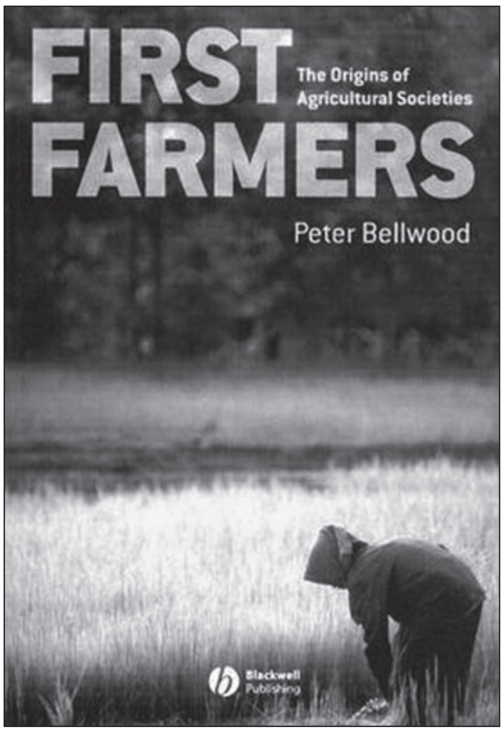
Review Feature

First Farmers: the Origins of Agricultural Societies
by Peter Bellwood

Malden (MA): Blackwell, 2005; ISBN 0-631-20565-9 hardback £60; ISBN 0-631-20566-7 paperback £17.99, xix+360 pp., 59 figs., 3 tables



There can be no doubt that Peter Bellwood's First Farmers *is a major new statement which presents a robustly expressed solution to one of those classic problems which provides a benchmark for theorization and justifies archaeology as a field. But agreement stops there. Few academic books published recently have evoked such highly charged reactions. On the one hand,* First Farmers *has impressed many critics, reached audiences far afield from traditional archaeological readerships, and garnered major book awards from professional bodies such as the Society for American Archaeology. On the other hand, it has been subjected to a level of concerted criticism rare in the academic world. As the reviews below show, it has clearly hit a nerve; the gloves are off.*

First Farmers *polarizes scholars in complex ways. Much recent work on agricultural origins, particularly in Europe, has had a strongly indigenist and particularistic tone, averse to mass movements of peoples and 'grand narratives' in general. But even advocates of grand narrative in general may take exception to Bellwood's 'language dispersals' thesis. Similarly, the very attempt to bring together linguistic, genetic and archaeological data in an account of the past is controversial to some, but even those who aspire to this kind of interdisciplinary synthesis rarely agree on how it can be carried out.*

Neither the book nor its critics here are likely to be the last word on the subject. But whether one agrees with it or not, First Farmers *is a welcome addition to the agricultural origins scene, which, at least in Europe, has been evolving over the last two decades towards a sort of eclectic middle-ground consensus in which difference of opinion is accommodated by eschewing bold generalization.*

Cambridge Archaeological Journal 17:1, 87–109
doi:10.1017/S0959774307000078 Printed in the United Kingdom.

Overview

Peter Bellwood

First Farmers contains my interpretation of the impact of food production on the world-wide distribution of human variation — archaeological, linguistic and biological — as visible at the regional appearances of historical records and prior to the massive population relocations of the past 500 years. It is a book about history in the broad sense, inclusive of prehistory. It contains a central theory about the relationships between increasing dependence upon food production, human demography and population dispersal, with farming cultures and languages in train. I have tried to evaluate data from archaeology, linguistics and genetics as three independent lines of evidence focused on a number of central questions: why did farming societies come into existence in prehistory; where, when and how did they originate, in geographical terms; and by which mechanisms and in which directions did they spread?

Since I am an archaeologist, I devote the greater part of the book to the archaeological record, focusing on those regions for which the archaeological, botanical and zoological disciplines suggest that food production arose independently from a hunter-gatherer background. We might quibble about exactly which regions should be included. For instance, were sub-Saharan Africa, India and the Amazon Basin independent foci of early farming or did they receive the knowledge by secondary processes? I am currently unsure. But whatever the full list, I infer that increasing dependence on food production led to upward trends in population density, ultimately resulting in population dispersal. The book thus deals more with the consequences than with the origins of agriculture. I ask what happened, as early farmers came to depend more and more upon agriculture and animal husbandry, in a world that was still very much the preserve of hunter-gatherers and still unravaged by the high population density diseases of more recent history.

It should be apparent that I regard agricultural dependence as a significant development in cultural evolution, and not just as a way of life that overlapped throughout prehistory with foraging. Nevertheless, agricultural dependence was not necessarily an instant result of agricultural origin, however that transition might be defined, and in some cases the two might have been separated by several millennia, as in the Natufian to Middle PPNB trajectory in the Levant. In addition, I infer that most early Holocene foragers would only have adopted agriculture under favourable demographic, social and environmental circumstances. After all, successful shifts into food production by foragers in history or ethnography, such that the erstwhile foragers have since maintained their cultural and linguistic independence, are virtually non-existent. Australians and Californians alike attest to this. Why give up hunting and gathering to take up farming? My book asks and attempts to answer this question.

I certainly do not deny that foragers took up farming in relatively independent ways from time to time during prehistory, but I see no evidence that this happened frequently. In my view, farming spread with farmers, and on some occasions spread to those foragers who were in direct interaction with farmers, on a much greater scale than it spread through unilateral adoption by foragers with no farmer presence in the vicinity.

Unfortunately, archaeology is a discipline poorly situated to pronounce directly on issues of human population dispersal, hence the very high levels of contentious debate that have encircled this theme in recent years. Archaeologists recover material culture, evidence of past economies, and the bulk of the world's ancient skeletal data base. But unless they focus on the latter they do not have a direct window on the actual humans, as opposed to their cultural creations. Furthermore, archaeologists often work with extremely thin and taphonomically biased data, particularly for those periods of time when food production was in initial stages of development. For instance, those of us who work in equatorial and tropical Island Southeast Asia are extremely fond of excavating caves and rock shelters — sometimes there is little else in the landscape that can be located. But such sites often concentrate in uplifted coral or limestone karst terrain far from regions of agricultural fertility. They tell us nothing about the origins of farming societies in lowland alluvial plains, and sometimes the records probably relate less to farmers than to the continuing hunters and gatherers who might have continued side by side with farmers, borrowing much of the culture and technology of the latter but not necessarily the farming.

In this regard, recent deep excavations in alluvial landscapes, aided by massive construction of multistorey building foundations or freeways, can revolutionize our knowledge of early farming archaeology. This has happened in Taiwan and the US Southwest, as I note in my book (pp. 134, 172), but such massive concentrations of digging equipment right beneath the noses of consulting or university archaeologists occur only rarely in developing countries. For instance, what kinds of waterlogged data bases lie on buried levées or valley flanks deep beneath the plains of the lower courses of the world's great rivers — the Mekong, the Indus, the Ganga, even Mesopotamia and the Nile? Recent discoveries in the deeply buried and waterlogged rice and millet bearing Neolithic sites (*c*. 3000 BC) at Nanguanli, in Taiwan, certainly brought home to me just how much we do not know about the history of agriculture in both southern China and Island Southeast Asia. Likewise, Las Capas did the same for preceramic Arizona. We cannot base conclusions on what we do not know, but we can be cautious about basing them on the minimal data that are sometimes all that we have.

From the archaeological record, I turn to the history of the major language families, as reconstructed by comparative linguists. The major language families of the world had all (if we exclude the Colonial Era) attained their geographical limits before history began. Alexander the Great had precious little enduring impact on the overall distribution of the Indo-European languages, and the Achaemenid Persians and Romans fared little better. Nor would the British have achieved very much in a linguistic sense if they had missed North America and Australasia. Language families spread, on the whole and as long-term vernaculars, with their speakers, and not on any large scale by language shift. Again, history is my guide. Imperial conquest and charismatic religions did not spread vernaculars through large populations. The Mongols spread no lasting linguistic heritage. The great majority of the world's Muslims do not speak Arabic, the majority of Christians do not speak languages that descend from ancient Hebrew or Latin, Hindus and Buddhists in Southeast Asia do not speak Indic languages derived from Sanskrit, and most citizens of most countries within the former British Empire do not speak English as their first language (the majority do not speak it at all — those who do not believe this should check the linguistic situation in India). As Colin Renfrew noted some years ago, if we are to explain the spread of Indo-European languages through Europe we need to look for a widespread transition in the archaeological record during which considerable population and cultural upheaval could have occurred.

Language family homelands, as identified by linguists from subgrouping criteria, also have an uncanny habit of clustering around the same zones chosen by archaeologists for their Neolithic revolutions. The Middle East, China, West Africa, the New Guinea highlands and Mesoamerica all come to mind as clear examples, and this linguistic situation is discussed at length in my book. Of course, those language families associated mainly with hunters and gatherers — Athabaskan, Khoisan, and the much-debated Pama-Nyungan — have other origins. But, for the farmers, is it all pure coincidence? I think not. I am well aware that many archaeologists do not favour any attempt to discuss linguistic prehistory or to search for the origins of language families, and I expect comments along these lines from some of the invited commentators. My position is quite clear. Language is as significant as material culture in human extra-somatic behaviour. Indeed, the patterning of language families might be the clearest non-biological evidence available to us for the Holocene history of ancient populations, farmers and foragers alike.

