

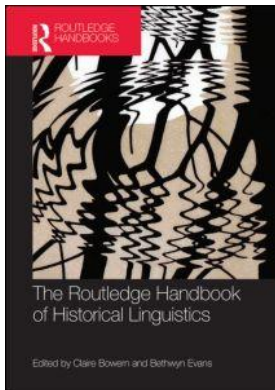
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### Prehistory through language and archaeology

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# Prehistory through language and archaeology

Paul Heggarty

## 1 Language as a window on the past

Unlike most of this book, this chapter is not about the histories of languages themselves. Rather, it explores what those histories can tell us of the populations and societies that spoke those languages through the past. There has been a tendency in other disciplines to overlook how linguistics can open up its own ‘window on the past’, in some respects just as surely as can our genes, or the material culture remains left by our ancestors and analysed by archaeologists, and even early historical records. Although there is great potential here, it remains a challenge to work out exactly what the (historical) linguistic record really tells us. Moreover, that record in turn cannot safely be read without the complementary perspectives of the other disciplines, which historical linguists themselves have likewise tended to overlook. This chapter approaches the cross-disciplinary task from both directions, then: how historical linguistics might not only enrich, but also learn from, its sister disciplines in the study of the past.

Archaeology, genetics and historical linguistics all differ radically in both their data sources and analytical methods. In principle that should only make the various disciplines all the more complementary, but in practice the subplots seem to contradict one another as often as they concur. Still, there was only one past, so the goal is a common one: to converge our different, partial perspectives into a more holistic understanding, coherent across all of our disciplines, of what really went on in human prehistory.

On the broadest level, the lessons lie in how language lineages form complex, unequal patterns from one part of the world to the next. Some regions attest to powerful convergence (e.g. the linguistic areas of the Balkans, Northern Asia or Mainland South-East Asia), while others show acute lineage diversity (e.g. New Guinea). Elsewhere, vast swathes of territory are deserts of linguistic diversity, filled only by a single, deep-time language family such as Indo-European, Afro-Asiatic, Sino-Tibetan or Austronesian. All of these patterns cry out for explanations in the (pre)histories of human populations.

For an illustration on a finer scale, while the Romance language family is known to derive directly from the Latin of the Roman Empire, in fact the distributions of the two do not match closely at all. Most of the Empire’s western half continues to this day the legacy of speaking ‘in Roman’ (Latin *rōmānicē*, whence ‘Romance’), but around the Eastern Mediterranean no such lasting imprint remains. The contrasts speak volumes about the different nature, scale and strength of Roman impact in different regions of the Empire, linguistic data all too often

overlooked by archaeologists and (pre)historians. Similarly, Germanic-speaking tribes overran and ruled much of Western Europe for many centuries, but their linguistic fates paint a very variable picture of their impacts. The Franks and Burgundians in France, Lombards in Italy, and Visigoths in Spain, for example, all vanished into linguistic oblivion (while the local Romance prevailed). In most of Britain, however, the Germanic speech of the Angles, Saxons, Frisians and others soon rose to dominance, ultimately to become English. In the latter case, archaeologists and geneticists have long debated the respective strengths of native (Celtic) and incoming (Germanic) populations, but rarely invoke the striking linguistic contrasts that contribute their own valuable perspective.

## 2 Archaeology, genes and language: weaving the records together

### 2.1 Language, genes, culture, 'peoples', migrations?

How, then, might one best correlate the linguistic record of the past against the archaeological one? Most helpful to start with is in fact a cautionary tale of how *not* to go about it. Early thinking relied firstly on monolithic, discrete concepts of languages, cultures, peoples, races, and migrations; and secondly, on simplistic equations between them all. The result was all too often that archaeologists, linguists and geneticists simply “built on each other’s myths,” as Renfrew (1987: 287) puts it. By the mid-twentieth century this had rightly engendered a generalised distrust of grand cross-disciplinary synopses.

Genetics and physical anthropology have long since given the lie to simplistic and mutually-exclusive categorisations of ‘peoples’, let alone ‘races’ (AAPA 1996). And although languages are typically passed on from parents to children, together with genes, they can also just as well be learnt by speakers irrespective of their own human genetic ancestry. Even a single bilingual generation can suffice to mediate a language switch, and break for good the link between a given population’s genetic and linguistic lineages. Such issues are covered mostly by Pakendorf (this volume), however, while this chapter will focus on how historical linguistics relates to archaeology. Here too, out-dated visions of ‘cultures’ and identities as monolithic entities, and the reflex to invoke long-range migrations of ‘peoples’, have given way to a more balanced vision in which more general *processes* of demographic and cultural change and dispersal often make for less romantic but more plausible explanations. Historical linguistics seems not to have fully caught up with this new understanding. It has tended to rely, for example, on traditional approaches of ‘reconstructing culture’ through language, or migrations inferred from splits in language family trees, in order to propose scenarios for prehistory.

Many of those scenarios are now being increasingly questioned by updated archaeological thinking, and by applications to language data of a range of new quantitative techniques based on modelling in terms of cultural evolution. This has drawn renewed attention to language data as a source on human prehistory, and sparked a vigorous debate between old methods and new. In this climate, this chapter makes no apologies for being provocative to both perspectives. Rather, it relishes a fascinating moment at which to be exploring language prehistory, as new insights challenge many of the old certainties, reinvigorating the field and throwing it wide open once more. However the methodological debate plays out, we can look forward to great potential for rapid progress in uncovering what language can tell us of our past.

## 2.2 Real-world cause ... linguistic effect

If the equation ‘language equals genes equals culture’ is too naïve, how *should* we go about our task? To find the real equation that links our disciplines to each other, we need to return to first principles, to ask how it is that language(s) can inform us of the past at all.

The basic answer is a simple one, and lies in the nature of language itself as a ‘social animal’. It does not just happen that language lineages spread, interact, diverge or converge, as if in a social, cultural and demographic vacuum. What determines the (pre)histories of language lineages, and shapes how they relate to each other, are just the real-world contexts that impacted upon the populations that spoke them. This relationship is unambiguously one of cause and effect; and in a direction that is equally ineluctable. Whether, how, and which particular languages diverge or converge is not just some natural law of ‘what languages do’ or ‘how languages evolve’. Processes in the real-world context – demographic growth or collapse, migrations, conquest, or more subtle socio-political and cultural changes – are the cause (see section 6 below); they alone determine entirely the linguistic effects of divergence, diversity and convergence.

## 2.3 Language change is not language divergence

Lest this cause-and-effect principle be misunderstood, let us clarify immediately that language *change* must be distinguished from language *divergence*. Much of this book has been concerned with the mechanics of how language changes, as a question internal to historical linguistics; and external forces certainly do *not* normally determine the form and nature of whatever particular language changes arise. The detail of why Latin *pluvit* ‘it rains’ changed to *plouă* in Romanian, for example – or to (*il*) *pleut* in French, *piove* in Italian, *llueve* in Spanish and *chove* in Portuguese – has precious little to do with archaeology or history.

What external forces do determine, meanwhile, is whether those changes (whatever linguistic form they take) either develop independently and differently, or come to be shared, from one region to the next. That is, real-world contexts dictate not so much which particular *changes* occur, but which patterns of *divergence* may emerge from any changes. External processes determine how coherent are the populations that speak any given language lineage through time and geographical space. They further govern which population groups or cultural characteristics, language among them, spread at which times, and in which ways and directions.

Within Indo-European, for instance, the Romance, Germanic and Slavic branches each diverged into its own sub-family, whereas Albanian and Armenian remained as singleton branches. All underwent continuous change through time, but in some branches the changes patterned into major internal divergence, while in others they did not. All lineages changed, so language change *per se* cannot account for these very different outcomes. The explanation is not a linguistic one at all, but echoes the contrast between, in the cases of Romance, Germanic and Slavic, the lasting expansive impacts of Rome and of the forces that hastened its fall; and for Albanian and Armenian, the absence of expansive drivers of such scale. The patterns observed across languages – of diversity, relatedness, divergence and convergence – are outcomes at the receiving end of a cause-and-effect relationship, moulded directly by ‘forces of history’ that are independent of language itself.

## 2.4 A non-uniform prehistory

Within historical linguistics (e.g. Ringe *et al.* 2002: 60–63), it is common to invoke the general principle of uniformitarianism: that the mechanisms behind (language) change cannot be assumed to have been different in the past from those we observe in the present, nor from one part of the world to the next. Certainly, at the micro-levels of the mechanics of language *change*, and of communication and social interaction between individuals, uniformitarianism is a reasonable default assumption.

As just pointed out, however, this chapter is not primarily about such mechanisms of language change or interaction between individuals. Rather, here we need to abstract away from those micro-level mechanisms to the forces that determined the “history of everybody over the last 13,000 years,” as Diamond (1997) puts it. For only by understanding the very unequal “fates of human societies” (Diamond’s alternative subtitle) can we account for the parallel fates of their respective languages. At this macro-level, nothing could be further from the truth than to imagine that the forces impacting upon human societies – above all, how some of those societies impacted upon others – have been constant worldwide and over time through the Holocene.

