is not evident whether this is because they have no interest in the archaeological evidence, or because they regard archaeology as irrelevant to the story they are telling. Yet to my mind, archaeology has much to contribute to our understanding of what transpired in the mid-first millennium BC across this region. I refer here to an issue that has puzzled Pacific archaeologists ever since systematic excavations began in Melanesia in the 1960s and 1970s, namely the widespread replacement of classic dentate-stamped Lapita pottery with incised and appliqué ceramic assemblages beginning around 2700 BP.

In the first edition of *On the Road of the Winds*, I raised this question of the widespread appearance across Island Melanesia of similar post-Lapita ceramic assemblages, and what it might mean:

> More interesting … are parallel style changes that occurred from the Bismarcks to Vanuatu and New Caledonia, and that are reflected to some degree in Fiji. These are the decline and abandonment of the classic Lapita dentate-stamped technique of decoration and its replacement primarily with incised designs, but also with designs executed using other techniques, such as end-tool impressing, fingernail impressing, relief, and appliqué … The question arises whether these developments – most of which seem to occur in the period from about 2500 to 1800 B.C. – were in any way related.

> One possibility is that similarities in these style changes reflect continued interisland and inter-societal contacts and thus the sharing of technical and stylistic innovations. (Kirch 2000: 162)

In my view, the new genetic findings strengthen the hypothesis succinctly advanced in that final sentence, that the widespread appearance of a new set of ceramic assemblages across the archipelagos of Island Melanesia in the mid-first millennium BC, assemblages characterised in large part by various incised decorative traditions but also by similarities in vessel form (for example, globular jars with everted rims) and aspects of manufacture, was the result of continued contact between the populations extending from the Bismarck Archipelago down through Vanuatu, and indeed to some extent into the Loyalties and New Caledonia. What we are probably seeing in these ceramic sequences is the archaeological reflection of the population movements recently revealed in the genetic record. Further exploration of this hypothesis, through detailed comparative analyses of mid-to-late first millennium BC ceramic assemblages across Island Melanesia, would seem a fruitful avenue for new research.

The real strength of anthropology has always been its holistic reach, its ability to integrate disparate kinds of data ranging from pots, to words, to genes. The recent advances in molecular biological anthropology are indeed remarkable; they give the biological anthropologists a full seat at the table. But they do not lessen the importance of other ways of knowing about the past. I hope that as such studies continue, there will be a real effort to engage with the full potential of the triangulation method.

ELIZABETH MATISOO-SMITH

The two recent papers by Lipson *et al.* (2018) and Posth *et al.* (2018), on aDNA in the Pacific, have a primary focus on Vanuatu. The conclusions of both of these papers are directly tied to and build upon the results of an earlier paper by Skoglund *et al.* (2016) and their description of the "First Remote Oceanians". I will briefly discuss my concerns with this basic premise and suggest that if this is not taken as given, the results and interpretations of the Posth and Lipson papers would not be drawing such attention as to warrant this forum discussion.

The genetic signature of the First Remote Oceanian "population" was described by Skoglund *et al.* based on the DNA from the petrous portions of three of the skulls that were recovered from clear ceremonial contexts at Teouma: burials 30A, 10B and 17. Sadly, the degraded nature of the DNA from the post-cranial remains and loose teeth from those buried at the site has meant that, to date, no DNA has been obtained from any actual burial from Teouma. Skoglund *et al.* also recovered similar data from one skull from the Talasiu site in Tonga, dated to 2680–2340 BP, or a few hundred years after those at Teouma. It was determined that the four Lapita crania had DNA profiles similar to aboriginal Taiwanese (Ami and Atayal) and Philippine populations (Kankanaey), and were clearly distinct from modern Near Oceanic populations, as well as ni Vanuatu. The DNA obtained led the authors to conclude that there was little to no Papuan ancestry, not just in the three Teouma crania, but in all of the First Remote Oceanians. In their conclusion, the authors did state, "While our findings cannot rule out the possibility that multiple groups – some of which carried substantial amounts of Papuan ancestry – settled Remote Oceania early on, the lack of such ancestry in both Vanuatu and Tonga can be more parsimoniously explained by later population movements bringing the Papuan ancestry." They then rightly pointed out that this view is "radically different" from previous explanations for the genetic diversity seen in Remote Oceania today. Therefore, it was their "more parsimonious explanation" that was promoted despite, but perhaps because of, the fact that it required a complete reassessment of the process of Lapita expansion, particularly regarding the degree of interaction in Near Oceania during the 300-plus years of occupation prior to populations moving out into Remote Oceania. Needless to say, this result made headlines worldwide.