The final chapter of the book deals with genetics. I am not a qualified geneticist any more than I am a qualified linguist, but I take the view that any general historian of human affairs should be able to understand how geneticists and linguists draw their historical conclusions, without necessarily having to reproduce all the biochemistry, statistics, phonemes and verbal prefixes in their heads. After all, in the final resort we are all historians, merely using different sources of data to draw our conclusions. The histories of NRY and mtDNA lineages might one day be turned into the history of real human beings, but we still have a long way to go, especially given current concerns about the inaccuracy of molecular clock calibrations. All mtDNA and NRY lineages must have mutated into existence in specific places, but turning each one into a homeland for a human population that lived five, ten or more (sometimes many more) millennia ago, using data drawn only from modern populations to do so, requires a considerable leap of faith. Subsequent population movements, genetic drift, natural selection and repeated founder effects in small populations surely mean that the genetics of a population occupying a region now will have only very hazy links with those of a population in the same region 5000 or 10,000 years back in time.

There is of course an answer, at least in theory. Large skeletal series of ancient DNA, demonstrably of early farmers in both putative homelands and regions of immigration, should be compared along similar time levels. Nowhere in the world has this yet been done to a satisfactory level. For example, in order to improve our understanding of the genetic origins of Neolithic Europeans, Neolithic DNA from across the continent should be compared, in statistically valid sample sizes, with both terminal Mesolithic European DNA and Early Neolithic DNA in Anatolia, the

Levant, the Caucasus, and anywhere else that could have served as a homeland region. Likewise, for understanding Southeast Asia and the Oceanic region, Neolithic Philippine, Indonesian and Oceanic Lapita DNA should be compared with Early Neolithic DNA in Taiwan, as well as with regional samples of terminal Palaeolithic DNA. No such large-scale comparisons have yet been made and, until they are, I will be unwilling to take seriously periodic claims that virtually everyone alive today (excluding colonial and postcolonial migrants like me) must have predominantly *in situ* Palaeolithic ancestors. In this regard, demic diffusion, as originally proposed by Luca Cavalli-Sforza and his colleagues, is a logical mechanism for population expansion, involving continuous population growth, fissioning, and intermarriage with other communities. Its workings are strongly favoured in my book, and human history during the past 500 years has seen a remarkable quantity of it. It matters little whether the wave of advance progressed smoothly, or by some forms of leap-frogging and stop-start motion — it is the ultimate progression itself that matters. The early farming dispersal hypothesis does not demand extermination of foragers on the part of early farmers; indeed they would probably have welcomed new members from foraging communities when land still remained more or less a free good, prior to the development of sufficient population density to promote resource competition. Modern populations obviously do not descend in absolute purity from their Neolithic/Formative forebears. Indeed, given the time spans involved since the initial spreads of food production, it should be obvious that the genes of both food producing and indigenous foraging populations will have mixed.

As well as ancient DNA, skeletal anthropology in general can provide significant data about early farming populations, and I acknowledge that excellent information now exists for some regions, such as Neolithic Europe and the Levant. But for most of the world, biological anthropologists do not have access to large skeletal samples that can reliably be dated to the earliest stages of agriculture, as already noted with respect to ancient DNA. This is again particularly true of Southeast Asia, where the record is heavily dominated by a number of well-dated but mainly Bronze and Iron Age samples from Thailand. Initial Neolithic samples from China, Vietnam, Taiwan, Island Southeast Asia and Oceania have not been systematically compared — they may be inaccessible for various reasons, poorly dated, or not yet discovered. Thus, it is not only the archaeological record in much of the world that is full of First Farmer black holes. Genetics and physical anthropology fare little better, and the linguists can only work with those languages that have been recorded or are still living. This is cause not for despair but for demanding a much greater level of cooperation between these disciplines, with practitioners of each becoming familiar with the historical-reasoning processes used by the others.

The final chapter of the book is a comparative survey, dealing with issues such as patterns and tempos of spread, friction with foragers, overshoot (the erstwhile farmers who became foragers), and whether or not one can see common stages in agricultural development. This chapter contains an embarrassing mistake — column 4 of Table 12.1 on p. 276, on rates of spread, should be headed km per year, rather then km per century (so much for proof reading!). While on the matter of tempo, although my tendency is to see agricultural dispersals as punctuational in impact, I am happy to acknowledge that many took a long time to come to fruition in actual years — 4000 for Neolithic cultures to reach New Zealand from Taiwan and the Philippines, for instance, and perhaps 3500 for them to reach Ireland and the Ganga Basin from the Middle East. Yet these are relatively short times in the total span of human evolution, especially when balanced against their considerable impacts. Again, during these millennia-long spreads there was obviously an immense amount of population interaction — can we expect that humans would ever have behaved otherwise? Not all Austronesian speakers came in sealed tubes from Formosa, just as not all Indo-European speakers came in sealed tubes from Anatolia.

Hence, I agree fully that biological populations, languages and cultures need not have evolved through time in absolute unison. Austronesians and Indo-Europeans proclaim this very loudly. But nor are these categories of human variation always completely uncorrelated, as some of the 'creolization' models recently so favoured by archaeologists would have us believe. Investigating history requires an understanding of how these three sources of data can be used in a supportive way — not by circular reasoning, but by understanding how one can draw separate lines of historical information and then compare them.

The future? Having written this book, I now have to keep up with the deluge of relevant material that is continuously being published. I cannot claim to have read anything in the two years since *First Farmers* was published that totally demolishes the early farming dispersal hypothesis for any region of the world. My own archaeological research and that of my colleagues in Taiwan and the northern Philippines has filled in many precise details about the movement of

Formosan Neolithic populations to the Batanes Islands and Luzon 4000 years ago. This was the first stage in Malayo-Polynesian (Austronesian) migration towards the Pacific, as so clearly expressed in the archaeological and linguistic records. Food production certainly *allowed* this migration to occur, although I have no wish to claim that it was the only or the final cause for all stages of movement. The combinations of actual social, economic and technological causes of any specific episode of early farmer population movement must have been very varied indeed. Our ancestors rarely fail to impress us when we finally come to understand their achievements.

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No Neolithic Revolution

Clive Gamble

Forty years ago, Cambridge undergraduate exam papers asked the following question: '"The Neolithic revolution was neither Neolithic nor was it a revolution". Discuss' (Sherratt 1997, 271). The question deliberately challenged the framework for prehistory erected by Elliot Smith (1934) and Childe (1935) that was followed up by Braidwood (1960) and others. The challenge, led by Higgs & Jarman (1969, 40), took as its central proposition 'the unwarranted assumption that all men' in the Palaeolithic 'were then hunter/gatherers'. They argued instead for a continuum of economic behaviour from predation to factory farming. They outraged many by prioritizing the recovery and analysis of bones and seeds over pots and stones.

Now, thirty years after Higgs's death in 1976, the 'unwarranted assumption' they questioned has been largely forgotten, the status of the Neolithic Revolution restored. Peter Bellwood's global study illustrates why the status-quo has had such pulling-power over the archaeological imagination for over 70 years, but also why it is an unsatisfactory reading of the data. He conducts, in commendable detail and scope, a comparative study of the *early farming hypothesis* (EFH) through a triangulation of archaeology, historical linguistics and genetics. The EFH postulates that 'the spreads of early farming lifestyles were often correlated with prehistoric episodes of human population and language dispersal from agricultural homelands' (*First Farmers*, p. 2). The mark of these dispersals can still be detected through Bellwood's three lines of evidence.

Why do I find the postulates and the approach so unsatisfying? Many others, including the Society for American Archaeology who awarded this book its prestigious Book Award, obviously do not, and students, I am sure, will be relying on this synthesis for years to come. However, this is a story archaeologists want to hear because they have been trained to listen only for this familiar version of the past. Consequently, new interdisciplinary opportunities, that here involves genetics, are bent to the master narrative of prehistory, that the world begins with farming. Alternatives are not explored and archaeologists readily fall into the trap, even though warned by Lewis Binford (1981) to tread carefully, of turning the past into a mythic version of the present.

My dissatisfaction with this narrative is not that of an ex-Higgs student reading Bellwood's brief dismissal of continuity in economic life (pp. 24–8). It is instead his bigger picture of what the past looks like (p. 278) that puzzles me.

> The overall shape of the past is here regarded as one of dispersal-based pulsation at intervals, with reticulation in the periods (often extremely long periods) between. We cannot expect that the results of all past dispersals will be unambiguously obvious in present-day linguistic and biological patterns. But the major ones should be.

His reliance on historical linguistics to test his hypothesis gives him a time frame of only the last 10,000 years so that the 'overall shape of the past' comes down to the observation (p. 193) that

> since much of the world is divided amongst quite a small number of major language families, recent human prehistory is perhaps to be written very much in terms of a small number of equivalent massive continent-wide dispersals of population.

So there is to be no place in the EFH for anything older than 10,000 years. Such a shallow time depth is driven by historical linguistics because earlier vocabularies do not, apparently, exist. Such a short chronology is supposedly confirmed by the rarity of any domestic crops and animals before this date and the genetic evidence on human populations can apparently be interpreted as Neolithic dispersals sloshing around the molecular pool. All of this confirms that the world, or at least the one Bellwood is interested in, began with agriculture. All subsequent diversity, cultural, cognitive and biological, stemmed from that late revolution.

https://doi.org/10.1017/S0959774307000078 Published online by Cambridge University Press

Now, to my mind, this is akin to saying that Australia began when Captain Cook dropped anchor or, as the National Curriculum for schools in the UK would have it, that British history started in 1066. Elliot Smith and Childe would enthusiastically have endorsed Bellwood's view and, indeed, they could have written the puffs on the back cover of *First Farmers* supplied by their modern counterparts, Diamond and Renfrew. The importance of the Neolithic Revolution is apparently confirmed and there is no need to muddy the waters by looking any deeper into the well of global prehistory. The shape of the past that flows from the Neolithic Revolution is therefore one of a few great migrations bringing benefits to the world at large. The allegory is of imperial expansion by which the benefits of metropolitan civilization are imposed on a subjugated and increasingly dependant global hinterland.