From a pre-Neolithic stage at which all humans lived in small-scale hunter-gatherer bands, only in some regions did a ‘Neolithic Revolution’ (principally the development of farming) set in train the eventual rise of ‘complex societies’ – complex in the archaeological senses of social stratification, and territorial and demographic scale. Moreover, the Neolithic itself played out in very different ways from one time-period and one part of the world to the next. Indeed in many regions, farming and complex societies never arose indigenously at all; and even where they did, deep gulfs persisted between the cultures, technologies and farming ‘packages’ of the indigenous civilisations of the Americas and those of the Old World, for instance.

Far from appealing to ‘uniformitarianism’, then, when it comes to the basic forces that shaped human prehistory it is only the very *lack* of any uniformity in how they applied across time and space that can explain, for example, the glaringly unequal impacts that the societies of Europe and the Americas had upon each other, or those that Bantu-speakers wrought on the populations (and thus languages) of much of Africa. World prehistory is patently not a story of equals and uniformitarianism – so nor is the linguistic panorama that it has shaped. Today’s catastrophic collapse in human linguistic diversity stands in stark contrast to the many previous millennia, over which conditions had been such that that diversity had arisen, not vanished (section 6.1).

## 2.5 Linking the disciplines: through processes

Patterns of language diversity or relatedness, and of divergence or convergence, are thus outcomes moulded directly by ‘forces of history’ independent of language itself. These are precisely the contexts that archaeologists too seek to uncover. For the same processes leave their imprints likewise in the material culture left behind by those same human societies, as well as in the bioarchaeological and genetic records (Pakendorf, this volume). It is here, then, through these processes, that the basic link between our disciplines lies.

Of the various aspects of the linguistic panorama, research on prehistory has hitherto focused very heavily on the world’s major language *families*, and how to account for them. As hinted at the close of this chapter in section 7, this focus is in fact rather unhelpful for the broadest perspective on (pre)history. But because historical linguistics itself has always focused on language families, inevitably this chapter must too. And for the (pre)historian, the

process that any language *family* effectively denotes is one of significant territorial expansion, for without it that lineage would simply continue as a single, small, coherent language. The key equation that links linguistics to archaeology (and genetics), then, is not that languages equal cultures, but that any language family equals some expansive process(es) – whether demographic, socio-political, cultural, or other.

## 2.6 Linking the disciplines: three levels

For any one family, debates tend to focus above all on its first major dispersal, on various levels. First, in *geographical space*, the language family's homeland is the small original region where its ancestral proto-language was spoken, and out of which it first began to expand. Second, on the *time* dimension, the date of that first expansion gives us the family's time-depth, by first setting in train its geographical divergence.

This already identifies two key levels around which the search for correspondences between linguistics and archaeology revolves: the 'where' and 'when' of language prehistory, i.e. geography and chronology. For a convincing match, the expansive processes that archaeologists detect in their record, and the effects (language families) that linguists see in theirs, obviously must together all point to the right place at the right time. Specific methods have been proposed to analyse linguistic data in ways that can pinpoint, or at least narrow down, when and where a given (proto-)language was spoken.

There is another level too, just as essential, but less tangible and thus far harder to devise specific quantitative techniques for, so it is approached more in terms of general theoretical models. As already stressed, a language family does not just come to exist for no reason. So there must also be a good fit between our disciplines on a third level of *causation*, the 'why' of language prehistory. This requires above all that any candidate for a real-world cause must be *commensurate in scale* with whatever linguistic effect it is invoked to explain.

An ideal family to illustrate these three questions of where, when and why is Indo-European, for the two main hypotheses on its origins – actually first advanced by archaeologists, not historical linguistics – paint radically different scenarios on all three levels. They place the Indo-European homeland either on the Eurasian Steppe just north of the Black and Caspian Seas (e.g. Mallory 1989), or in the northern arc of the Fertile Crescent, through central-eastern Anatolia (Renfrew 1987). They set the time-depth when it first began to disperse and diverge at around either six millennia ago, or nine. And they see the process(es) by which it spread to such dominance as either the domestication of the horse, nomadic pastoralism and long-range migrations, or the spread of farming by 'demic diffusion'.

Most of the rest of this chapter is structured around these three levels of when, where and why. Sections 4, 5 and 6 take each in turn to survey the main methods and models employed, both traditional and new. Naturally, discussion of individual illustrative cases has to remain concise here, but further coverage on each can be found in the seven corresponding continent-by-continent chapters on language prehistory within Renfrew and Bahn (2014). Many cases continue unresolved, so the aim here is not just to set out the methods and models, but critically to assess the value and reliability of each, in particular in the light of the relevant aspects of archaeological understanding, often rather overlooked in historical linguistics itself. Before starting our survey, however, there is one fundamental and general issue to address first, for it underlies the thinking behind so many of the individual methods and models, both traditional and new: the 'family tree' concept.

### 3 Models: trees, evolution ... and real population history

However appealing the neatness of binary branches and evolutionary models may seem, the cause-and-effect relationship immediately suggests concerns when applied to language. It would seem naïve to assume that the trajectories by which human societies developed through time and geographical space, and how they impacted upon each other, should yield outcomes that would always, or even primarily, take the form of discrete changes of state and sharp branching separations, ‘preferably binary’. And the phenomena that have shaped those societies in so many complex, non-discrete ways are the same that determined the relationships between their languages.

This is not to deny that there is much to be gained from modelling. Provided that it is intelligently done, in full awareness of the inherent idealisations, modelling can certainly open up powerful analytical approaches not feasible without the more easily manageable structures that the tree idealisation permits. Bayesian approaches, in particular, are devised precisely to handle uncertainties such as those entailed by cross-cutting signals incompatible with any single branching pattern (Dunn, this volume). The danger lies in still sticking too close to the comfortable tree idealisation, however, when hypothesising on the real-world population contexts that might underlie the patterns in our linguistic data. Traditional historical linguistics also remains too closely wedded to its branching classifications, as almost an ideal in itself – note for instance Robinson (1992: 249, 263), or how Ringe *et al.* (2002: 78, 110) talk of the ‘failure’ to find a ‘perfect phylogeny’ because of the ‘problem of Germanic’ and its ‘recalcitrant data’. As we shall shortly see, even in the best-studied of all language families, abstract debates among historical linguists have often included majority proposals that seem to fly in the face of what is otherwise known about population prehistory. Not surprisingly, such debates all too often end up bogged down in sterile disputes on which is the correct ‘tree’ for the family; disputes destined to remain never-ending in the many families where there probably never was a neat branching tree in the first place, as known population histories often imply.

English itself provides a clear example. Most linguistic reference works still explicitly identify its closest relative as Frisian, once the dominant language of the North Sea coast and offshore islands of the northern Netherlands and north-eastern Germany. Frisian does hold a handful of characteristics in common with English, and not with other continental Germanic varieties. Yet our historical and archaeological understanding of Germanic-speaking settlement of England allows for no dominant role either for the ancient Frisii, or for any other single group that may be more directly ancestral to today’s Frisians. There was conspicuously not just one group involved, and others like the Jutes, Angles and Saxons all loom larger. The historical linguist’s construct of Anglo-Frisian sits very uncomfortably with all else we know of the origins of the population whose language ultimately came to be English, arisen out of a great mixing of close Germanic dialects, within which any emergent ‘Frisian’ characteristics were just one contribution. On all non-linguistic evidence, it seems implausible that a uniquely Anglo-Frisian *branch* could ever really have existed, which seriously misrepresents how English actually arose. Romance too, and Indo-European as a whole, likewise illustrate that even for the best known of all language families, centuries of research has still not led linguists to an agreed clear branching sequence. The most plausible explanation is simply that there never was one in the first place, in what actually happened in (pre)history. The real ‘problem’ is the model.

From the perspective of a realistic vision of population prehistory, it is above all in shaping historical linguists’ *expectations* that the tree model has led us most astray. It is as if the

default assumption in the discipline still remains that language families *should* diverge into tree patterns, and that the dialect continuum can safely be seen as an exceptional case. Worse still, continua are often effectively dismissed, as if fundamentally defined by splits, but where contacts between the branches have since inconveniently confused that true underlying picture. In some modelling applications, dialect continua may have to be analysed in terms of “splits then contact,” but that is still to conflate different population histories (see Heggarty *et al.* 2010: 3829–3831).

Language data themselves, across great swathes of the world, deny the tree idealisation. Dialect continua, not neatly branching trees, characterise much of the Arabic-speaking world, Bantu over most of the southern half of Africa, the Sinitic family across China, the Indic languages of most of India, and large parts of major New World families such as Algonquian – to mention just a few examples. Even in Europe, dialect continua have only receded in recent centuries, with the rise of new social and national structures. Those should not mislead us into assuming that language families most typically diverge into neat branching structures: for most of history they appear not to have done so.