In early 2018, the Lipson *et al.* and Posth *et al.* papers appeared online within days of one another. Interestingly, these two papers involved subsets of the original author list from Skoglund *et al.*, some authors appearing on all three papers. Both papers report results for additional ancient samples from Vanuatu and more genomic data from modern ni Vanuatu populations. Their results indicate predominantly Near Oceanic ancestry for the 11 (Lipson) and 12 (Posth) new archaeological samples from Vanuatu that they studied, which date from 2600 BP to 200 BP, and both identify a likely Bismarck Archipelago (Tolai/Baining) ancestry for the Vanuatu populations. These two new papers are very interesting indeed and if they had appeared before the 2016 paper, the results would not challenge what many would accept was a consensus view of Lapita expansion into Remote Oceania: Heterogenous groups of Austronesian-speaking peoples, whose Lapita ancestors had been in the region for 300-plus years, left the Bismarck Archipelago, not necessarily all from the same location, and skipped over the Solomon Islands, to settle the western portions of Remote Oceania; interactions with the homeland region were maintained for a few hundred years before voyaging reduced and independent indigenous cultural developments ensued (Burley 2013; Matisoo-Smith 2016; Sheppard 2011). Similarly, the thought-provoking title, "Language continuity despite population replacement", would not be necessary. As Posth *et al.* point out, this scenario is an unusual, if not an unheard-of, phenomenon worldwide.

So, the argument of both new papers regarding rapid population replacement is based primarily on the DNA from the Teouma skulls, yet little is made about their unusual presence and context. In their 2010 discussion of the archaeological context, Valentin *et al.* (2010) point out that it could not be determined if the skulls found at Teouma came from the cemetery or from an entirely

different location. Later, Valentin *et al.* (2016: 293) state "Although disconnected from the infra-cranial skeletons these cranial remains seem to have belonged to individuals of the same group, as shown by similarities in isotopic values measured in bone collagen and in dental enamel", citing Petchey *et al.* (2014) and Bentley *et al.* (2007).

Bentley *et al.* (2007: 654) suggest that at least one of the skulls (Burial 17) is an immigrant, due to the more terrestrial diet as indicated in isotopes obtained from their dental enamel. Cranium 30A had not been studied for isotopes, leaving only 10B, which appears to have similar isotope values to 17 other burials from Teouma. However, when the isotope values for other ancient Pacific populations, such as those in the Bismarck Archipelago (Shaw *et al.* 2009, 2010) are compared to those of Teouma, the values are all remarkably similar. The Teouma results easily fit within the ranges found at Watom. Therefore, based on the isotope data, the Teouma crania could have come from many other parts of the Pacific, including the Bismarck Archipelago. Their treatment in the cemetery certainly distinguishes them from all of the other crania of the deceased. The assumption that they represent the burials from the Teouma cemetery is therefore not necessarily well supported.

Rather than suggesting a complete replacement, might it be "more parsimonious" to argue that the three ceremonial skulls are not necessarily representative of the founding population as a whole, but perhaps an unusual subset or possibly curated skulls of ancestors or even offerings? Before we go and rewrite Pacific prehistory to match a picture of population replacement being uncovered in a continental European context (Allentoft *et al.* 2015; Haak *et al.* 2015), perhaps we should just stop and consider all possibilities. The range of possible explanations may not be as intriguing to the editors at *Nature* or *Science*, but presenting all scenarios, I would argue, is better science.