If you think this is too strong a reading of Bellwood's position turn to his p. 65 and examine the section on 'The real turning point in the Neolithic Revolution'. Here we are told it is the 'results of inexorable human and animal population growth' that fuel the transition to agriculture and lay the foundations for population dispersal. How reminiscent of Childe's (1942) Neolithic Revolution, that drew on the analogy of the Industrial Revolution, and where it was the 'upward kink in the population graph' (Childe 1958, 71) that explained change. Unchecked population then fuels Bellwood's 'full-steam ahead' (p. 67) dispersals from centres of origin followed by diversification.

However, Bellwood pulls back from the global colonial model when it comes to explaining local sequences. While he seeks to apply the EFH globally he nonetheless concludes (p. 25) that

> any overall explanation for a trend as complex as the transition to agriculture must be 'layered in time', meaning that different causative factors will have occurred in sequential and reinforcing ways.

I read this as special pleading on a case-by-case basis for the EFH. Such a rubber-band of a concept can be wrapped round any evidence to contain the desired result. The elasticity made me think of Braidwood's (1960) 'culture was not yet ready' model to explain agriculture that was famously challenged by Binford (1968). It is, however, still in vogue (Watkins 2004) as a blanket to throw over the fire of contradictory evidence.

So let me unpack Bellwood's 'shape of the past' some more. Critical to all three arms of the EFH is the concept of homeland. These homelands refer to language, people and the availability of domesticates to domesticate. Homelands have a star-burst quality followed by spread-zones in that follow up, 'full steam-ahead', phase. This terminology owes much, although unreferenced, to Vavilov and his school that studied plant distributions and inferred centres of origin for domestic species. As discussed by David Harris (1996, 5–7), Vavilov's centres of varietal diversity among plants were coterminous for him with the home of primeval agriculture. This was picked up by Sauer in his use of geographical hearths and later by Harlan and Hawkes's nuclear centres of agriculture and civilization. While Bellwood's EFH is perhaps more allied to the modifications of Vavilov's position by his successor Zhukovsky, its intellectual dependance on the geographical approach of Sauer (1952) is very apparent.

But as Harris points out, Vavilov's equation of centres of crop diversity with centres of agriculture was ill-founded and has obstructed our understanding of what agriculture is and how it arose. The static colonial model of active centres and passive hinterlands needs to be replaced by more complex social and economic networks that arise from entangled histories. Homelands, hearths and centres of origin all see the shape of the past as a few innovative hotspots, tightly circumscribed; but, as Harris (1996, 6) ruefully comments, 'students of animal domestication have benefitted by not having a "Vavilov" to define "centres" for them!'.

Harris's point is borne out by Melinda Zeder and her colleagues (2006) in a recent review of the archaeology and genetics of domestication, where the evidence points to a more complex pattern than homelands. Their review, moreover, provides an example of how archaeology and genetics can work together without once having to resort to speculations about the Proto-Nostratic word for a goat. The genetic evidence, as they show, now points strongly to a variety of geographical and temporal patterns for animal and plant domestication. There are single sites in small geographical areas but there are also, and in particular among animals, multiple domestication events. The pig is a classic case. Across its huge 'wild' range it has been domesticated independently at least six times, according to genetic evidence, and four on the archaeology. Most striking is the genetic information on much earlier dispersals of domestic forms that Zeder *et al*. (2006, 139) define as a unique form of mutualism. It was thought that the African bottle-gourd reached the Americas as flotsam but now its phylogeography shows that it came overland from Asia, along with the dog, in the Late Pleistocene. Some of the earliest inhabitants of the New World already had a mutual relationship with elements of their environment that had strong selective advantages for both partners. While this would gladden Higgs and support his 'unwarranted assumption' mentioned above, its real impact is to argue for a very different model than homelands and star-bursts. A model of these diasporic tracks as networks of relationships and resources, each of which has a distinctive biography, is ultimately more satisfying than an arrow launched from a homeland.

An example of such networks also comes from Zeder's review. The 'first global economy' links eastern Africa with India through genetic and archaeological evidence. The track is reminiscent of the southern coastal route that many have commented upon as a key ecological zone for any dispersing species (Kingdon 1993; Lahr & Foley 1994; Macaulay *et al*. 2005). This Indian Ocean corridor or track involved overland movement as well as transport by ship. The genetic evidence indicates that zebu cattle, banana, yams and taro were all introduced into Africa while sorgum, millet and donkeys went the other way. Again, rather than homelands or centres, these were networks that set up those unique mutualistic relationships and then, under social and environmental selection, incorporated and moved the elements in a variety of directions.

With so many key domesticates linking India and eastern Africa, the genetic evidence provides an opportunity to test the EFH. However, when we look at the language maps and the dispersal arrows that Bellwood draws from his homelands (figs. 1.3, 4.7), they are all pointing in the opposite direction from this Indian Ocean network of exchange and innovation. The same mis-match is also the case for the Amazonian origins of cassava, rated the sixth most important crop in the world. It seems likely to have been a single domestication event. Archaeological evidence in the form of fossilized starch granules from stone tools indicates the age and dispersal direction. Most importantly, this led north into Panama and the Caribbean. The lingusitic families, however (fig. 10.10), show everything heading south.

What is happening here reminds me of Thor Heyerdhal (1950) and his belief that common sense could explain archaeological evidence. He set out to prove that Polynesia was peopled from the east since this was the direction of the prevailing currents. While he courageously showed it could be done, all the archaeological, and now genetic, evidence indicates that common sense alone is not an explanation. Polynesia was colonized against the currents and the winds (Irwin 1992).

Bellwood (p. 24) is also a believer in common sense when it comes to the benefits and desirability of agriculture. Common sense regards the Neolithic Revolution as the correct origin point for the present (Gamble 2007). Common sense expects language, genes and culture to fit together to show this was the case. However, the archaeology and genetics of domestication show that they clearly do not, and the balsa-wood raft that is the EFH is already leaking long before it has been pushed out from the shore. Indeed, as Bellwood (p. 144) shows in one of his own research areas, Papua New Guinea, the EFH never left harbour. The origins of agricultural societies requires a better explanation than this if, as the author hopes, it is to provide our world with 'a story to bring people together'.

Instead, what the genetics and archaeology are showing is how, at all times and places, and long before the Neolithic, the scaffold of social networks has allowed people to interpret the world and appropriate the resources relevant to that interpretation. The answer to that Cambridge exam question remains the same, although the argument and evidence has altered.

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Mesoamerica and the Southwest

Steven A. Le Blanc

At first appearances, *First Farmers* is a grand summary of what we know about the origins of agriculture. In fact, it is a masterful evaluation of a series of interlinked hypotheses which have been developed or initiated largely as a result of the work of Peter Bellwood.

That farming spread primarily by the expansion of farmers and not the adoption of domesticates by local populations, and that foragers are unlikely to convert to farming except under special conditions, is contrary to much previous thinking on the issue. It remains to be seen whether this model survives the test of time, and certainly the full story will turn out to be more complex than this; but that is beside the point. An important aspect of the over-all farmer

https://doi.org/10.1017/S0959774307000078 Published online by Cambridge University Press

expansion model is that it can be tested in multiple, independent cases. If correct, the model provides not only a general proposition about how farming spread but also one about how foragers adapt. Moreover, the model would require that much of the world's population genetically derives from fewer sources than has traditionally been assumed, and so there would be less genetic diversity than one would otherwise presume. As geneticists have come to realize that there *is* less genetic diversity among humans than theory would predict, perhaps the farmer expansion model will help shed light on the issue.

This model, or complex set of hypotheses, is just the kind of general proposition that the New Archaeology said should be a goal of archaeology. The farmer expansion model is general in its application, with examples from multiple continents, and explains a great deal of behaviour that otherwise might appear idiosyncratic. Interestingly, the farmer-expansion model is also an excellent example of the benefits of what used to be called the 'four field approach' in American anthropology. It uses data and interpretive frameworks from linguistics, archaeology, biological anthropology and cultural anthropology. The failure to consider all of these disciplines as closely related and needing to be taught under one umbrella was a criticism the American anthropologists levelled at their European counterparts. Ironically, the four field approach is dying out in the United States, and it is Bellwood, coming from the European tradition, who now demonstrates its power and usefulness.

In today's increasingly specialized world, it is getting harder and harder for one person to command these various disciplines well enough either to develop such a broad model or to pull together and evaluate the information on one. While it is the case that Bellwood has done a masterful job of pulling these diverse data together, a major issue is whether the researchers in each relevant geographical region and relevant sub-discipline are able to further test and evaluate the model, much less extend it or modify it if that turns out to be appropriate. While *First Farmers* is a comprehensive explication and defence of the farmer-expansion model, it has been preceded by considerable work by Bellwood. It is this earlier work that has already impacted on thinking by many Americanist archaeologists.

Of the various farmer expansions that have been proposed, the one involving Uto-Aztecan speakers of Mesoamerica spreading to northern Mexico and then to the American Southwest has been developed most recently and serves as a good example of Bellwood's critical role in the process. It is also the one I am most directly involved with. I have worked in the Southwest for over 30 years and looked at the distribution map of Uto-Aztecan speakers many, many times. I have been a fan of the concept of farmer expansion since the publications of Renfrew's Indo-European and Bellwood's Austronesian spread hypotheses. It is quite embarrassing that it never occurred to me that Uto-Aztecan might be another example. While I am generally of the mind that it is more important to figure out 'where to go from here' rather than figure out 'how did we get here', it is worth looking at the genesis of this new paradigm for New World agriculture.