Excessive adherence to the tree idealisation has arguably been the single biggest spanner in the works that has frustrated and confused attempts to fit our languages more plausibly and coherently into population (pre)history as seen by other disciplines. So far-reaching are its repercussions for many of the methods now to be explored that we shall necessarily meet some of them below; see also Heggarty *et al.* (2010) and François (this volume).

## 4 Language chronologies

### 4.1 Dating by amount of change and divergence

A range of techniques aspire to date when a particular language family began to spread geographically, and thus diverge. At the heart of the main approaches is that every language lineage changes through time. This is of course why, once a (proto-)language is somehow spread into different regions no longer in close contact, changes accumulating differently from one region to the next will lead it to diverge into a family of related languages. So there is in principle some relationship between amount of time elapsed and amount of change and divergence observable within a family. Various techniques have sought to harness this relationship in reverse, to get from language change data to estimated dates. Here there is space only to discuss the two best known techniques, but together they illustrate the issues involved.

To linguists observing the early days of radiocarbon dating in archaeology, there seemed an enticing parallel in the ‘decay’ of cognacy counts in basic vocabulary. This was the inspiration behind glottochronology, devised in the 1950s by Morris Swadesh (with a number of variants since). Much more recently, a very different method has been introduced by Russell Gray and colleagues. This draws on Bayesian techniques first developed for phylogenetic analyses in the biological sciences. These techniques are explained more fully by Greenhill (this volume) and Dunn (this volume), and only summarised here for the purposes of our focus on the archaeological perspective.

The two techniques have in common firstly the type of change that both use as data: the differences that accumulate between the vocabularies of related languages as they diverge, as some of the words that they inherited in common (‘cognates’) shift away from the original meaning or drop out of use. Secondly, both techniques are ‘calibrated’ – albeit by very different means – against known cases of language divergence over recent, historically attested timespans.



There, however, the similarities end. Firstly, glottochronology dates from a simple, overall amalgamated count of how many cognate losses separate each individual pair of languages in a family. The Bayesian phylogenetic approach, meanwhile, makes use of the full detail in the patterns of exactly which cognates are shared across which combinations of languages across the entire family. Vast numbers of ‘family tree’ configurations are hypothetically possible, which are therefore sampled for those most highly compatible with the patterns across the cognate sets in the data.

Secondly, glottochronology assumes one specified, general and constant rate of loss of cognates in all languages. A simple formula returns how long it would have taken, at that fixed rate, for any given number of cognate losses to arise. In the Bayesian approach, wherever within a particular family any reference dates are historically known, they are applied to calibrate the corresponding tips or nodes in the trees. All modern languages are thus calibrated to the present day, and past languages to the date-ranges of the written records that attest to them, as far as those are known archaeologically or from references in the texts themselves: Hittite tablets, Oscan inscriptions, Tocharian and Gothic texts, and so on (a total of 34 calibrations in Bouckaert *et al.* 2012: Fig. S2). From such calibrations a time-depth for the entire family tree is in effect extrapolated, though crucially the ‘clock’ used to do this is a ‘relaxed’ one, which can accommodate significant variation in rates of change over time: “on average within 47% of the mean rate” for Bouckaert *et al.*’s (2012: SI 7, 24) Indo-European database, for instance. The overall result is not one tree with one date, but a distribution of date estimates from thousands of trees selected as those most plausible given the language relationships represented by the input data (Gray and Atkinson 2003: fig. 1b–e).

These two main approaches can produce markedly different results, as in the Indo-European case. Glottochronology gave a time-depth of 5,500 to 6,500 years, in line with the Steppe hypothesis (see section 1.1 above) and traditional thinking on other levels too (sections 4.2, 5.1 and 6.3). Bouckaert *et al.*’s (2012) Bayesian phylogeny, however, yields results that instead match the nine millennia or so entailed by the Anatolian hypothesis (although Bouckaert *et al.*’s [2013] correction gives a rather younger estimate: see Heggarty [2014]). Many historical linguists remain far from convinced by the Bayesian phylogenetic approach, but have struggled to explain why it should not be at least as good as, if not an improvement upon, glottochronology, long viewed as “decisively discredited” by many in their own discipline (Dixon 1997: 36). As best appreciated from Dunn (this volume), the latest phylogenetic techniques are in a different league to simplistic glottochronology, in how fully they make use of the detail in the language data, and in the degree of sophistication developed to handle a range of issues already encountered in the biological sciences, and which offer partial workarounds for many of the concerns that arise when applied to language data too. (For more details, see Heggarty [2014].) This is our best shot yet at a method explicitly to *measure* amounts and pace of change in core vocabulary (and variation in both).

The perspectives of history and archaeology, though, do raise objections to the logic of dating by amount of change and divergence. Certainly, the cross-disciplinary parallel with carbon dating is deceptive. Above all, language is not subject to laws of the physical environment of the same ineluctable nature as those that govern radiocarbon isotope decay. Rather, recalling the cause-and-effect relationship (section 2.2), language divergence is determined instead by the many processes that shape the contexts through which speaker populations live, and which are anything but constant over time.

Rates of change (and divergence) can and do fluctuate in fits and starts, under external impacts, such as those of Norse and then Norman French on English (see Heggarty 2010). And even without any such impacts, the basic determiner of whether and to what extent a single

ancestral language diverges into multiple descendant varieties is the degree of coherence of its speech community. That coherence is governed not by language itself, but again by the unstable ‘forces of history’: demographic, geographical, social, political, cultural, and so on. Latin-speaking settlement was largely contemporaneous over what thereafter became the Romance dialect continuum across south-western Europe. As attested by Roman authors themselves, regional divergence began all but immediately (and necessarily so for different substrate effects from one region to the next). The *time-depth* since divergence began is thus more or less the same for any two varieties of Romance: the two millennia or so since first Roman settlement.

The net *degrees of divergence* observed across the family today, however, vary greatly between different points within the old Romance continuum. Provençal is less divergent from Catalan than from Galician, for instance. But this is not because of different ‘split dates’ between the speech in these different regions; there is no sense in which Catalan ‘split’ earlier or later from one or the other. Degrees of divergence across Romance reflect not different start dates, but how many of the innovation waves spread far enough to be shared by any two given regions within the continuum.

Any measures of language divergence can thus also reflect differences not in time-depth but in the degree of coherence of speaker communities, especially across a continuum. Much of what determines that coherence is geographical proximity, which thus often makes for a rough proxy for degree of divergence. Catalonia is simply not so far from Provence as Galicia is, and their respective divergence measures are effectively largely a function of speaker community coherence over distance, not time. Glottochronology fails to recognise this other determiner of language divergence, and thus churns out dates that are historical nonsense. This has been clear ever since Rea (1958) showed how the original formula calculates that Italian ‘split’ from Catalan in 1516, and from Portuguese in 1440. Even if one tries to correct these dates through an adjusted rate of change, that will still produce historically meaningless variation in dates, when the start of divergence across all of Romance effectively began at the same time, Roman expansion.

How far might the latest phylogenetic techniques overcome the objections? Certainly, the model incorporates not a fixed universal rate of change but multiple calibrations specific to each family, and above all a relaxed clock. Furthermore, Bayesian aspects of the approach used are purposely devised to handle and work around uncertainties, including in chronological calibrations. On these grounds, this technique should be more promising than glottochronology.

Nonetheless, concerns remain. In Bouckaert *et al.* (2012: SI Fig. S1), Romance is still necessarily presented as a binary branching tree (by maximum clade credibility, see Dunn, this volume: section 4.3.2), with dates still patently unrealistic in historical terms. The Spanish–Portuguese ‘split’, for instance, centres around AD 1530, when those lineages were already clearly distinct. The time-depth of the root of Romance is accurate in Bouckaert *et al.* (2012), but presumably thanks to Classical Latin being available as a calibration point. All further ‘split dates’ within Romance, however, long postdate the actual expansion of the Roman Empire. Bouckaert *et al.*’s sub-tree does not match what really went on as the family diverged, and their map sequence reconstructs Romance spread into Iberia, for example, over a millennium too late (Bouckaert *et al.* 2012: SI Movie). The problem remains as with glottochronology: the degree to which languages diverge is *not only* a function of a sequence of ‘splits’ extended through time, but that is effectively the only way in which this model can represent it, for it allows no role for speaker community coherence. For an exploration of the wider implications for Indo-European as a whole, see Heggarty (2014).

The repercussions are widest and most disconcerting for the critical issue of calibration itself. Our very impressions of how rapidly languages typically change and diverge over time

have been shaped in rather circular fashion. For Indo-European itself has served for many linguists as an impressionistic benchmark for how quickly languages may change and diverge – but over a timeframe largely ‘calibrated’ on the assumption of the traditional Steppe hypothesis. Given all the uncertainties surrounding linguistic dating, it is ill-advised to presume such hypothetical matches between a language family and a particular archaeological culture or expansion, in order to invoke dates for the latter as if they were calibrations for the former. This truly does constitute “building on each other’s myths.” Note the repeated objections to this effect among the many critical commentaries published with Holman *et al.*’s (2011) recent proposal of another dating technique, their ASJP chronology, also ultimately based on the logic of dating by measures of change and divergence.