In addition to my concerns regarding the interpretations presented in these Pacific aDNA papers, I add my voice to the growing pleas for caution coming from several regions regarding the "bone rush" and "industrialisation" of ancient DNA research (Bardill *et al.* 2018; Prendergast & Sawchuk 2018). We are increasingly seeing a situation in which a few large and extremely well-funded international laboratories are in a race to collect, analyse and publish as much ancient DNA data as they can get their hands on, from every geographical region, as quickly as possible. This rush to publish leads to a lack of discussion both professionally and with local communities to consider all possible interpretations and ways of explaining the implications of results. The results that speak the loudest, "the first" or most controversial are often published in the "top" journals, and tend to make the biggest impact, even if they are wrong or present only one possible explanation. A singular statement, such as that the First Remote Oceanic populations "had little to no Papuan ancestry" (Skoglund *et al.* 2016: 510) makes the headlines. The later re-analysis of the same samples that shows as much as 3–4% Papuan ancestry (Lipson *et al.* 2018) gets little attention. We seem to have reached a point at which the publication is what is driving the research rather than the other way around. I urge my colleagues to stop, take your time and consider all interpretations of data and, most importantly, make the archaeological and cultural context at least as important as the new DNA data. Think before you distribute the rare and precious Pacific samples to multiple laboratories to see who can get the fastest result. Science should be collaborative, not competitive.

ÅSHILD NÆSS

Lipson *et al.* and Posth *et al.* bring much-needed data into the longstanding discussion of the presence of supposedly "Papuan" traits in languages of Vanuatu, for which a variety of explanations have been proposed (Blust 2005, 2008; Donohue & Denham 2008; Pawley 2006); the results give sound support to "the idea that large numbers of Papuan speakers who had adopted key elements of Proto-Oceanic culture arrived in Vanuatu shortly after the first wave of SM Austronesians" (Blust 2008: 455).

While others are better placed to discuss the implications of these findings for Vanuatu linguistics, I will offer a brief comment on how they may help shed light on the history of the Reefs – Santa Cruz (RSC) languages, spoken in Solomon Islands' Temotu Province, just north of Vanuatu and east of the Near/Remote Oceania divide. These languages were long assumed to have a non-Austronesian substrate (e.g. Wurm 1978, 1981, 1992); however, Ross and Næss (2018) provided evidence that they belong to a first-order subgroup of Oceanic that has no members outside of Temotu Province, implying that the original settlers came directly from the Proto Oceanic homeland in the Bismarck Archipelago and were, presumably, of "First Remote Oceanian" genetic stock. However, recent work (Delfin *et al.* 2012; Duggan *et al.* 2014) has complicated the picture by showing that the genetic make-up of modern-day Santa Cruz is largely of Near Oceanian origin, a result that is further confirmed by the new studies.

While the latter pertain to Vanuatu rather than specifically to the Santa Cruz archipelago, they show a population history in the overall region where an early Austronesian settlement was followed a few centuries later by an influx of people with predominantly Papuan genetic material; this goes a long way towards explaining the apparent mismatch between linguistics and genetics in RSC. What remains to be explained is what the impact of this second migration was on the RSC languages, where the putative non-Austronesian substrate has largely been refuted (Næss 2006; Næss & Boerger 2008; Ross & Næss 2007).

For reasons largely to do with chance, comparative-historical work on RSC has focused on Äiwoo, the language of the Reef Islands some 70 km north-east of Santa Cruz – though Næss and Boerger (2008) include data from Natügu of Santa Cruz, and Ross and Næss (2007) draw on phonological data from all the Temotu languages. It so happens that Äiwoo is the RSC language that most

clearly preserves a number of Oceanic structural patterns. The most striking of these is the so-called symmetrical voice system, a pattern of clausal organisation that is common in the western Austronesian region, particularly Taiwan and the Philippines, but uncommon in Oceanic – indeed, it has generally been thought to have been lost by the time of the Proto Oceanic (Lynch *et al.* 2002: 60-2). While this raises the possibility that Proto Temotu was in fact a sister rather than a daughter of Proto Oceanic, I consider it more plausible that some version of the system was retained in Proto Oceanic (Næss 2013, 2015), and that its retention is an indication of Äiwoo being a grammatically very conservative Oceanic language.