The idea of farmer expansions and language family spreads was 'in the air' in the 1980s, as Bellwood has said. Various Southwestern archaeologists had proposed parts of the overall model around that time (e.g. Berry 1982). Most importantly, Matson (1991) was beginning to put the pieces together and Mabry (1998) was finding unexpected early farmer villages in southern Arizona. Both were questioning whether the early Southwestern farmers may have been Uto-Aztecan speakers. Quite independently of them, Peter Bellwood was asking why the Uto-Aztecan language distribution was not being seriously considered as an example of farmer-expansion. The first time he put forth the idea in print was 1994 (Bellwood 1994). A more elegant rendering of the model was delivered at a seminar in 1998 (Bellwood 2001), and one was published a bit later (Bellwood 2000b). A lecture by Bellwood stimulated Jane Hill to revisit the locus of the origins of Uto-Aztecan. At almost the same time, the dictionary of Hopi — a northern Uto-Aztecan language — became available (Hill *et al*. 1998) which gave her the wherewithal to re-evaluate these origins and argue for a southern — Mesoamerican — origin of the language family (Hill 2001). Prior to this, it had been widely accepted that Uto-Aztecan originated far to the north of Mesoamerica (an idea still held by some). Jane Hill's work gave Matson the piece he was missing (Bellwood's early papers not being in the literature domain of Southwestern archaeologists) and he and Bellwood seem to have converged on the present Uto-Aztecan speaking farmer expansion model independently.

This possibility sparked interest on the hypothesized farmer expansion at the same time as new data and new interpretations of old data were coming out. This fortunate confluence of events has resulted in a better evaluation of the data regardless of whether the hypothesis is correct or not. Virtually all the new data have come from the northern portion of the Uto-Aztecan range. The presence of large concentrations of early farmers in such areas as Tucson (Mabry 1998), La Playa, Sonora (Carpenter *et al*. 1999) and Chihuahua (Hard & Roney 1999) was virtually unknown 10 years ago, although there was the hint of all this earlier (Huckell & Huckell 1984).

In a further coincidence, there has been a reconsideration of the earliest farmers at the northern extremity of the Southwest, lead principally by R.G. Matson (1991) but also F.E. Smiley (Smiley & Robins 1995). The new realization is that the Western and Eastern Basketmakers were culturally very different. This has become a component of what might be called the Bellwood-Matson model, and it is proposed that the Western Basketmakers were candidates for being Uto-Aztecan speaking migrant farmers while the Eastern Basketmakers were indigenous foragers who converted to farming. This would make the Eastern Basketmakers equivalent to the Basques in Europe. The Bellwood-Matson model provides a fascinating point of departure for model building and testing. To date, new studies on cordage, burials, tooth genetics, DNA, and especially linguistics have been directly prompted by the hypothesis. The current evidence from various disciplines has been recently summarized (Matson 2003; Le Blanc in press). At this point, the evidence is compelling but not overwhelming. Nevertheless, it certainly has helped cast the discussions of the adoption of agriculture in the American Southwest and northern Mexico in a framework quite different from even ten years ago. This may go down as one of the fastest paradigm shifts in the history of archaeology. Moreover, the Uto-Aztecan case now has enough evidence behind it to require it to be considered in ways comparable to Indo-European, Austronesian, or Niger-Kordofanian language spreads. How fast this small seed has grown, how beneficial different perspectives have been, and how key was Bellwood's role.

If the farmer expansion model is correct, how would it have worked on the ground? Understanding this should provide insights into the process of invention and adaptation, as the founding groups would have been constantly faced with new environments, landscapes and neighbours. My own perception is that the movement of farmers would have involved competition with resident foragers and, because farmer growth rates would have been very high, soon there would also have been competition with neighbouring farmers. This competition would have kept group sizes large and set the stage for rapid increases in social complexity. Also, warfare would have resulted in forager females being incorporated into the farmer gene pool much more commonly than males. If correct, all this should be reflected in the archaeology and in modern and ancient DNA. Thus, the initial model results in a series of cascading derivative models. These should provide the means to test, refine and extend the fundamental premise.

I believe, right or wrong, the farmer-expansion model will rank with the major ideas that have impacted on the thinking of archaeologists. It is exciting to be involved in some small way with the evaluation of Bellwood's important contribution.

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Europe

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In a recent report on a multi-disciplinary conference exploring East Asian population histories, Bellwood & Sanchez-Mazas (2005, 483) concluded that such conferences

> present data on a scale that can often seem overwhelming, particularly to social scientists not versed in the complexities of population genetics and vice versa. The past has been complex in the extreme, and to recover all this complexity event by event would be impossible. *We need to propose overarching hypotheses that can account for the comparative data from linguistics, genetics, and archaeology with as little stress as possible*. [added emphasis]

It is one's attitude towards the value of any such 'overarching hypotheses', in contrast to detailed regional interpretations, which is likely to determine one's primary response to Peter Bellwood's impressive and generally balanced book. Do these attitudes have to be presented as alternatives? Probably not, but questions of scale, complexity, and resolution, and how one might deal with comparative (but not necessarily comparable) data, are at the heart of the issues under discussion here. The quote above implies that describing or explaining 'process' is preferable to an illusory search for 'events'; but one person's (or narrative's) 'event' is another's process (Knapp 1992; Pluciennik 1999). Understanding 'process' and 'history' are overlapping but different aims. To set out my own stall: my doubts in general about 'overarching hypotheses' — especially those that promise 'as little

https://doi.org/10.1017/S0959774307000078 Published online by Cambridge University Press

stress as possible' — can be summarized as: the past was not only more complicated but is consequently more challenging and rewarding to study. Both generalizations and particularisms include and exclude relevant material; and of course no explanation can be total. To use Alison Wylie's words, we need to tack back and forth between not only the past and the present, theory and data, but also this scale and that, the general and the particular, and between data sets which, in my view, do not represent or refer to exactly or entirely the same entities, events or processes. Unsurprisingly, the interpretations and explanations one offers, and finds compelling, depend on one's questions and interests.

Bellwood's basic argument is that genetic, linguistic and archaeological-cultural landscapes show sufficient over-lap or convergence to strongly suggest that the primary factor determining such distributions was the demographic expansions associated with early farming populations, carrying particular genetic lineages and languages with them. Such arguments have now been rehearsed for almost two decades, initially in relation to archaeology and language (Bellwood *et al*. 1995; Renfrew 1987), archaeology and genetics (Ammerman & Cavalli-Sforza 1984) and, more recently, all three (Renfrew & Boyle 2000; Bellwood & Renfrew 2002). Unlike many of those earlier publications, Bellwood spends much of the text discussing the archaeology, rather than looking at possible linguistic or genetic patterns (only 'possible' because the methods and models for producing trees, clusters or other representations of both current relationships and past histories is a matter for much debate and with highly variable outcomes) and attempting to fit the archaeology around them. In many ways, the book is a *tour de force* as a global overview and, as an out-line of the archaeological issues, it is an excellent introduction. I would also commend his later chapters on genetics and language as introductory reviews, though I would disagree with the relative weights and constructions we should put on much of these data.

The regional surveys of the archaeology are preceded by discussion of hunter-gatherer ethnography and demography, in which Bellwood argues that it is hard to see forager adoption (or multiple independent invention) as the major mechanism for spread once agriculture or an agricultural package has come into being in any particular region. Bellwood is at pains to note that recent ethnographies and histories may have their limitations as processual analogies; but, driven by his own hypothesis, those analogies drawn from ethnographies necessarily produced within a globalized colonial and imperial milieu, like farming, always win the argument. For example: in Europe, the area which I know best, Bellwood summarizes the evidence, not unfairly though naturally emphasizing the fact that the end result was farming; but, in many areas, the actual or potential presence and contribution of existing foragers is played down. For example, he asserts that 'it is clear' that the transition in northern and eastern Europe was 'essentially driven by an LBK-TRB cultural phylogeny' (p. 80), though many would question this, especially in the light of recent work in Poland and elsewhere (e.g. Nowak 2001), which suggests that forager practice and world-view was crucial in the formation of the TRB. Bellwood's conclusion sits awkwardly after his admission of so much variability:

> Just observing that some Mesolithic populations probably became incorporated into a Neolithic cultural landscape tells us nothing very useful at all. What we want to know is what really *drove* the Neolithic expansion, a question for which the activities of those Mesolithic hunters who happened to be in the vicinity of the action may not have been terribly relevant (p. 81).

This seems both to beg the question and to be a gross misrepresentation of the multiple processes of socio-cultural and economic change in northern and eastern Europe at the very least, where the transition to farming took several thousand years — from the inception of the Neolithic until the later Bronze Age, if we want to use those terms. Many of us do precisely want to 'know' about socio-cultural hybridity and socio-economic transformations — about history, as well as process.

There could be no clearer demonstration of the unsubtlety of this kind of scale of analysis, and the real 'streamroller effect' — not of Neolithic expansion, but rather of this farming/genetic/linguistic hypothesis, which flattens past cultural landscapes, processes and differences in its wake. This is a classic example of 'agricultural thinking' (Gamble *et al*. 2005), which I would see as yet another manifestation of the bad side of social evolutionary thought and the associated dismissal of hunter-gatherers (Pluciennik 2005). There are difficulties with this approach on several levels. It is worth emphasizing that nearly all the archaeological evidence we have suggests that the spread of agriculture in Europe was regionally variable in space, time and tempo. These differences suggest mixtures of pioneer colonization (by exogenous farmers and by foragers-turned-farmers), demic expansion, partial and wholesale adoption, cultural hybridization, emulation and so forth (e.g. Forenbaher & Miracle 2005; Gronenborn 1999; Jeunesse 2003; Nowak 2001;

Tringham 2000; Zilhão 2001; Zvelebil 1998). In much of northern and especially northeastern Europe, we may talk about inter-community relations — if we want to insist on always polarizing these as either 'foragers' or 'farmers' — lasting and changing over anything from one to three millennia. In much of southern and western Europe, we are also looking at co-existence and change, typically over at least half a millennium — 500 years of socio-cultural history which is surely of interest in itself and not just for what it leads to, from one particular perspective of subsistence. Some, including myself, would argue that, under these circumstances, it is not necessarily productive always to address these historical issues in terms of the 'transition to farming' at all, let alone to characterize them as a single process, a 'driven' Neolithic expansion.