On calibration, too, other disciplines in population (pre)history bring a sobering perspective. Their experience gives no good reason to assume any great uniformity or regularity through time either in the coherence of speaker populations, or in their impacts upon each other. Rather, awareness of just how varied were the trajectories of human societies across the globe gives strong grounds to expect the opposite. Researchers with a quantitative penchant often (conveniently) presume that stepping back to a ‘big picture’ scale should even out any small-scale irregularities over the long run, so that some ‘average’ rate of change might after all be workable enough for linguistic dating. But in archaeological and anthropological thinking, the default vision is quite the contrary. If long-term trends are sought, they are not so much constant over time as exponential. Change is very uneven, but continuous and ever faster and more far-reaching since the ‘Neolithic Revolution’: change in the demographic and geographical scales of human societal units (from bands to tribes, chiefdoms, states and ‘civilisations’); in the disparities between them in complexity, organisation and technology; in population densities; in the ability to travel great distances, and thus the propensity to interact with and impact upon each other; and so on. All these potential drivers have in the long run been intensifying over time, so presumably their linguistic impacts should have accelerated too. Yet all we have to go on for our impressions or ‘calibrations’ of rates of language change over time are just those very latest periods for which we have reliable histories. That is, our calibrations are themselves biased towards the atypically rapid change of the most tumultuous recent times, and unreliable for extrapolating back ever earlier into prehistory.

Archaeology knows only too well the potential scale of the implications from an eventual realisation that chronological yardsticks long taken as comfortably fixed could in fact need wholesale rethinking. The ‘radiocarbon revolution’ unleashed a cascade of revisions throughout the relative, stratigraphic chronologies for Europe and the Near East, which had to be both shunted and stretched back in time. The cautionary tale should not be lost on historical linguistics. The current stand-off between different techniques for linguistic dating may ultimately resolve only with our discipline’s own revolution in its entire chronological framework for language prehistory.

For further references and discussion of the issues raised in this section, see Heggarty (2006, 2010, 2014).

#### **4.2 Dating by cultural reconstruction?**

A different approach to linguistic dating is ostensibly founded on the Comparative Method of orthodox historical linguistics (Weiss, this volume), through specific words reconstructed to the common proto-language of a given family. Linguistic palaeontology adds to this a number of further assumptions, however. Its basic claim is that if a word can be reconstructed

back to a proto-language, then the people who spoke it must have been familiar with what it referred to. If that referent is one whose first existence archaeologists are able independently to date – say, an artefact such as a wheel – then the proto-language supposedly cannot have been spoken before the date of its ‘invention’. Widely discussed examples are Proto-Indo-European \*k<sup>w</sup>e-k<sup>w</sup>l-os and \*Hrot-h<sub>2</sub> (Beekes 1995: 37), reconstructed from derivatives in many daughter languages in historical times, in a range of meanings to do with wheeled transport (although also in more general senses of rotation and cyclicity). The expansion and divergence of Proto-Indo-European has thus been claimed to date to no earlier than the appearance of the wheel and certain other technologies such as the plough, the domestication of certain species (including the horse, sheep, goat and vine), and the use of secondary products such as the fleece, milk(ing) and wine. Yet while the apparent logic seems attractive at first sight, on closer inspection it fast unravels. (Many of the concerns are raised also by Epps [this volume], but here the focus is on how archaeology, far from providing the dated real-world framework upon which linguistic palaeontology can rely, only reinforces the case against the whole methodology and its basic assumptions.)

On the level of principle, and despite simplistic assertions to that effect, linguistic palaeontology patently does *not* inherit the status and reliability of the Comparative Method itself. The deception is because it glosses over a critical difference between the two levels on which that method works. When linguists reconstruct from correspondences in sound and meaning, these two levels play radically different roles (Anttila 1989: 365). Reconstruction works essentially on *sound* ‘laws’, in the sense that the changes they plot are precise, regular, identical and repeated across all words that have the corresponding phonological context. This consistency and predictability is what gives linguists such confidence in the sound sequences they reconstruct. That confidence does not carry over to the level of meaning, for we have no strict ‘meaning laws’ to predict precise semantic change (see Urban, this volume). On the contrary, while sound change is exceptionless enough to make reconstruction viable on that level, we cannot safely reconstruct the *precise* meaning or semantic origin of a given word string, especially far back in prehistory.

The supposed Indo-European ‘wheel’ words are themselves powerful cautionary tales. Note for instance how Beekes (1995: 41) reconstructs back to Proto-Indo-European the whole expression \*sh<sub>2</sub>uens k<sup>w</sup>ek<sup>w</sup>los ‘the wheel of the sun’. This, and similar senses in astronomy, time and lifecycles (*the wheel of the heavens, of time, of fate, of life, etc.*) were doubtless known to human societies long before wheeled transport, but also provide an obvious source from which terms for the later wheel artefact could naturally derive, independently and repeatedly, long after the proto-language stage. Indeed on closer inspection of exactly the same ‘wheel’ vocabulary across Indo-European languages, for Coleman (1988: 450, emphasis added), “it looks as if ‘wheel’ was *not* in the proto-lexicon” of Indo-European. Similarly, the \*h<sub>2</sub>erh<sub>3</sub>-trom root by no means necessarily referred to what we now see as its prototypical *plough* sense, rather than some technological predecessor or other (as just one possibility, some form of planting stick?). Nor did speakers in prehistory suddenly need to ‘invent’ words for *wool* or *fleece* upon ‘discovering’ them. Rather, it was only centuries of human selection that progressively bred sheep hair thicker, into what we *now* consider that those terms ‘prototypically’ refer to. Indeed, the assumptions of linguistic palaeontology conveniently fail to ask the essential question of where its much-vaunted roots came from in the first place, as if they could not have existed before the ‘inventions’. Languages do not invent new roots at will; rather, as changing circumstances require, they typically recruit into the ‘new’ meanings roots that they already have, and which therefore necessarily can be reconstructed on the level of sound. Even in its modern technological sense, cognates of

*mouse* reconstruct back to a Proto-Germanic root. That hardly confirms that its speakers had computers; nor did their *houses* or *ships* look much like ours.

The perspective from archaeology forces a keener realisation of how circumstances were indeed changing through a long prehistory. The domains in which linguistic palaeontology has been most applied for dating purposes are early technologies, and species under domestication. Popular impressions notwithstanding, both of these domains are typically characterised not by sudden ‘invention’, but by protracted transitional processes, in many aspects all but unnoticed by speakers themselves (Diamond 1991: 164). Domestication by definition requires genetic change, through human selection, in the nature of a species as a whole: change that cannot be achieved overnight. In the course of a long and continuous process of cumulative, incremental change, a proto-form cannot set a chronological cut-off point. There is no reason why there should have been any particular point when speakers should have necessarily changed their word for a species under domestication. Rather, the expectation is that the name for a domesticate would naturally continue that originally used of its wild ancestor, *without* change (hence the distinguishing qualifier *wild* added to the undifferentiated head noun in *wild cat, horse, oats*, etc.). Many key technologies, likewise, developed by long chains of smaller steps, with every reason to keep the former name for what remained essentially the same thing, albeit somewhat improved, for the same purpose. Alternative explanations for why we are able to reconstruct sound strings for words that in much later languages appear meaning *fleece* or *wheel* are far from forced, then. On the contrary, they reflect the commonplaces of derivation and meaning development, unavoidable especially in domesticates and early technologies, where the referent itself is progressively changing, but only incrementally.

A common defence argues that whatever doubts may undermine any individual proto-form, a coherent, detailed and full set of them, within a narrow semantic field – e.g. seafaring technology in Proto-Malayo-Polynesian (Pawley and Pawley 1998) – can confer rather more confidence in that ‘reconstructed’ cultural context. In principle, the shallower the time-depth, the higher the cognate survival rates, and the more consistent the meanings across many scores of lexemes, the less the concerns (see Epps, this volume). Nonetheless, quite what constitutes ‘enough’ on any of these criteria remains highly subjective. There is certainly much value and interest in careful, exhaustive surveys such as *The Lexicon of Proto Oceanic* (e.g. Ross *et al.* 2007); but it is precisely one of its virtues that it is generally careful to avoid the traditional pitfall of claiming far more chronological or geographical precision than the language data actually warrant (see section 5.1 below).