While a systematic comparison of the basic structural features of Äiwoo and the Santa Cruz languages is pending, it is clear that there are significant differences. Focusing on clause structure, available data (Vaa 2013; Boerger ms) show a rather different picture than in Äiwoo, with a complex set of valency-changing morphemes that do not obviously add up to anything like the Äiwoo symmetrical voice system. Assuming that the Äiwoo system is the original one, this amounts to a restructuring of some of the most basic properties of clausal organisation.

Another domain where the Santa Cruz languages are strikingly different from Äiwoo is phonology, where Äiwoo has added two vowel phonemes to the standard Oceanic five-vowel inventory, but these can be straightforwardly accounted for through phonemisation of an original process of front–back assimilation; the SC languages, on the other hand, all show ten oral vowels and, for Natügu and Nalögo, an additional five nasal vowel phonemes. In general, although many of the basic grammatical categories are the same across all of RSC, details of, for example, aspect/mood systems and person marking differ considerably, and there has been a remarkable lexical differentiation to the point at which there is little or no mutual intelligibility, or even surface similarity, at first glance.

In light of the new genetic data, one possible explanation for this situation is that present-day Äiwoo derives from a group of first-wave (Lapita) settlers who left Santa Cruz for the Reef Islands before the second migration arrived. While the Santa Cruz languages were significantly affected by contact-induced language change as a result of the new arrivals, Äiwoo was at the margins of the region and therefore did not experience the same intensity of contact. It has therefore retained its conservative Oceanic characteristics to a much greater extent than the languages of Santa Cruz. More detailed comparative studies may help to confirm or disprove this hypothesis.

What would the nature of the language contact situation following the second migration have been? Blust (2008) favours a scenario in which a number of different "Papuan" linguistic groups acquired Oceanic as a lingua franca after their displacement into Remote Oceania, and this hypothesis seems to be adopted by Posth *et al.* But such linguistic absorption seems difficult to reconcile with the almost complete genetic replacement shown by the current data. It is noteworthy that the incoming genetic material originates largely in New Britain, usually posited as the homeland of Proto Oceanic (Pawley 2008). A better fit with the linguistic facts would seem to be a scenario in which the new arrivals already spoke a variety of Oceanic, at least as a second language, having acquired this along with the other "key elements of Proto-Oceanic culture" suggested by Blust; and where this variety, influenced by one or more non-Austronesian languages, was used in the new settlements. Returning to Reefs – Santa Cruz, then, what we are looking at is not so much a return to the original hypothesis of Oceanic/Papuan contact advanced by Wurm but, rather, a scenario of contact in Santa Cruz, a few centuries after original settlement, between Oceanic speakers and new arrivals already speaking some variety of Papuan-influenced Oceanic. I anticipate with great interest a broader discussion of how the current linguistic situation in Remote Melanesia can be interpreted in light of the new knowledge about migration history.

ANDREW PAWLEY

The two papers by Lipson *et al.* and Posth *et al.* report exciting findings, though one wishes that some of the conclusions had been formulated more cautiously, given the geographical and temporal limitations of the population samples; for example, individuals from Lapita sites are limited to Efate and Tonga.