Even within the classic European culture linked with demic expansion, the LBK, there are many subtleties of process, and I would argue often more interesting ones than those described within the demic expansion strait-jacket. Beyond the references given above, we could note that recent analysis of skeletal morphometrics (Pinhasi pers. comm.) on three nearby LBK sites suggests three very different populations biologically speaking, but with very similar material culture expression. What is that telling us about cultural and biological process? Similarly, work by Bentley *et al*. (2003) suggests not only regional but also inter- (and intra-) site variability, that is, the outcome of different socio-cultural processes over space and time. Despite all the interpretive and methodological controversies, confusions, misapplications and misunderstandings associated with genetic data (Bandelt *et al*. 2002; MacEachern 2000; Zvelebil 2002), the exogenous 'Neolithic' genetic lineages represent perhaps around 20 per cent of the modern European gene pool (Richards 2003), though even if we went with Chikhi (2002; Dupanloup *et al*. 2004) and a 50 per cent average 'contribution' across Europe, I (and probably most of my European colleagues) would still find the roles of Mesolithic hunter-gatherer communities 'terribly relevant' over most if not all of the continent. In brief, archaeology in Europe tells us that the transition to farming — if (and it is a big 'if') we want to view the last eight millennia or so through that particular lens — was a very complicated and long-drawn out affair. Given the recent relative convergence of archaeological and genetic data in suggesting all kinds of processes of 'admixture' (an unhappy term for culture in that it suggests the blending of fixed elements), I find, *contra* Bellwood, the use of comparative linguistics as a way of producing historical phylogeographies to bolster the main hypothesis, largely a red herring (cf. Gamble *et al*. 2005, 210). This is mainly because of the uncertainties of dating and the insistence on a single demographic and linguistic process. Language change can and plausibly could happen without demographic and genetic swamping. Linguistic methodologies seem to offer little chance of ever achieving the required chronological resolution, and with the additional problem of the equifinality of multiple processes. Despite Bellwood's arguments, I would suggest that versions and indeed mixtures of modes of linguistic shift including contact-induced language change (e.g. for Amazonia: Hornborg 2005; for Europe: Zvelebil 2004) are potentially highly relevant, and would emphasize the existence of non-analogue processes. Contemporary ethnography can only ever be a guide, and sometimes a misleading one, if we are considering contextual historical processes. Specialists in each area and discipline will provide and have provided similar caveats (for example: Golla *et al*. 2003; Fuller 2003; Anderson 2003; Gronenborn in press).

Clearly, Bellwood's own position is that the global scale is required as a framework and that, at such a level, similar or comparable demographic processes are responsible for many of the apparent patterns in archaeology, language and genetic data. Crudely, that argument is that early farming tends to be expansionist; and that language families often occupy geographically contiguous areas which bear some relationship to those early 'neolithic' cultures. (At the individual language level, there is relatively little agreement between genes and language distributions — as one might expect for such a mobile cultural attribute: cf. Bandelt *et al*. 2002). However, one person's myopia may be another's 'close reading'. Ideally, we should all be aware of the various contexts for our and others' work and data, including different scales of analysis. There are real challenges in how to use analogy, or rather also in inferring plausible non-analogue situations of socio-cultural (including linguistic and demographic) processes, as well as issues of 'fact' and interpretation. Ethnographic analogies, while rightly informing any archaeologist's approach, will only take us so far. We have to rigorously explore (or hypothesize) difference too. Demographic regimes and demic expansion (which itself can take many different forms) may well have been important parts of agricultural spread. However these were also expressed in many ways with different consequences in different contexts (archaeologically, for instance, in settlement patterns, cultural shift, relations with different communities). It is these historically variable and, in many ways, unique cultural processes which interest many of us as much if not more than the necessarily simplified global picture.

Do, then, different scales necessitate different methodologies? Do the various data sets bear the same relationship to the original entities or processes? This is a difficult series of questions, epistemologically and ontologically. Are we asking the same (or at least related) questions about the same things? Is the farming/language and gene dispersal hypothesis compatible with the 'integrationist' paradigm proposed by Zvelebil (2004, 44) and attractive to many European (and other) archaeologists precisely because of the room it offers for regionally nuanced interpretations? There is clearly over-lap; but, as one of the main protagonists of the farming/genes/language hypothesis (Renfrew) has recently discovered, even at a regional level, the genes apparently — though equally problematically — tell a different story (Haak *et al*. 2005; see also Ammerman *et al*. 2006).

Bellwood is to be congratulated on producing a book which sets out succinctly, clearly and on a global scale the 'overarching hypothesis'. However, unlike Bellwood, in this instance I am all for intellectual stress, and would judge that the cracks have long begun to show.

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Is Genetics Coming Between Archaeology and Linguistics?

Martin Richards

What kind of evidence do you prefer to draw on when trying to reconstruct prehistoric human dispersals? Popular opinion has it that nowadays there are three fairly direct kinds: archaeological, linguistic and genetic (or the biological, more generally). There are other lines of evidence too, such as ecological or palaeoclimatological, but they are less direct. Peter Bellwood's preferences are very obvious. He is an archaeologist, but his great love is linguistics. He has built the edifice of the (now 'modified': p. 270) 'Out of Taiwan' model for the Austronesian dispersal upon it, although he uses archaeology (the most direct line of evidence) to try and back it up. He would like to be able to generalize the model to other language families as well. The problem is the genetics.

Bellwood's preference emerges in a curious contrast between chapters 9 and 11 of *First Farmers*. Chapter 9 is a hymn to the marvels of historical linguistics. Chapter 11, focusing on the biological evidence, is very different in tone. Bellwood makes a valiant attempt to get to grips with the debates going on in human population genetics, evolutionary genetics and 'archaeogenetics'. It is certainly gratifying to see the genetic data discussed rather than simply ignored; but there is not much sense of excitement that genetic data might finally allow us to get a more direct purchase on past human movements and help us to distinguish alternative hypotheses (at least within an archaeological framework). Yet some of us feel that the coming together of archaeology with genetics is likely to be a much more fruitful union than the rather bumpy relationship between archaeology and linguistics — a pessimism fuelled to a considerable extent by the impasse reached in the debates about Indo-European (Gamble *et al*. 2005).

We geneticists are characterized by Bellwood as a fractious lot, and no doubt this is fair comment. But when Bellwood contrasts two opposing 'schools' in human evolutionary genetics, a 'phylogenetic' one, and an opposed (population-genetics) one, he associates the former with cultural diffusion and the latter with demic diffusion. This is really a simplifying move too far. Favouring the 'phylogenetic' approach need not entail an adherence to cultural diffusion, and a more population-based approach does not inevitably lead to advocacy of large-scale migrations. The 'phylogenetic' (or 'phylogeographic') approach has, for example, estimated very high rates of demic expansion into southern Africa, alongside the Bantu languages (Pereira *et al*. 2001; Salas *et al*. 2002), work which Bellwood unfortunately does not describe. There is no intrinsic bias in one or other methodological position, so far as I am aware, that would incline the adherents of one or other approach towards either cultural or demic diffusion. It is true that some on the 'phylogenetic' side have a suspicion of mega-meta-narratives, but others no doubt find them rather appealing.

Moreover, as Bellwood is well aware, we are talking merely about a spectrum of dispersal *versus* acculturation. What was the extent of the role of each? In the case of Europe, the 'phylogenetic' school is towards one end of the spectrum, and the 'population-genetics' school towards the other. We may disagree on whether the dispersals are best described as a 'wave of advance' (or even 'demic diffusion') but we agree that, in Europe at any rate, the Neolithic was transmitted at least in part by movements of people and in part by word of mouth. We all agree too that making any estimates at all is fraught with problems. It may even be that the spread of genetic lineages was not the result of dispersals at all, in the conventional sense, but resulted from changes in socioeconomic arrangements. Dramatic narrative statements such as 'the cultural diffusion school was clearly not going to give in quietly' (p. 258) owe more to populist fantasies like that of Sykes (2001) than to anything that actually took place.

To what extent does the genetic evidence support Bellwood's main thesis? Despite attempts to iron over the problems, Bellwood's text seems a little schizophrenic, as if he has not entirely made up his mind. At one point he cites with approval a remark once made by myself and my colleagues, suggesting that 'grand syntheses … in which farming, languages and genes all expand together, should become a thing of the past' (p. 260) When he refers back to Europe at the end of his discussion of Southeast Asia, he mentions a figure of 20 per cent Neolithic lineages as plausible for both regions (a not unreasonable figure for either case). Yet my suspicion is that he just cannot get the Great Migrations out of his head. Only a few pages further on, we find him coming down firmly on the side of the maximalists in the European demic-diffusion camp, a position which he says has taken on the attribute of 'common sense'. I will not repeat the obvious critique of their position in detail here (Richards 2003); suffice it to remind readers that, if one measures admixture between two populations, it is no more legitimate than with genetic clines to assign all of it to a single process — such as a Neolithic expansion — since there is no dating. But then Bellwood explicitly states (p. 262) that he has chosen the evidence to fit his views. It is more usual in science to try and do things the other way round, but perhaps he is just being more honest than the rest of us.

This uncertainty persists when Bellwood moves on to Southeast Asia and the Pacific. To be fair, I do not envy him having to review the evidence in 2004, when the situation was certainly contradictory and rival interpretations clamoured for attention. Even so, his aim again seems to be not to take the evidence at face value but to reconcile it with preconceived notions (so that we, or at least he, can 'relax': p. 269). The dust has yet to clear even now but I suspect that when it has, in several years' time, there may yet be some surprises in store.