In practice, precisely where the greatest claims have been made for dating by linguistic palaeontology, the ideal conditions are certainly not met. The one coherent set of terminology most widely invoked is ‘farming lexicon’, and here any optimism fast dissolves in the face of the sterile stand-offs between flatly contradictory subjective interpretations, repeatedly for many a deep-time language family around the world. (For references on all the following cases, see the corresponding continent chapters on language prehistory within Renfrew and Bahn [2014].) In both Dravidian and Afro-Asiatic, largely the same reconstructed terms have been variously interpreted as demonstrating that speakers of their respective protolanguages were either farmers or *pre-agricultural* gatherers of wild grains; and for Indo-European, either fully agriculturalist, or Steppe pastoralists. For Uto-Aztecan, the standard view has it that farming vocabulary does not reconstruct, but one controversial analysis invokes linguistic palaeontology to claim that Proto-Uto-Aztecan speakers were maize farmers. For Mayan, conversely, the *presence* of agricultural vocabulary is very curiously taken by Campbell and Poser (2008: 346) to imply that the family must be *post-agricultural*. In sum, in many of the

cases to which it has been most enthusiastically applied – the broadest and oldest language families, and early technologies and domestications – as a supposed *dating* technique, linguistic palaeontology turns out to be so malleable, subjective and contradictory as to be of no real use in resolving the chronological debate. For a fuller treatment of how an archaeological perspective only reinforces the doubts, and references to the long list of sceptical linguists, see Heggarty (2006) and Heggarty and Renfrew (2014a: 31–32).

## 5 Geography: locating homelands

### 5.1 ‘Words on the ground’: toponymy, substrates, ancient loanwords?

It is all the more curious how long some historical linguists held to the presumptions of linguistic palaeontology for dating proto-languages, when its utility for locating their homelands has long been a *cause célèbre* of subjectivity and unreliability. Depending on whether one subjectively prefers a reading *mountain* or just *hill*, for instance, Proto-Indo-European was supposedly demonstrated as spoken in a region either mountainous (e.g. by Gamkrelidze and Ivanov 1995: 574–577) or not. More speculative still are inferences constructed on such basic and ubiquitous terms as *hot* and *cold* (Mallory 1989: 114). As *relative* terms these tell us nothing reliable about whatever absolute temperature ranges they may be applied to in any context; nor about weather and climate differences, since contrasts in temperature depend also on day and night, fire, even bodily illness. Concepts so universal as *hot* and *cold* cannot narrow down a proto-language’s homeland. Even the apparently promising approach of mapping the overlap in the territorial ranges of a set of specific plant or animal species soon unravels, as in the famous case of cognates meaning *beech* in some Indo-European languages, but *oak* in others (see Gamkrelidze and Ivanov 1995: 535–538). In the Indo-European case, many historical linguists have effectively abandoned such proposals as self-contradictory speculation, and inconclusive. Pawley’s (2011) essay on “Were turtles fish in Proto Oceanic?” well illustrates the inherent difficulties for a very different family and environment.

Indeed, the limits on what a reconstructed lexicon can really offer are better appreciated from the more careful and realistic approach taken by the Oceanic Lexicon Project. Even from this exhaustive survey, and for the fairly recent and straightforward case of Proto-Oceanic, the most that can be claimed is that the reconstructed lexicon is *compatible with* a general island/maritime environment within the Tropics, and perhaps at best Oceania specifically. But that is hardly enough to advance precision on the Proto-Oceanic homeland. In fact, a homeland most plausibly in the Bismarck Archipelago can “be inferred with reasonable confidence without considering evidence provided by lexical reconstructions” in any case (Ross *et al.* 2007: 34). However interesting the reconstructed lexicon can be in various respects, what it can bring to the homeland question is unhelpfully vague. “Few if any of the reconstructed [...] referents are unique to the Bismarcks,” so they cannot “identify the Bismarcks as the only possible location of Proto Oceanic.” They may be “consistent [...] with” and “not [...] disconfirm” (Ross *et al.* 2007: 34, 153) the Bismarcks as a possible homeland, but they hardly exclude anywhere else in the wider region.

Besides linguistic palaeontology, however, there are various other approaches that seek to identify specific ‘words on the ground’ as evidence of past language distributions. These are purely linguistic techniques, however, on which an archaeological perspective has little more to offer. Readers are referred to any standard work on historical linguistics; what follows here is only the briefest of summaries.

*Toponymy*, i.e. the study of the etymologies of individual place names, can in clear-cut cases provide irrefutable evidence that a language was once spoken (if not necessarily dominant) in a given territory. The great limitation, however, is that it is only the more recent cases, where we tend to have other historical knowledge, that seem clear and undisputed. As one steps ever further back into the past, place name etymologies become increasingly unsure, debated, and speculative. Celtic toponyms are few and far between even in easterly regions of Britain, and in continental Europe proposed Celtic etymologies all too often compete with other interpretations of the same place name, or remain otherwise uncertain hypotheses.

Attempts to push toponymy back further in time have focused on types of place name potentially ‘longer-lived’, i.e. for major features fixed in the landscape rather than settlements, but even these – such as proposed ‘Old European’ hydronyms – cannot dodge two particular pitfalls that toponymy faces. Firstly, proper names in general, and place names in particular, can come from many different sources. It is not possible to predict or pin down *a priori* the semantics of whatever roots may have been applied. Secondly, place names show a tendency to phonetic attrition or ‘corruption’ that is especially far-reaching by the standards of the rest of a language’s vocabulary, as in many English place names pronounced in extremely eroded form, e.g. <Worcestershire> as [wɔstəʃə]. A tumultuous history helped similarly ‘deform’ *Ebor(acum)* into *Eofor-wic*, *Jor-vik* and ultimately *York*, changes that would have been irrecoverable had we not had written records.

These two problems combine to make it easy to come up with a putative case for a subjective etymology – and all the harder to confirm it objectively. In practice, place name etymologies, especially far back in time, typically end up widely disputed, and contribute nothing reliable to deep prehistory. Much the same goes for attempts to establish etymologies for the other main type of proper noun invoked, the names of particular individuals or tribes as recorded in early historical documents: witness the ongoing disputes as to etymologies and identities of the Wends/Veneti, for example (Schenker 1995: 3–5).

One further type of ‘words on the ground’ evidence involves comparing past stages of independent language lineages. If they present evidence of contact with each other – usually detected most reliably in the form of ancient loanwords exchanged between them – then one might infer that their respective speaker populations too must have been in some form of contact. The most straightforward interpretation is that they lived in regions directly contiguous to each other, and again in clear-cut cases such evidence can seem compelling, as for example with the Quechua and Aymara families in the Andes (see Heggarty 2008: 38–43).

The Quechua/Aymara case, however, is from a time-depth shallow enough to leave such abundant evidence as to inspire confidence. In other cases, especially as one reconstructs into ever deeper prehistory, hypothesised deep loanwords are typically fewer (for they are more likely to have been replaced since) and the correspondences between them less clear-cut. Moreover, they depend upon reconstructions for remote periods for which our control of chronology is uncertain; we may in fact be imagining loanwords between stages of language lineages separated from each other by millennia. Different sets of putative loanwords can duly end up being invoked to argue for conflicting prehistories. Did Proto-Indo-European have a relatively northerly homeland, for example, given the apparent loanwords exchanged with Proto-Uralic? Or did it instead lie far to the south, to judge from claimed loanwords with Semitic languages instead? Loanwords may also be exchanged indirectly: not straight between languages A and C, but either by the mediation of some language B that lay between them, or both originating in B itself. Even a handful of clear-cut loanwords, then, do not absolutely confirm direct contiguity of homelands.

It is not just loanwords between ancient languages that have been argued as putative evidence of prehistoric contacts, but indications on other levels of language too: i.e. convergence in more general, abstract characteristics of *language structure*. (For an example from the Far East, see Janhunen [1996: 200–201, 231]) Such hypotheses are generally extremely tentative and inconclusive, however: there are good reasons why general structural features do not necessarily stand as firm evidence even of language contact, whether direct or not (see Heggarty 2006: 186–188; Heggarty and Renfrew 2014a: 21–23).

One particular instantiation of ‘loanwords on the ground’ seeks conversely to *exclude* a given region as a candidate for a family’s homeland: words taken as indicating that a specific language was *not* originally spoken in a particular territory, but must be ‘intrusive’ to it, such as the Anatolian and Greek branches of Indo-European. The basic logic, though, is to presume that a *lack* of knowledge about the origins of a word – we are *not* able to detect any surviving cognates – can be trusted as if it were positive knowledge as to where it did come from, and when (an underlying substrate). See Salmons (2004) for a fuller explanation, and illustrations of why these are subjective over-interpretations.

So common are cognate losses that it is standardly accepted that the lexicon that we can reconstruct for a deep-time proto-language is doubtless far poorer than it really was. This is tantamount to acknowledging that it is positively predicted that a fair number of roots will *survive* in only one branch of a family to which they are perfectly native.

## 5.2 The ‘focus of diversity’ principle

Two approaches to locating homelands are based directly on the internal classification of a given language family, but in very different ways. The first technique is based on an assumption that all else being equal, the homeland is likely to be closest to where one finds the greatest diversity of major, deep branches within the ‘family tree’. Although often referred to as the ‘centre of gravity’ principle, a more accurate term would be ‘focus of diversity’.