The findings about the replacement of "East Asian/Austronesian" DNA by "Papuan" DNA in Vanuatu populations, beginning within a few centuries of the initial Lapita settlement, are not unexpected. The DNA evidence is consistent with similar conclusions about substantial post-Lapita population movements into Remote Oceania based on archaeological evidence (concerning seemingly parallel changes in ceramic style from the Bismarck Archipelago to Vanuatu, New Caledonia and Fiji) proposed by scholars such as Bellwood (1978), Green (1963), Kirch (2000), Spriggs (1997) and Summerhayes (2007). Note, for example, the following remarks by Spriggs (1997: 158-9):

> It is an old idea that post-Lapita cultures, or those contemporary with Lapita represent a secondary migration of people into the Island Melanesian region … If there was a secondary movement of population from the Bismarcks to the south and east, it was a movement from the same general area as the original Lapita spread … The population would, however, be more mixed with the original Bismarcks' inhabitants than the previous Lapita spread, providing the more "Melanesian" phenotype that is found today in Vanuatu, New Caledonia and to a lesser extent in Fiji.

What is new is DNA evidence confirming that such secondary movements had begun no later than c.2400 BP and that the main source of the Papuan intrusion was the Bismarcks, particularly New Britain. The question arises as to what explanation(s) (social, economic, political, epidemiological etc.) can be offered for substantial

movements of people of "Papuan" genetic make-up from the Bismarcks into Remote Oceania after about 2400 BP, bypassing the Solomons, and for their subsequent incorporation into and genetic domination of established communities who nevertheless retained their original Oceanic languages. It would make sense if these movements began not as late as 2400 BP but very soon after the initial Lapita settlement of Remote Oceania, making use of networks established by the Lapita settlers and at a time when populations in Remote Oceania were still small.

In the paper by Posth *et al.*, there are some eyebrow-raising remarks about linguistic matters:

(1) "Population replacement with language continuity [referring to the case of Vanuatu] is extremely rare – if not unprecedented – in human history." One needs to look no further than Near Oceania to find scores of Austronesian languages that are spoken by populations of largely Papuan phenotype, no doubt the result of intermarriage between "South-East Asian/Austronesian" and "Papuan" populations. There are also a few cases of communities that have retained Papuan languages although their speakers are now of partly "Austronesian" phenotype, indistinguishable from neighbouring communities who speak Austronesian languages; for example, Koita and Mailu in Central Province, Papua New Guinea.

(2) "Bayesian phylogenetic analyses of the … Austronesian languages broadly support the 'express train' model of the Austronesian expansion, whereby Austronesian-speaking groups had negligible cultural or genetic interaction with indigenous Papuans in Near Oceania before moving further into the Pacific." The spread of Lapita from the Bismarcks across the south-west Pacific as far as Tonga appears to have been very rapid, but archaeological evidence indicates that more than 500 years elapsed between the arrival of Austronesian-associated Neolithic cultures in the Philippines around 4000 BP and their appearance in the Bismarcks around 3300–3200 BP. The comparative method of historical linguistics identifies many phonological and grammatical innovations that define the Proto Oceanic interstage (Lynch *et al.* 2002; Pawley 1973), consistent with a formative period of a few centuries in Wallacea and/or Near Oceania before the spread of Oceanic languages into Remote Oceania.

What factors allowed the bearers of the Lapita cultural complex and its forebears to retain so much of their South-East Asian genetic heritage for several centuries as they moved through Wallacea and Near Oceania? The migrating populations must have been small but socially cohesive, carrying a substantial number of females. Their social cohesion was underpinned by their distinctive way of life as pottery-making farmers and fishers. They probably had descent groups headed by hereditary chiefs (Hage 1999), who organised the construction of and voyaging in ocean-going canoes, and perhaps, as Blust (1980a,b) argues, they had marriage rules, such as a preference for cross-cousin marriage, that favoured recruitment of spouses from designated descent groups.

(3) It is a half-truth to say that Vanuatu is "per capita the most linguistically diverse place on earth". Vanuatu has a lot of shallow linguistic diversity; that is, it contains many (indigenous) languages that are quite closely related, all belonging to the Oceanic subgroup of Austronesian. With respect to deep diversity – that is, the number of languages/language groups that are not demonstrably related, or only distantly related – Vanuatu does not remotely approach the diversity of various areas of New Guinea of roughly comparable size and population. For example, Madang Province in Papua New Guinea (population 360000) has about 170 languages belonging to at least four apparently unrelated families – a degree of deep diversity greater than that of the whole of pre-Columbian Europe.