Bellwood re-states his 'two-layer' view that Southeast Asian populations before the advent of the Neolithic were physically 'Australomelanesian', and that they were partly or wholly replaced by 'the Austronesians'. The latter are a mythical people (or linguistic construct), said to have originated in the mid-Holocene farming communities of southern China but only to have started speaking Austronesian languages after moving to Taiwan. From there, they rapidly populated most of Southeast Asia and the Pacific islands. The problem is that recent genetic evidence is hard to reconcile with this picture. In the 1990s, largely ignorant of the archaeological background, most geneticists looked at Southeast Asian and Pacific history through the lens of 'out-of-Taiwan/express train to Polynesia' *versus* 'indigenous Melanesian origins', the latter being inaccurately attributed to John Terrell. It was Stephen Oppenheimer (not a geneticist, or for that matter an archaeologist) who woke us up to a much wider range of models, including those proposed by Wilhelm Solheim (2006; who is nowhere mentioned in Bellwood's book so far as I can see). Some of us came to question the idea (that really still survives in this book despite all the qualifications) of a single monolithic 'Austronesian expansion', and we realized that the mitochondrial DNA (mtDNA) data were in fact compatible with a range of models and perhaps more suggestive of an origin for Pacific islanders (*not* all Austronesian speakers, despite what Bellwood says on p. 269) in eastern Indonesia than in Taiwan (Richards *et al*. 1998). This remains true today, and the Y-chromosome evidence in particular shows little trace of a large-scale Holocene expansion from China to the Pacific (Kayser *et al*. 2000; Capelli *et al*. 2001). Our analysis of the so-called mitochondrial 'Polynesian motif', found at high frequencies in Remote Oceania and at moderate frequencies in coastal Near Oceania, but rarely elsewhere, remains controversial, since we argued that it was more than 5500 years old and therefore unlikely to have been part of a dispersal from Taiwan that arrived in eastern Indonesia around 2000 BC. Bellwood lists three possibilities that might explain the age of the motif in eastern Indonesia.

The first of Bellwood's possibilities is that Asian-derived populations were in eastern Indonesia before the mid Holocene, and that the dispersing Austronesians picked up the motif from them. This is possible but it has a curious feature: it would mean that all of the 'Austronesian' mtDNA lineages had been lost in the dispersing population, to be replaced largely by the 'older Asian-derived' lineages.

His second possibility is that the dating is simply wrong because of the well known difficulties with the molecular clock. We will never know the rate for certain, but internal cross-checking with independently dated events (such as the colonization of previously

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unsettled islands) can help. In fact, we under-estimated the width of the 95 per cent range in 1998; better methods have since been introduced (Saillard *et al*. 2000). Even so, it still seems that our suggestion that the motif is probably more than 5500 years old is likely to be correct, although it almost certainly did not arise before the Holocene. Trejaut and his colleagues, cited in a footnote here as a rejoinder to our estimate (p. 291), estimate the age in Taiwan of the ancestral lineage to be about 13,000 years and the motif itself to be around 9000 years (Trejaut *et al*. 2005). No doubt these estimates will be revised further, but as they stand they provide little solace for an advocate of a mid-Holocene dispersal from Taiwan.

Bellwood's third explanation is that the motif is indigenous to the region, meaning that it evolved within the 'Australomelanesian' population itself. This often seems to be what Bellwood assumes we have claimed but we would rather do away with the hard-and-fast distinction between 'Australomelanesians' and 'Austronesians' altogether. MtDNA and Y-chromosome lineages in island Southeast Asia are distinct from those of Oceania and, to a considerable extent, distinct from those on the mainland as well. Some have been evolving off-shore (or on the Sunda continent) for 50,000 years or more, whereas some appear to have spread from what is now the mainland in the late Pleistocene, and still others in the Holocene (Hill *et al.* 2007). So a sharp 'racial' division in population make-up around 2000 BC seems difficult to sustain.

In the end, it is mainly the distribution of the languages that creates the impression of a Great Migration 4000 years ago. Historical linguistics lends itself naturally to this sort of tale: it is very top-down, whereas both archaeology and genetics in one way or another work up from the individual. As we know from the problems with Indo-European, trying to read history from the present-day distribution of languages can be a fruitless exercise. I may be wrong, but I like to think that the union of archaeology and genetics holds rather more promise.

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The Rudiments of Agriculture and Domestication

John Edward Terrell

My disagreements with Peter Bellwood about the character and processes of human social life are so basic that, while I am happy to tell you about them, I do not think that he and I would get far if we tried to talk together about the origins of agricultural societies. By the same token, if you agree with Bellwood as strongly, say, as Catherine Perlès (2006) does, then it probably would be a waste of your time to read what I have to tell. Before I say anything, however, I want to stress one thing. Alternative ways of looking at how people make a living are good to have. I do not need to convince you that Bellwood is wrong and I am right. I think, nonetheless, that he starts off on the wrong foot, and for me — and others, I hope — there are more exciting ways to think about what people do and why they do (or do not) do so (e.g. Hart 1999; 2001; Terrell *et al*. 2003). Here are three ways in which Peter Bellwood and I evidently part company on how to think about what it is that people do to make a living and how they have ended up doing what they now do.

Heterogeneity

There is little agreement today on the best definitions of foraging and farming as distinct states or stages of human subsistence life. Nobody doubts that people over time, intentionally or unintentionally, have altered some species to such marked degrees that these hapless organisms are no longer viable on their own if they do not receive our care and protection. It is also plain to those who think about such things that, for some species of plants and animals favoured by *Homo sapiens*, signs they have been the focus of a great deal of our attention are, for plants, increasing seed size over time and, for animals, decreasing bone size. A hidden flaw, however, undermines many currently accepted ways of thinking about domestication and the origins of agricultural societies. What is too often overlooked or under-rated is that there are 'all degrees of plant and animal association with man' (Harlan 1992, 64). Hence, one reason why I think Bellwood's account of the origins of agricultural societies falls short may be obvious. He wants us to take typological categories such as *hunter-gatherers* and *farmers* not just as convenient 'tools for thought' (Waddington 1977) but as real people. Yet it has long been recognized that different people use different mixes of what are conventionally seen as farming and foraging behaviours to make

https://doi.org/10.1017/S0959774307000078 Published online by Cambridge University Press

their living. Consequently, trying to sort people into categories as contrastive as 'farmers' and 'foragers' cannot be done except in the realm of make-believe. Archaeologists and students need more realistic ways to deal with the basic down-to-earth heterogeneity of our human ways.

False equifinality

Back in the days when it looked like general systems theory was the road to take in the social sciences to reach profound and career-enhancing discoveries, it was commonplace to observe that 'the same final state may be reached from different initial conditions and in different ways' (von Bertalanffy 1968, 40). Like many others, Bellwood evidently accepts the idea that the beginnings of agriculture in several different places on earth constituted one of the major revolutions in the history of our species — the great divide between Palaeolithic and Neolithic times. It has long been conventional to assert that different people arrived at 'the same final state' (variously labelled as 'domestication', 'agriculture', 'farming' and the like) at different times and places in different ways using different species; but, just as we must accept that different people now and in the past use or used different mixes of farming and foraging to make their living, so too we must accept that different species have responded genetically and otherwise to our use of them in different ways and to differing degrees, ranging from nothing obvious at all (domesticated elephants, for instance) to quite the opposite extreme (e.g. sunflowers and the many breeds of dogs). Thus it takes more than a grain of salt to accept that 'farming' and 'domestication' as a set of behaviours having variable consequences alike lead to a 'same final state' towards which some people have been variously progressing in different ways at different times.

The same conclusion holds at the other end of the spectrum or 'continuum' of societies that fall somewhere between the 'pure farming' and 'pure foraging' (e.g. Bailey & Headland 1991; Smith 2001). As no less a cultural evolutionist than Leslie White remarked, even people normally classified as exclusively hunter-gatherers have always had accurate and abundant knowledge of the flora and fauna in the places where they like to roam, so 'the origin of agriculture was not … the result of an idea or discovery; the cultivation of plants required no new facts or knowledge' (White 1959, 284). Bellwood asks us to divide people into the agricultural Haves and the agricultural Have-nots. Some people, he insists, became superior — or at any rate, overwhelming in their numbers — because they were gifted or lucky enough to become agriculturalists. I have read enough Karl Marx and Leslie White to feel that such an easy explanation for why things are the way they now are is more like Reality TV than like how people around the world really live.

The devil is in the details

Many years ago, I was told by a well known scientist that it is just carping to complain that someone's explanation for what we see around us — the MacArthur-Wilson theory of island biogeography was the idea on the table at the time — can only, say, account for 70 per cent of the variation observed. My feeling then as now is that 'almost true' explanations are a dime-a-dozen; just because some idea sounds eminently plausible — and being 70 per cent right sure sounds a lot better than the flip of a coin — does not make an idea right. Magicians and scam artists make their living off people ignorant of this basic fact of life.

Ever since Malthus, scientists have agreed that there is an intimate correlation between resources and population numbers. At some level of understanding, therefore, Bellwood's insistence over the years that employing agricultural ways of putting food on the table must have led to growing human numbers and consequent greater demand for places to live is an idea that is nowadays nearly tautological. What is new is, for one thing, Bellwood's insistence that 'scale is a significant factor in culture-historical explanation' (p. 10) and that the development or adoption of agriculture must have led to major human 'upheavals', 'bursts' and 'punctuations'. As I have said elsewhere (Terrell 2005, 971), the case studies he offers in this book to support these claims strike me as examples of the fallacy of coincidental correlation — in the familiar form *post hoc, ergo propter hoc*, for instance.