The principle does seem valid at least in a few particularly clear-cut cases. Austronesian, for example, extends across a vast geographical range, but at the highest level, all but one of its deepest sub-lineages are found on just one island: they form the indigenous (pre-Han) languages of Taiwan, strongly suggesting it as the entire family’s homeland (Kikusawa, this volume). Across the Bantu-speaking southern half of Africa, meanwhile, that family’s focus of diversity fits with evidence from other disciplines, all pointing to a population expansion (with farming and iron-working) out of a homeland in the Cameroon–Nigeria region (see Filippo *et al.* 2012, and further references therein).

On the other hand, other language families reveal that the principle is far from fool-proof. Within Indo-European, the region hosting the highest diversity of deep branches is the Balkan peninsula, but Romance and Slavic are clearly latecomers here, and both leading hypotheses look elsewhere for the family’s original homeland. The basic case against the principle, as in this instance too, is simple and obvious: where the focus of diversity may lie today, or even in the earliest language distributions we know of, need not reproduce the original picture in earlier times.

Since the reality in some cases does match the principle, but in others contradicts it, its usefulness hangs on the balance between them. Specifically, are the counterexamples exceptional enough for the principle still to be trusted? On this, the perspective from what we know of prehistory suggests unfortunately not. In particular, the world’s independent centres of origin of agriculture and ‘civilisation’ attest to a long ebb and flow of expansive complex societies, out of different points within each. In the Near East, for example, a rich archaeological



and historical record suggests so, and the surviving language data reveal a linguistic correlate: a series of language dispersals and extinctions. Aramaic and Arabic, for example, are known to have effaced many earlier languages of the Near East (Woodard 2008). There is no reason why this should not have prevailed over the preceding few unrecorded millennia too.

In China likewise, repeated major language expansions are usually taken to have begun out of the *same* general homeland area: both Sino-Tibetan and then the Sinitic sub-family out of the North China Plain, followed most recently by Mandarin itself. Within Sinitic, the focus of diversity is pointedly *not* in the region that commands near unanimity as its homeland, but along the coast and hinterland of south-eastern China. This can just as well be seen as illustrating the logic that in cases of repeated dispersals out of a core culture area, where a language family's diversity will most likely *survive* is on the contrary on the periphery, furthest from the focus and the threat of being repeatedly overwritten.

In short, whenever we happen to know better, historical linguists have no compunction in overruling the focus of diversity principle. It is bordering on the mischievous, then, that so many still so readily invoke and set so much store by it in unknown cases. It is still widely claimed to support Afro-Asiatic having originated in the Horn of Africa, for example. That understandably fails to convince advocates of a homeland in the Levant instead, where earlier diversity may simply have been lost (an outcome we know of repeatedly ever since earliest history there, at least). Indeed the experience of historians and archaeologists only leads us positively to expect the world's main expansive foci of civilisation to be precisely where the linguistic picture will be most distorted, under a palimpsest of serial dispersals overwriting whatever diversity arose from the previous ones.

### 5.3 Mapping language dispersal through phylogeny

A more reliable way to exploit the classification of a language family should be to look more in detail at how its main branches relate to each other linguistically, in relation to how they also pattern geographically. Such patterns can in principle distinguish stages through which one might trace an expansion back in time towards its homeland. They can also indicate whether a language spread in one direction or another, valuable particularly when attempting to correlate with expansive processes visible in the archaeological record.

As one straightforward example, the language(s) of the Scythians and Sarmatians, once spoken on the Pontic-Caspian Steppe, rank as the westernmost of all languages of the Iranian family. Their linguistic classification, however, sets them within its Eastern branch, quite distinct from the Western Iranian found to their south, beyond the Caucasus mountains (e.g. Kurdish and Persian). This suggests separate movements, and also that Scytho-Sarmatian hailed from far to the east, the core of Eastern Iranian origins (note Pashto in Afghanistan, for instance).

Many other cases are more complex, however. So to discern a finer and objective picture, recent approaches have again looked to more quantitative and detailed phylogenetic analyses. Gray *et al.* (2009) make much of how their Bayesian phylogeny of Austronesian languages maps impressively closely onto a geographical path out of Taiwan and through Island South-East Asia into the Pacific, complete with apparent 'pulses and pauses' matching the assumed stages of expansions and divergence. Still, between the clear start and end points of Taiwan and Oceania, the sequence in the presumed 'pulses' is so compressed that many branches presumably hang effectively on just a few words each. Others interpret this differently: as a weak discriminatory signal also compatible with a dispersal across Island South-East Asia that was much less regimented and progressive, and more haphazard and network-like (Donohue and Denham 2010).

These conflicting interpretations are reminiscent of the case of Indo-European, where the lack of consensus on its highest-level branching is open to interpretation as a result of early expansion not by a sequence of long-distance migrations, but by a more progressive, continuous ‘demic diffusion’ (see section 6.7). A radically new approach is Bouckaert *et al.*’s (2012) Bayesian phylogeography. Languages in a phylogeny are calibrated to known reference points not only in time (see section 4.1 above), but also in geographical space: actual coordinates of where the languages are or were spoken. Tracking back *pari passu* on all three levels together, this technique estimates not just when the family’s divergence began, but where it most likely emerged from, given the modern distributions and the tree topology that relates them.

An archaeological perspective does raise concerns, however. Bouckaert *et al.*’s basic model for geographical movement, the very simple default of a ‘random walk’, is clearly a big idealisation of how and why human populations and cultural traits actually spread. And while arguably reasonable for territorially continuous expansions, it remains to be seen how it would fare for much more scattered and discontinuous families such as Austro-Asiatic, Quechua or the main Amazonian families. Also, the model effectively works back from earliest known distributions of all languages covered, but those are largely an artefact of whenever *writing* began in a given region, not when that speech lineage first reached it. Similarly, where surviving records are too sketchy to include a particular language, the method is (currently) blind to it. Bouckaert *et al.* (2012) thus fail to place much Celtic in continental Europe, but it was definitely there. There are also queries as to how the particular phylogeny generated, effectively ‘Indo-Hittite’, really fits with the dispersal process it reconstructs, and the prehistoric dispersal of agriculture: for details see Heggarty (2014).

Nonetheless, the default assumptions are at least an unbiased starting point, and most striking is how heavily the results come out against the traditional Steppe hypothesis, and pinpoint instead precisely the homeland proposed in the rival Anatolian hypothesis. A neutral eyeballing of the earliest known distributions of the various Indo-European branches has in fact never inclined towards a Steppe origin, and Bouckaert *et al.*’s approach effectively just reconfirms this, only now in a sophisticated and objective quantitative analysis. Given the very nature of language prehistory, it would be naïve to imagine that this phylogeography will alone definitively resolve many of the key debates. But the new perspectives it can bring certainly make it a welcome addition to the historical linguist’s toolkit on the level of geography, given the doubts – archaeological as well as linguistic – that attend all our traditional techniques.

## 6 Causation: models and expansive processes

Many difficulties remain, then, with methodology on the levels of both time and place. So what of the third level, to which historical linguists have arguably devoted rather less attention: *why* does any language family exist at all?

While the dimensions of time and geographical space are clearly defined, and at least in principle tractable also by quantitative approaches, the *why* question is inherently not so amenable to specific methods that might yield as a ‘result’ some clear-cut identification of a specifically defined cause. It is more a question of principles and models for the key formative processes in population (pre)history, and how those might in turn shape language relationships too. Known cases of language family expansions through recent history do provide important bases for inference, but as we have seen (section 2.4) unfortunately there are no good grounds for simply assuming that things worked the same in prehistory, especially at earlier phases as

complex societies were first arising, or even before they did so in any given part of the world. For that, we must necessarily look primarily to archaeology for an understanding of the formative processes underway, and their likely respective impacts, in different periods and parts of the globe.

The processes that might ‘count’ as explanations can be stated in general terms:

For a language family to arise, some factor(s) must be decisive enough to confer either upon one human group – or at least upon its language, usually as part of a broader cultural complex – some greater impulse, ability or propensity to expand, whether at the expense of others, or into territories hitherto not settled by humans.

The range of such expansive processes visible in the archaeological record (or at least inferred from it) makes for a wide choice indeed. The task here is to find ways to assess which are the most plausible as drivers of language dispersals of various forms. To this end, before looking at the main potential candidates individually, it is worth first considering some basic criteria for discerning some structure among them.

A first criterion emerges from genetics. Expansive processes vary greatly in the outcomes they entail for any relationships between linguistic and genetic lineages. A language may spread either because it is brought by a new incoming population into a new territory, or by the population already there switching to a different language. In the former case, linguistic and genetic lineages will continue to match, but not in the latter. Assessing the degree of match or mismatch on these levels, then, may hold the key to whether the factors behind a particular language expansion were of a type that involved either large-scale demographic replacement, or significant cultural transfer between more stable populations (Pakendorf, this volume).

Second is the principle of commensurate scale on the two sides of the cause-and-effect relationship (section 1.1). The most potent linguistic effects we see – the greatest language family expansions – will be most naturally and convincingly explained by the most powerful among the processes affecting the fates of human societies. For any potential expansive process, it is to be kept in mind how its impacts compare with those of other factors shaping population (pre)history; and for any language family, how it ranks in scale *vis-à-vis* others around the world.