(4) "[M]any aberrant, seemingly Papuan, linguistic features are evident" in the Vanuatu languages. This is a contested claim (Blust 2008; Pawley 2006). A few candidates for "Papuan" features have so far been identified, some stronger than others.

CHRISTOPHE SAND

I remember vividly that the first scientific result on the age of the Holy Shroud of Turin that was presented to the public concluded that the cloth was of late medieval age. A boost in media release followed this announcement, giving a short-lived fame to the scientists who had obtained the dates. But as time went on, and without negating the ^{14}C dates, the fulfilment of new analysis (on the fibres, pollen etc.) led to new results, which showed a more complex history for the Shroud, one that started about 2000 years ago in the Middle East.

If I start my comments on the recent papers of Posth *et al.* (2018) and Lipson *et al.* (2018) with this well-known episode of late twentieth century science, it is to remind us that any first set of new results on old remains must be treated with total caution. In these two papers, the authors, relying on DNA results extracted for the very first time from 3000–2000-year-old skeletons on Vanuatu, conclude that there was "almost complete population replacement" (Posth *et al.*: 733) by "Papuan" people (a term in itself not defined and culturally misleading) originating in the Bismarck Archipelago after initial Lapita discovery by groups of South-East Asian ancestry, without major shift in the Austronesian languages basis inherited from the "first settlers". To make their case, the geneticists rely for the first settlement phase on a set of four results from the burial ground of Teouma on Efate and on one tentative Lapita-related burial on Urupiv, which show very little "Papuan" genetic admixture and are classified ubiquitously as "Lapita-Austronesian" (Posth *et al.*) or as of "First Remote Oceanian ancestry" (Lipson *et al.*). Graphically,

these "Lapita" are positioned as an isolate apart from all present-day populations. About the same number of samples serve to demonstrate the genetic change in the second half of the first millennium BC. The immediate methodological question that arises from the papers can be summarised as follows: are less than a dozen samples from a handful of sites statistically meaningful to write anew the first phase of human settlement in Remote Oceania, when archaeologists can today identify a significant diversification of cultural trajectories *during* the 200–300 years of Southern Melanesia's Lapita period? No one today denies a significant Asian input in the Lapita Saga, shown from linguistics to artefact typology to the study of the skeletons of Teouma. But to go from there to a scenario of "population replacement" at the end of the Lapita period by relying on such a ridiculously small DNA sample is premature, to say the least. Lapita was not a one-arrow, down-the-line settlement process, but comprised multiple points of origin in the Bismarck Archipelago, successive departure periods over a couple of centuries by a diverse array of populations, a number of leapfrogs and bypassing of islands, and return voyages northwards, all this certainly accompanied by the development of a whole set of pidgin-Austronesian languages along the way. Any of these events would have allowed a diversification of the genetic pool and the presence of the "Papuan" markers in Central and Southern Melanesia at any stage *during* the Lapita period. The process of genetic diversification was, unsurprisingly, an integral component of the Lapita period, as Near Oceania was the only demographic reservoir from which the progressive settlement of the many islands of Remote Oceania could have been fuelled. Accordingly, there was never any "population replacement", but a rapid diversification of genetic, cultural and linguistic variability in the Melanesian Crescent. This is what the burials of the small island of Urupiv illustrate. If a "replacement" had taken place, all cultural chronologies of Vanuatu (and New Caledonia) should show a hiatus: this is *not* the case, just as the claim (Lipson *et al.*: 1161) of an abrupt disappearance of long-distance interactions in Southern Melanesia at the end of the Lapita period is also not true.