Bellwood has repeatedly said that (1) the development of certain agricultural practices in Asia and (2) the employment there of certain locally occurring plant and animal species as domesticates had (3) a lot to do with the so-called 'Austronesian Diaspora' in the western Pacific (Terrell 2004a). Yet the more we learn about the ancient Pacific, the harder it is to believe that what people have done with plants and animals there has much to do with what happened far off and a long time ago in China or Southeast Asia (Denham 2006a,b; Terrell 2004b). An increasingly strong case can be made that agricultural practices in Asia and the Pacific are analogous rather than homologous (Denham 2006b; Terrell 2002) and Bellwood gives agriculture far too much credit in the colonization of new places in Oceania (e.g. Burley *et al.* 2001; Leavesley 2006). Knowing how to harvest and, if need be, how to cultivate particular species (for the most part locally Pacific in origin) was undoubtedly important in ancient Oceania, much valued both by pioneering voyagers and by those content with staying at home. But accepting this reasonable claim is a far cry from agreeing that agriculture, Asian in origin or otherwise, had much to do with the Austronesian Diaspora or with its manifestation as the 'Lapita dispersal' from the Bismarck Archipelago around 3000 years ago (Terrell 2004a; Terrell *et al.* 2002). However 70 per cent compelling Bellwood's thesis about food production and human population numbers may look when seen from far off in scale and far away in time, the notion that scale is a significant explanatory factor in human affairs is more apparent than real. The devil is in the details; and Bellwood and I have both been archaeologists long enough to know that the Devil is a trickster, a master of appearances and correlations.

Is there any wonder why I find Bellwood's metanarrative about the evolution of the human condition to be less than persuasive? What he tells us fits too poorly with what I see as some of the basic characteristics of the human condition, although what he writes seems entirely consistent with enduring Western ideas about Progress, Power, Ethnic Superiority, and the Origins of all things bright and beautiful.

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Reply

When I was informed of the names of my critics, I knew that sour grapes would be the order of the day. Critics are always more likely to protest than supporters are to support, at least when it comes to media exposure. Obviously, the topic of first farmers population dispersals is sufficiently important in the history of human affairs to generate ideological debate at a very significant level. Steven LeBlanc's name was not on the list in the early stage, so his appearance came as a pleasant surprise and I am grateful to him for his generous and supportive comments. His statement does not need further comment from me and, like him, I will follow forthcoming developments in Mesoamerican and Southwestern archaeology, linguistics and genetics with great interest. I am aware that many of my colleagues in these three disciplines, in many parts of the world, harbour similarly supportive views to those of Steven LeBlanc, but this is not the venue for their expression.

My comments here concern the other four commentators, who clearly differ from me in ideology (a reflection of intellectual environment) and in what they regard as legitimate data for the study of the human past. From my perspective, it is hard to understand Richards's belief that Austronesians are 'a mythical people', Terrell's belief that foraging and farming have always been behaviourally undifferentiated, and the views of Gamble and Pluciennik that the achievements of Palaeolithic peoples in Europe are somehow being denigrated, for dubious reasons, beneath a tide of 'Agricultural Thinking' (Gamble *et al*. 2005). In addition, Terrell and Gamble, archaeologists by training, ignore or denigrate the value of linguistic data. Richards, a geneticist, is forced to do the same, and he also ignores the archaeology to compound the problem, at least with respect to the Austronesian issue. It is a pity that no historical linguist is represented in the discussion. I can only wonder what many of my linguist colleagues would think of the idea, represented by these commentators, that the history of a language family can never reflect anything useful about the history of the native-speaker human population that has 'inhabited' the family since its earliest reconstructable stage.

I will commence with Martin Richards, since he represents a significant and purely genetic perspective. Naturally, I agree with his comments on southern Africa and Bantu languages, and had I seen his recent paper on this theme in time I would certainly have referred to it in my book (Richards *et al*. 2004). I think we both agree that the European Neolithic theatre is currently evolving towards a state of better understanding. I disagree with him that we cannot attribute admixture or a genetic cline to Neolithic expansion, simply because clines cannot be dated. This is self evident, but does it render all attempts to explain clines as irrelevant?

I have deeper issues with Richards's comments on Southeast Asia. I do not regard Austronesian dispersal as a 'preconceived notion' — there are 350 million or more speakers of Austronesian languages, and speech is as much an analysable marker of population history and dispersal as are genes or artefacts. The Austronesians are hardly 'mythical', even if biological variation makes it clear that not all ancestors (e.g. for modern Melanesians) migrated from Taiwan.

Austronesians did not start 'speaking Austronesian languages after moving to Taiwan', and statements such as this simply reflect superficiality. I do not refer to the works of Wilhelm G. Solheim because they are not, in my view, well informed with respect to current understandings of Neolithic archaeology or Austronesian linguistics in Southeast Asia. Nor are the publications of Stephen Oppenheimer (Bellwood 2000a; Bellwood & Diamond 2005).

Much of Richards's perspective is coloured by his research on the genetics of Polynesian ancestry with Stephen Oppenheimer (e.g. Oppenheimer & Richards 2001; 2002). So it is surprising that he does not refer to a number of recent genetic analyses that support an origin for ancestral Polynesians in Asia, especially *via* mtDNA, but allowing for acquisition of some NRY haplogroups in Island Melanesia (Cann & Lum 2004; Cox 2005; Cox & Lahr 2006; Kayser *et al*. 2006; Pierson *et al*. 2006). None of these analyses specify Polynesian genetic origins separately from other Austronesian speaking populations in Southeast Asia, thus bypassing the Oppenheimer & Richards model, which demands an evolution of Polynesians in Palaeolithic eastern Indonesia. All of these analyses point to varying degrees of admixture between migrating and indigenous populations, just as we would expect.

Richards still advocates an age of more than 5500 years for the nucleotide substitution that brought the mtDNA 'Polynesian motif' into existence in eastern Indonesia. It was this time depth (originally put at 17,000 years) that allowed Oppenheimer & Richards (2001) to locate this substitution amongst Wallacean Palaeolithic populations and to rewrite (or ignore) the archaeological and linguistic history of Southeast Asia in order to give priority to the genetics. As Richards notes, I have argued against this interpretation. So has geneticist Murray Cox (2005), whom Richards does not reference. Geneticists differ quite remarkably in their views when it comes to debating the precision of molecular clocks, and they require a yardstick for calibration if they are to know at what average rate over time a nucleotide at a specific locus is replaced. One only has to examine the recent literature on that celebrated Polynesian mtDNA motif for this to become clear, with the range of dates calculated in the last few years running from 34,500 to 1000 years ago at the outer confidence limits of the calculations concerned (Oppenheimer & Richards 2001; Cox 2005; Trejaut *et al*. 2005; and see comments on relativity for molecular clocks by Penny 2005). Because of this, I have no argument against the need for calibration for molecular clocks, as suggested by Gamble *et al*. (2005; a paper that includes Martin Richards as an author), provided one knows which is the correct calibration to use and provided one can allow for the exponentially increasing rate of measurable genetic change as one approaches the present (Penny 2005).

However, I do have to ask why Richards, Gamble *et al*. (2005) wish to calibrate a molecular clock for European mtDNA lineages only against Late Glacial recolonization between 15,000 and 11,500 years ago, as opposed to another period of European prehistory during which there is very considerable archaeological and skeletal evidence for movement of people (Brace *et al*. 2006). This is, of course, the Neolithic, which commenced in Anatolia and southeastern Europe close to 9000 years ago and spread in diversified forms across the continent until it reached Britain about 6000 years ago. Richards and Gamble (I do not implicate the other authors in Gamble *et al*. 2005) are guilty here of 'Forager Thinking' and are adopting a circularity of interpretation worthy of a *post hoc, ergo propter hoc* incantation by John Terrell. Indeed, when I examine figure 1 in Gamble *et al*. 2005, with arrows showing Late Glacial movements from southern France to the North European Plain, I have an uncanny suspicion, fuelled by common sense (see my comments on Gamble, below), that I am looking not only at a forager movement *c*. 13,000 to 9500 BC but also at a Neolithic movement *c*. 6000 to 5500 BC.

Another major problem concerns Richards's unwillingness to engage with new archaeological information from Southeast Asia. For Taiwan and the northern Philippines, a wealth of new data points unequivocally to a continuous sequence of Neolithic cultures in Taiwan from 3000 BC onwards and a move into the Batanes Islands and Luzon between 2500 and 1500 BC (Bellwood & Dizon 2005; Hung 2005; Tsang 2004) and into the western Pacific after 1500 BC in the form of the Lapita archaeological complex (Bellwood & Hiscock 2005). The Taiwan and Philippine data relate to pottery styles, stone adzes, fishing sinkers, spindle whorls, movement of Taiwan jade and slate, presences of pigs, dogs and rice, and the straightforward chronological priority of at least a millennium for Neolithic Taiwan over areas to the south (Bellwood & Dizon in press). These Neolithic cultures did not spread from eastern Indonesia or Melanesia, where antecedents were completely lacking. Put another way, the current state of understanding of both Austronesian historical linguistics and Neolithic archaeology in the western Pacific region leaves little doubt that both cultures and languages moved from southern China, through Taiwan, into Island Southeast Asia and Oceania. There was some backflow the other way — the Batanic settlement of Lanyu Island (Botel Tobago), for instance — but this does not annul the main trend.