A final level on which one can perceive some structure is on the time dimension. The many different processes that have shaped population and cultural spreads have by no means kept the same significance relative to each other over the long and fluctuating trajectory along which human societies and civilisations developed. So as we move on now to survey the various expansive processes involved, we might usefully distinguish them by the respective periods at which they are most likely to have had significant linguistic impact. It is particularly helpful, in fact, to begin from the best known more recent cases, stripping them away progressively in order to prevent them clouding our visions of what other processes may have underlain them deeper in the past.

### **6.1 Drivers in the modern era**

We are today living through what is doubtless the swiftest and most far-reaching phase of extinction of human linguistic diversity. Language replacement is proceeding apace on a vast scale (Evans 2009), driven by a set of circumstances ever strengthening since the dawn of the Modern Era (i.e. from c. 1500). The expansions of the colonial and imperialist periods emerged out of just those parts of Europe at the forefront of the rise of the nation state,

precisely that form of society most characterised by inexorable language standardisation. Literacy became a much wider social phenomenon, while long-distance transport brought very unequal societies into direct contact, and facilitated huge population transfers (not least from Europe and Africa to North America). A compelling array of far-reaching social and technological changes have together rewritten the ground rules for language history, especially in promoting language replacement, much of it conspicuously not in step with genes, but by language shift.

So dramatic an acceleration in language replacement, but only in recent times, makes it all the more questionable to invoke recent cases of language shift as models for remote periods in which modern forms of state organisation, literacy, transport and communications were entirely lacking. Moreover, although many expansive processes have long been able to bring about population and language dispersals over wide areas, the modern conditions for actually holding together a coherent speech community far across them, for centuries thereafter, were generally lacking (except arguably in highly mobile nomadic or maritime societies).

To take on board how different the pre-Modern world was in these respects entails another lesson for historical linguistics. Right up until the eventual rise of the nation state and other modern drivers that have progressively polarised the linguistic panorama into monolithic national languages, through prehistory and early history we must positively expect a far greater incidence of dialect continua. This duly explains their prevalence so widely across the world wherever these modern conditions have not yet been in place long or powerfully enough to efface them (section 3). This deception of the modern world, tempting us to downplay dialect continua, has distorted our views of all earlier language (pre)history in critical ways: flattering presumptions of more splits and isolated ‘migrations’ than there ever really were (section 3), and imparting a bias to underestimate the time-depths of original dispersals (section 4.1).

## 6.2 Prestige and utility in language shift

These present-day processes are far from the only ones that may drive language shift, of course. Others do not require any overt socio-political and administrative structures, and may thus have held over periods long before the Modern Era. These less formal motivations are generally seen in terms of language ‘prestige’, though *utility* is arguably more appropriate. The model often invoked is the *lingua franca*, adopted not necessarily by imposition from above, but thanks to its common utility to a broad and mixed population speaking multiple different native languages. *Lingue franche* can thus also spread through relatively egalitarian social interactions and networks, without needing hierarchical societies of top-down control.

There are a number of roles and purposes for which one language might acquire greater social or cultural status or utility, and thus potential to spread, than another. Much has been made especially of trade, and of religion (Ostler 2005), though in part because trade goods and religious iconography happen to be unusually conspicuous survivors in the archaeological record. But whatever their impact on other levels of culture and society, trade networks and religions alone have generally been rather poor drivers of *first* language replacements. In many cases, *lingue franche* remain principally as second languages, and do not replace native tongues. They do not even represent expansions at all, then, on the level of native languages and families that is central to language prehistory. Like Swahili still today (Wald 2009: 885–886), many *lingue franche* count far more second- than first-language users. This leaves them highly susceptible to their apparent (but only second-language) ‘expansion’ collapsing back in on itself once circumstances change: witness ‘The Death of Sanskrit’ (Pollock 2001)

across South-East Asia, and the declines of many once widespread *lingue franche* of the Mediterranean (Phoenician, Greek, ‘Sabir’) or the eastern coast of Africa (Arabic, Portuguese, Swahili), none of which established itself as a *first* language across the whole region. Where trade and religion are not accompanied by more formal and powerful conquest-type expansions, clear cases of language replacement seem relatively few. Indeed, both main subtypes of *lingua franca* normally go back to a single clearly identifiable language of formal administrative control and/or cultural primacy. The pidgin/creole subtype usually builds its lexicon largely from this dominant language, hence identifications as French-, English- or Arabic-based creoles. The *koine* subtype, meanwhile, centres on one dominant dialect among a set of them, as Hellenistic Greek was based on the dialect of Athens.

The expected linguistic correlate of *trade* – or of cultural contacts of other forms – is by no means necessarily an expansion of one particular language, which replaces others as a native tongue and then diverges into a family. In the many periods and areas where contacts between neighbouring groups operate ‘down-the-line’ on more local scales, rather than by long-distance trading voyages, the likely linguistic result is effectively the opposite. For speakers can interact through the corresponding linguistic pattern: rolling, localised bilingualism (or multilingualism) in a chain of different languages linking across geographical space. Their languages do converge on each other, in loanwords and eventually in broader typological characteristics, but they remain genealogically distinct. The outcome is not a language family but a linguistic area, broad but loosely defined, over core and periphery zones. Such is a plausible scenario for how linguistic areas arose in Amazonia or the early Central Andes, for example.

Spreads of *religion* also turn out not to equate well with those of language families: witness the distributions of Buddhism or Christianity, for instance. Or to take an individual language family, Arabic is much less widely distributed than Islam, and reflects more closely the projection only of Arab military power and settlement. ‘Fossilised’ sacred languages, meanwhile, such as Church Latin or Sanskrit, are simply chimeras in terms of first language distributions: they do not replace living local tongues, nor do they change and diverge to leave daughter languages. Romance derives not from Church but Vulgar Latin, its expansion mediated not by religion but by the temporal powers of Rome. Much the same might be inferred as to the real process behind the spread of the Indic languages, derived from the vernacular Prakrits, not the ‘refined’ Sanskrit.

Trade and religion do not seem to be prime drivers of language expansions, then. Apparent partial correlations in their distributions typically boil down simply to all three being carried along together as the baggage of more powerful expansive processes.

### 6.3 Conquest and ‘elite dominance’

Quite how much one might wish to describe these more powerful processes in terms of conquest or invasion is debatable, for they can take many forms. Certainly, the linguistic impact does not depend on whether a conquest is centrally organised: rapid conquest of Britain by the unified army of Rome did not result in Latin replacing the local Celtic languages; that was later achieved, however, by a collection of disparate Germanic tribes; while the coherent Norman conquest again failed linguistically.

A more useful criterion on which to distinguish among conquest scenarios is that of the demographic balance between the indigenous and any incoming population. This determines not only the genetic signal that a conquest may leave, but also, it would appear, its likely linguistic impact. All cases of external conquest involve at least some genetic input from

outsiders, but quite how much can lie anywhere along a continuum from a tiny elite to a mass population replacement. Still, at least as models for our purposes here, it is useful to contrast these extreme poles.

In the popular ‘elite dominance’ model, a conquest is accomplished by incomers demographically insignificant relative to the indigenous population (hence ‘elite’). The dominated population is assumed to switch language to that of the dominant elite: the spread of English across Ireland might be argued as one such case. At the other extreme are invading populations so numerous and sustained as to swamp the indigenous one, and thus spread their own language(s) by *population replacement*. In North America and Australia, within a century or so, ‘European genes’ far outnumbered indigenous ones; or in terms of languages, speakers of European tongues likewise came to far outnumber speakers of indigenous ones. (In part this resulted also from demographic collapse among indigenous populations with no developed immunity to pathogens introduced by the incomers.)

The case might be made for ‘elite dominance’ as the mechanism behind the spread of some languages, at least in recent centuries. But in pre-Modern times, dominant elites in fact time and again conspicuously *failed* to spread their own language; rather, it was they who assimilated linguistically to the demographic majority they had conquered. Among a wide range of known historical examples are: the speakers of Turkic and Mongolic who became the Mughals in India, and the Yuan Dynasty in China; all known incursions from the Steppe into Europe too, save for Hungarian; all Viking conquests (rather than first settlements); all Germanic-speaking elites established after the fall of Rome in France, Iberia, Lombardy and North Africa. In every case, the elite’s language soon vanished. (Contrast the result in England, where there was instead wholesale settlement by incoming Germanic-speakers, far more than a mere elite *vis-à-vis* the local population.) This is not to deny that an elite can have linguistic impacts, which can certainly be significant, but in other respects: in the form of superstrate effects that *modify* the majority indigenous language, rather than eliminating it. The Normans left a mass of loanwords in English, but their own language lineage nonetheless died out in Britain.