The assumption that present-day populations in each island are the same as those that were there 4000, 3000 or 2000 years ago is untenable In Melanesia, which is rich in oral tradition highlighting clan movements and known to be a region of regular natural cataclysms potentially affecting localised continuity in human occupation. This analytical prerequisite of DNA analysis appears as one of the main pitfalls of these studies. Inter-island comparisons will start to be scientifically robust only when it will be possible to compare genetic data from the *same* chronological periods. Furthermore, genetic conclusions for Oceania will have to take into account the major bottleneck process that has resulted from the massive population crash experienced by Pacific Islanders in the past four centuries, due to the introduction of European diseases. This led to the disappearance of somewhere between 80% and over 96% of the indigenous populations in the Islands, partly reconfigured during the nineteenth century by the indentured colonial workers across the region.

Reading the two papers raises a number of other shortfalls and misinterpretations of the archaeological data. But I devote the end of my comment to another aspect of the publication of these papers: their impact on present-day Pacific Islanders. Some scientists consider that their work has, above all, the objective to provide new data and interpret them. Appealing scenarios that make the headline news are always welcomed. But hard data, especially when it comes to ancient DNA and the reconstruction of past settlement episodes with descendants who are alive today, cannot be used in a vacuum, separated from the anthropological context. Consequently, the use of titles such as "population replacement" or "population turnover" can have a devastating impact, not the least being to spread amongst Oceanians the idea of "invasion". In New Caledonia, where the indigenous Kanaks have strong political power and are trained in jurisdictional defence, the Customary Council of High Chiefs has been approached in 2017 to validate the start of a first important genetic study on the indigenous inhabitants. In a context of political decolonisation and after having fulfilled a detailed study of the implications of a genomic study on the Kanaks, the Council has decided to impose a three-year moratorium, considering that the protection of the DNA results was not robust enough to potentially allow for proper control of the data obtained. Especially, the results could potentially be used for future studies without the consent of the participants (as is the case in Posth *et al.*, fig. 8). This decision was in part influenced by the recent reappearance, on web forums, of the old-fashioned colonial theory that "the Kanaks were not the first settlers", relying on publications like the two discussed in the present forum. By declaiming (even if this is not their goal) on slim scientific data and with binary conclusions, the historical rights of the indigenous populations of Southern Melanesia, Western geneticists perpetuate a form of intellectual colonial power and do not contribute to highlighting what is the prevalent characteristic of the deep past of the south-western Pacific: its extraordinary complexity.

PETER SHEPPARD

I am happy to see these papers, which provide us with an interesting new dataset with which to consider Pacific settlement – particularly as they demonstrate something I have been arguing since 2006 (Sheppard 2011; Sheppard & Walter 2006), based on then-available data, that the initial movement into Remote Oceania from the Bismarck Archipelago leapfrogged the Solomon Islands. I will comment on two issues: first, the conclusion that there is a two-step migration into Remote Oceania of a population of "East Asian" Lapita genetic ancestry, followed post-Lapita by a population of Bismarck Archipelago or "Papuan" genetic ancestry; and, second, that there are issues around population sampling and variation.

I am very partial to the proposed description of Lapita settlement of Remote Oceania made by the linguist Andrew Pawley in 2006:

> The notion that Lapita canoes from the Bismarck Archipelago carried some people of "Papuan" as well as "Southern Mongoloid" phenotype does not seem at all far-fetched. The Lapita colonisation of Remote Oceania was astonishing in its speed and scale. Dozens of new settlements were founded in different parts of Remote Oceania within a century or two, and this must have involved the movement of considerable numbers of people organised by ambitious and adventurous leaders. It is hard to imagine that the Oceanic-speaking Lapita migrants could have accomplished this rapid colonisation without recruiting men and women from non-Oceanic speaking communities that they came in contact with in Northwest Melanesia. Whether they were recruited as spouses, as slaves or in some other role, we may never know. That is not to say every Lapita canoe setting sail for Remote Oceania carried some passengers of Papuan stock, only that some vessels did.