John Terrell's text is highly circumlocutional but he appears to suggest that, if cultures can exist in the ethnographic and archaeological records that straddle conceptual borderlines between foraging and farming (as discussed by Smith 2001), then any concept that involves evolution of a farming lifestyle in prehistory to greater levels of population density is automatically unjustifiable. I disagree entirely, not merely because demographic data exist that suggest increased birth rates amongst early farmers (Bocquet-Appel 2002; Bocquet-Appel & Naji 2006), but more importantly because I regard agriculture not as a simple result of domesticating plants or animals but as a result of behavioural moves into dependence on cultivation, resource management, sedentism and other characteristics that ultimately favoured food production over food collection. The presence of a certain level of non-dichotomy in the ethnographic present, as advocated by Terrell, is accepted but this acceptance certainly does not rule out the prehistoric expansions of the early farming populations and their economies that I discuss in my book, and I rather resent the implication that I am unable to make a conceptual distinction between foragers and farmers. The forager-farmer continuum is a total red herring in this context.

Terrell also claims that my views represent a false equifinality. He relates them to his 'fallacy of coincidental correlation', complete with that Latin flourish. He does this without any attempt to engage with any real body of data apart from the Lapita dispersal in the Pacific, an event rather marginal for the main issues raised in my book. How would Terrell explain the dispersal of Austronesian languages? Presumably, it would be by language shift alone in Island Southeast Asia, a viewpoint with which I do not agree, as my book documents at length. In order to hold such a view, one must believe that there are no correlations between languages and their speakers at all — languages become like common colds, floating virus-like between receptive but unsuspecting hosts. Such a wonderful idea needs a mechanism beyond simple imagination.

As far as Lapita archaeology is concerned, Terrell overlooks the information that those communities in Island Melanesia who stamped their pots with dentate patterns between 1500 and 1000 BC also grew tubers and managed tree crops, and kept pigs and dogs. They undoubtedly had seaworthy canoes — without them they would never have reached their island homes. They were able to travel much further than their hunter-gatherer predecessors in Indonesia and coastal Melanesia[1] because they had agricultural production (useful on very small islands), domesticated animals, very good maritime technology (supported, many suspect, by encouraging wind patterns), and perhaps an interest in searching for new lands, opportunities, naive faunas, obsidian, whatever. I most certainly do not claim that 'agriculture' somehow forced the Lapita expansion to occur in the absence of any other determinant, but I do question any idea that it could have occurred without it.

Furthermore, when Terrell claims that agricultural practices in Asia and the Pacific are analogous, not homologous (i.e. representing parallel developments rather than shared ancestry), he completely overlooks the linguistic cognates for all the major foods produced in both areas. Wolff (1994) and Zorc (1994), for instance, reconstruct terms for yams, aroids (both *Colocasia* and *Alocasia*), banana, coconut, sugar cane, sago and breadfruit, as well as pigs and dogs, to either one or both the reconstructed linguistic vocabularies termed Proto-Austronesian or Proto-Malayo-Polynesian. These two vocabularies were located in Taiwan and the Philippines respectively, prior to any Austronesian movements into Oceania, and none offer any suggestion that Island Oceanic (i.e. excluding New Guinea) people invented their agricultural systems independently (see Pawley 2002, 266 on this important issue of cultural continuity). Had they done so, we would expect newly innovated terms for all crops in Near Oceania or multiple borrowings from Papuan languages, and these we do not find. Despite the unarguably independent development of agriculture in the New Guinea Highlands, this was not the sole source for Oceanic agriculture.

Clive Gamble's statement that the genetic evidence 'can apparently be interpreted as Neolithic dispersals sloshing around the molecular pool' is a misrepresentation of what I was suggesting in Chapter 11 of my book. He also states

> All of this confirms that the world, or at least the one Bellwood is interested in, began with agriculture. All subsequent diversity, both cultural, cognitive and biological, stemmed from that late revolution.

This again is ridiculous. Very large regions of the world witnessed hunter-gatherer continuity until recent time, especially in Australia, southern Africa and large parts of the Americas. It is not my intention to hide this or denigrate the achievements of these people. I am, of course, concerned in my book with the histories of agricultural populations within the past 12,000 years or so, the period within which the archaeological record indicates that agriculture has been practised. If by doing this I omit detail concerning

https://doi.org/10.1017/S0959774307000078 Published online by Cambridge University Press

the preceding two million years of human existence, then so be it. My perspective on the role of Neolithic/ Formative cultures is opposite to that of Gamble, in that I do not wish to date all language family origins and initial spreads to the local Palaeolithic record, or to regard Neolithic assemblages as derived entirely from local pre-Neolithic forebears, or to agree that all modern populations have only local Palaeolithic ancestors. In my book, I address the evidence for this viewpoint in some detail, and I invite those with broader geographical and disciplinary perspectives to examine the evidence objectively.

Gamble peppers his narrative with random attacks on people such as Childe, Braidwood, Diamond, Renfrew and Watkins, and accuses me of intellectual dependence on C.O. Sauer. This is an additional smokescreen for Gamble's position that homelands for agriculture cannot exist. He is confusing the primary domestication of major economic crops and animals with the subsequent histories of these species as different groups acquired them across much of the world. For instance, I cannot agree with Gamble that pigs have been domesticated 'independently' on several occasions — this is a concept entirely separate from the observation that the world's domesticated pigs carry genetic signs that they were not all domesticated in one source region. Ancient keepers of domesticated pigs allowed 'wild' genetic material to filter into their pig populations on a continuous basis in different regions, just as they have done ethnographically in New Guinea. Hence, lots of regional pig species ultimately became incorporated into the domesticated pool. This has no relevance for Gamble's concept of independence of origin ('at least six times') of pig domestication from separate hunting backgrounds. Yes, I agree there is more to the history of animal and plant domestication than a simple concept of homeland, but I make no attempt to hide this in my book. As for bottle gourds and dogs, I accept that hunters and gatherers manipulated their breeding trajectories; after all, Australians had dogs (dingoes) for over 3000 years but they were not the ultimate domesticators.

Gamble's reference to the Indian Ocean passages of zebu cattle, bananas, sorghum and other species also puzzles me. I discuss this axis of crop and animal transmission in my book, but would make the point that these transfers were not occurring in the early millennia of farming, which after all began in the Levant soon after 9000 BC, about 7000 years before sorghum reached the Harappan. The arrows in the maps to which Gamble refers were not intended to document these later movements. Furthermore, my map 10.10 was not intended to show the movement of cassava into Panama and the Caribbean, but was documenting a possible expansion of language families on the South American mainland. I do not doubt that this crop had an Amazonian origin and moved in the directions favoured by Gamble; and, surely, anyone who actually takes the time to read Thor Heyerdahl in the scholarly version (Heyerdahl 1952, that goes way beyond the Kon-Tiki expedition) would know that his views were based on far more than mere common sense.

Finally, I do not suggest anywhere in my book that the patterns of languages, genes and cultures will always match perfectly in their historical implications. Both Terrell and Gamble appear to believe that I am demanding such correlations, and that if they do not exist with 100 per cent precision then the whole structure of the early farming dispersal hypothesis tumbles down. But the Austronesians, not to mention the Indo-Europeans, the Bantu, and the Afroasiatic and Turkic speakers, are all living examples that such correlations can never be absolute. In terms of disciplinary isomorphism, we need to examine why, or why not, in specific contexts, and not resort to blind flailing against straw concepts. And, of course, the 'EFH' (Gamble's term) never left harbour in New Guinea, owing to lack of productivity (no pre-Austronesian domestic animals), a probable restriction during early millennia to the relatively isolated highlands, and lack of maritime technology. But once coastal New Guineans overcame these problems through contact around 3000 years ago (back to the Lapita phenomenon), they were quick to move — witness the existence of an Island Melanesian population today, many speaking Austronesian languages, from eastern Indonesia to Fiji. This population does 'exist' in genetic terms, as examination of current NRY and mtDNA data will make clear (see the references in my reply to Martin Richards).

I also agree with Gamble that Palaeolithic people could 'interpret the world and appropriate the resources relevant to that interpretation'. But I am interested in understanding history, not in defending red herrings.

I have left Mark Pluciennik's comments to last since these are perhaps the deepest in their ideological content. It has never been my intention, as he implies, to present overarching hypotheses as either/or alternatives to detailed regional interpretations. Pluciennik clearly holds a gradualist perspective on the transition to agriculture in many regions of Europe. I do not argue against this but reinforce that my interest is with the initial spread of farming into regions such as northern and western Europe, not with the developments and interactions that continued for millennia afterwards. Pluciennik, like the three commentators previously discussed, rejects linguistic perspectives, but I argue that the process of language shift that he favours is a very poor model for a movement of related languages (or a single language) on a continental scale amongst societies constituted on a loosely 'Neolithic' (or even Bronze Age) social and political scale. Widespread (i.e. on a continental scale) and absolute language shift in the absence of substantial amounts of native speaker colonization did not even work for most historical empires, as I discuss in my book. On the other hand, I agree with Pluciennik when he requests that we consider the former existence of what he terms non-analogue processes, by which I presume he means processes not documented in the historical or ethnographic records. Events might have unfolded in the past in ways that are totally without analogues in the world we know now. I wish Mark Pluciennik success in his quest for intellectual stress in this arena.

Reflection

Looking back over this exercise, I think I have enjoyed it. Issues that have easy resolutions soon pass from academic visibility. Some of the commentators have required from me a much broader perspective that I could ever have provided, even in a book as broad as *First Farmers*. This is especially true of Mark Pluciennik, who requests concentration on the totality of Neolithic prehistory in northern Europe. I leave the battlefield still enthusiastic about the EFH, as Gamble terms it, insofar as it focuses on the initial spreads of farming populations rather than on all the subsequent millennia of forager-farmer interaction. Given that it has been created from multidisciplinary data, the least I can expect is that those who wish to overthrow it will try to do so from a similar multidisciplinary perspective. This has yet to happen.

Note

1. I accept an independent development of arboriculture and tuber agriculture in the New Guinea highlands, as my book will indicate, but Melanesian archaeologists have yet to demonstrate that this life-style spread into adjacent Indonesian and Melanesian islands, or even to coastal New Guinea, in pre-Austronesian times.

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