I am therefore surprised to see the interpretation in these papers which indicates that "East Asian" (actually distinct samples most like Island South-East Asian) populations maintained their genetic identity for perhaps 12 generations in the Bismarcks and then led the settlement of Remote Oceania, followed later (post-Lapita?) by a second major migration. This seems rather complicated! That "Asian" or "First Remote Oceanian" (a better descriptor) populations distinct from their Bismarck neighbours were early seems clear in Posth *et al.* (fig. 1), given the sample, and is very interesting, suggesting that some elite cultural factor is at work. That other Lapita-period settlers had Bismarck DNA more like modern populations is also apparent. Sample TAN002 from Tanna dates within the late Lapita period and the samples from Malekula (Mal02 and Mal04), which do not plot in the PCA with the other Early Lapita samples, are also described in Posth *et al.* as late Lapita. These samples *appear* to plot nearest to Baining and Mussau (it is hard to read the plot). A late Lapita push into the established settlements of Remote Oceania may be indicated, perhaps at the same time as the movement into the Western Solomons and along the south Papuan coast. Personally, I would be very surprised if a simple two-migration model was accurate, especially at a post-Lapita time when archaeology indicates increasing regionalisation consistent with the settling in of a growing founding population. Of course, the sample is still limited and differences in variation of small samples and over interpretation of results can make big differences, as shown in these data as discussed below.

Genetic admixture and processes such as the founder effect can alter the genetic make-up of populations over time, making descendent populations sampled today unlike ancestral populations. This would apparently account for the difference between the ancient First Remote Oceanian population and the East Asian samples, and the fact that the early Lapita samples are unlike any modern population sampled. Admixture has clearly been at work in the modern populations from Near and Remote Oceania. The most interesting case is the data identified as from the "Polynesian Outlier" of Santa Isabel, reported (fig. 1) in the original Skoglund *et al.* (2016) paper and repeated in the Posth *et al.* paper. Isabel people speak North-West Solomonic languages and are culturally like their neighbours in the Western Solomons. They are not Polynesians. In the PCA, these samples are quite variable and plot away from the Solomons and towards the Polynesian Outlier group. I would guess that descendants of people from Ontong Java, known to have been marooned and captured in the Western Solomons in the nineteenth century, have been sampled. Similarly, geneticists should be aware that early missionaries brought Polynesians to serve in the early mission stations in the Solomons and Bismarcks. The Tolai were missionised by George Brown, who brought Fijians to New Britain, and I would hazard a guess that these "Fijians" were from Lau and genetically Tongan. Postulated ties between the Bismarcks and Tonga (Lipson *et al.*: 6) might relate to a very recent "migration". The Santa Isabel error itself does not, of course, alter the result regarding Lapita, but it raises questions about the degree of variation reported in the other populations.

The Isabel data seem highly variable compared to other samples. The ISEA grouping includes samples (based on Skoglund *et al.* 2016) from Taiwan through the Philippines into Borneo and Sulawesi, an area of complex history promoting admixture, yet despite that the grouping remains pretty tight. The Bismarck samples, from a very small area, seem to have a similar degree of variation. Why is this? Many of the comparatively ancient Remote Oceania samples plot in with modern Bismarcks, yet others plot off isolated, in a trend paralleling the modern samples. Perhaps this variation can be easily explained and may be an artefact of the particular form of data reduction and presentation. If population variation is a function of admixture, founder effect and sampling, then the picture seen here seems confusing. I find it hard to understand why some population samples are more or less variable than others.

Linkages of language and biology in the past are either assumptions or hypotheses. Unfortunately, as hypotheses they are virtually untestable and so fairly useless. Neither archaeologists nor geneticists dig up languages. In the abstract of the Posth *et al.* paper, we see "Austronesian-speaking Lapita culture". How do we know? The Lipson *et al.* paper generally avoids these assumptions. More important than these language games are attempts to understand the settlement process and the factors underlying it. It is here that knowledge of genetic structure (e.g. tightly defined versus more variable populations) in the process could be of some help.

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