When and why conquests either do or do not bring about language replacement is clearest in cases where the same conquering entity spreads its language only to some areas, not others, as for instance in the Ottoman, Inca or Roman empires. Longer or shorter duration of control is sometimes suggested as potentially relevant, but in fact correlates rather poorly in these three examples. Again, more relevant seems to be whether conquest did or did not entail an incoming demographic component significant relative to the indigenous population. Certainly the large-scale pattern across the Americas is that European languages have failed yet to replace indigenous ones only in two types of context. One is where European settlement has been too thin or nil, as in remote parts of Amazonia and the Arctic. The other is where native populations were and remained densest, so that they were not swamped by incoming ones: in the two heartlands of pre-Columbian civilisation, Mesoamerica and the Central Andes. Here too, mass language replacement is now well underway, as the Modern Era increasingly rewrites the linguistic rulebook (section 6.1). But in earlier times, dominance *only* by elites, without major demographic incursions, offers precious few instances of language replacement, and far more counter-examples. This clear tendency from earlier historical cases suggests that historical linguists would do well to curb any enthusiasm for invoking elite dominance as a stock explanation for major language families.

## 6.4 Technologies

Conquest is, in any case, a description of a scenario that might lead to language replacement, not a deeper explanation of why that conquest was possible in the first place. Such explanations boil down to those factors through which speakers of one language are able to dominate those of another. While this is not a linguistic question *per se*, such factors are so often invoked as (indirect) explanations for language spreads that we do need to survey them briefly here.

In many cases at least part of the explanation lies in technologies for *transport*. Advanced seafaring was a prerequisite for the arrival of any human languages to the remotest Pacific islands, though as a first settlement rather than a language replacement. It is also widely and plausibly proposed (e.g. Gray *et al.* 2009: 482) that earlier stages in the dispersal of the same Austronesian language family might also be tied to specific developments in seafaring technology, not least the outrigger canoe. Mobility was key to how lineages such as Eastern Iranian, Turkic, and Mongolic spread across the geographically vast but thinly-populated Eurasian Steppe, from the first millennium BC until late Mediaeval times. Much has been claimed for linguistic impacts long before then, too, attributing the initial spread of Indo-European to the domestication of the horse and development of the wheel. These were no overnight revolutions, however, but drawn-out processes of development, and quite where and when any significant ‘conquest’ advantage may have been conferred is unclear. The eventual devastating impact of the Scythians, Huns and Mongols does not carry over to Steppe peoples millennia earlier, without spoked wheels, war-chariots, the stirrup, or tens of centuries of cumulative human-controlled selection and breeding of the horse. Many of these might in any case best be characterised more as technologies of a second type: military, rather than transport.

Many conquests are also owed to a further set of ‘technologies’, if one can call them that, of *state organisation*. Most often imagined to support language expansions is writing, but this risks misconstruing its true significance before the Modern Era. Where a culture that has writing meets one that has not, that difference usually reflects wider, pre-existing contrasts in socio-cultural context and development. The same broader conditions that favoured the first group’s development or adoption of writing in the first place would likewise tend to favour the expansion of its language in any case.

Technologies can certainly provide the wherewithal for conquest, but the precise question here is different: to what extent do they support those particular forms of conquest that do lead to language replacement, rather than those that do not? Ocean-going ships and navigation, horses, swords, early firearms and writing enabled the conquistadors to win control of the Aztec and Inca realms in very short order. But for centuries thereafter their Spanish failed to dislodge the major indigenous language families in their core regions, where they had become established thanks to earlier expansions achieved without any of those technologies. So any association between major language replacements and particular technologies seems at best a very indirect one. In fact, acute advantages in transport, weaponry and state organisation are precisely what empower a tiny invading elite such as the conquistadors to dominate much larger populations. That is, they can facilitate just the sorts of elite dominance conquest that typically *fail* to cause significant language replacement (section 6.3) – unless they also act as the precursor for significant population replacement, or until the developments of the Modern Era.

## 6.5 Subsistence modes, and farming/language dispersals

A recurrent theme in many language expansions in pre-Modern times is a simple demographic one, then. In a partial analogy with the phenomenon of drift to fixation in population genetics,

it would seem that – all else being equal – where two languages are brought together into a new joint population, if one of them has many more speakers than the other, then over time it will tend to win out, irrespective even of whether the minority is a powerful elite. It follows that processes able to facilitate language expansions should include particularly those that have a direct bearing on relative population sizes. Other than in cases of demographic collapse (as in certain Old World–New World encounters), this means any factors through which one group can effectively ‘outpopulate’ its neighbours.

The most extreme case of disparity in demographic potential is where nomadic hunter-gatherers find themselves pitted against settled farmers already possessed of diverse and adaptable subsistence packages, and in favourable environments to which those packages are well suited. When all of those conditions are met, farming can unquestionably support far greater population densities, by a factor of 50 even for early farming techniques in Europe and western Asia, for example, according to Renfrew (1987: ch. 6). This is the simple but powerful logic behind the *farming/language dispersals hypothesis* in its prototypical form, for in cases that do fit the extreme contrast just outlined, known linguistic outcomes can indeed be very one-sided: the hunter-gatherers’ languages, if they survive at all, invariably end up cantoned into inhospitable areas of little value to agriculturalists. The fate of the languages of the now vastly outnumbered and fragmentary San populations of southern Africa, when confronted by expansive farmers and metal-workers speaking Bantu languages, makes for a clear illustration.

Nonetheless, ever since its controversial invocation by Renfrew (1987) in the Indo-European case, the farming/language dispersal hypothesis has come in for heavy scepticism, especially when some advocates (notably Bellwood 2005) have generalised it to many other major language families, and even modern European languages individually, across the globe. Regrettably, debate has often descended into a sterile exchange between all-or-nothing stances (see Heggarty and Beresford-Jones 2010: 187–188). A more balanced assessment needs to take explicit account of each of a series of key qualifications, as per this definition:

Only across those parts of the world where agriculture became established before the Modern Era, many (but not all) of the most significant language families – in both geographical range and speaker numbers – dispersed along with, and primarily thanks to, the spread of agriculture there.

So to start with, while critics point to major hunter-gatherer language families, such as Eskimo-Aleut, Athabaskan, Na-Dene and Pama-Nyungan, to imagine that this somehow disqualifies the hypothesis, as ‘exceptions’ (Campbell and Poser 2008: 339–340) to it – language dispersals, but without farming – is to misconstrue the proposal. Not even its most ardent advocates (e.g. Bellwood 2005: 2) imagine the hypothesis as a panacea that could account for *all* language families, when it is perfectly plain that it does not and cannot. When properly understood, with the above qualifications, the hypothesis is by definition not applicable across most of the regions where those families are spoken (the Arctic, sub-Arctic, and the arid interior of Australia), where agriculture is only marginally viable (if at all), and/or never arose indigenously. Rather, it is precisely the point that only in such contexts have widespread hunter-gatherer families survived long enough to be known to us – only where agriculturalist languages have not (yet) reached their territories to replace them.

Properly assessing the hypothesis also requires awareness of a whole string of aspects in which ‘farming vs. foraging’ is no all-or-nothing dichotomy. The question is essentially one of *degree*: just how significant a role may the spread of agriculture have played in how many



of the world's major language expansions? There is space here only to summarise two illustrations of the many qualifications and refinements required. (For fuller treatments and a worldwide survey, see Heggarty and Beresford-Jones 2010, 2014a, 2014b.)

Firstly, many subsistence regimes do not fall neatly under some prototypical form – neither sedentary farming nor mobile hunting and gathering. Linguistic outcomes are thus potentially different again in cases such as: pastoralism, whether based on the horse or not (e.g. on the Eurasian Steppe); *shifting* cultivation rather than permanently settled farming (e.g. in Amazonia); and mixed subsistence regimes such as where rich maritime resources are exploited as much as farming (e.g. coastal Peru or Early Neolithic Northern Europe).

There can also be great qualitative differences between one form of agriculture and another. Over time, and long after farming first began in a region, a range of developments can take it across thresholds of intensification, ratcheting up productivity and thus population sizes, with potentially concomitant linguistic impacts too. Such intensifications may result from the development of new, richer farming ‘packages’, of secondary products (wool, milk, etc.), and of farming ‘technologies’ such as irrigation.

Farming vs. foraging is in any case just an instantiation of a more general *subsistence/demography model* of language expansions (‘Model 1’ in Renfrew 1987: ch. 6), widely applicable across subsistence regimes of many other forms. All that matters is that a given package of subsistence technologies – whether for hunting, farming, pastoralism, or any other subsistence regime – confers on the population that uses it markedly greater expansive potential over another in a given region. Differences in subsistence technology can be just as acute between groups who both practise hunting and gathering, and especially where environmental conditions are harsh, as per the scenario usually invoked to account for how Eskimo-Aleut spread to replace the preceding Dorset culture, and whatever language(s) they spoke (Fortescue 1997). Conversely, where hunter-gatherers (and shifting agriculturalists) inhabit less inhospitable, more fertile and productive regions, these may present quite the opposite linguistic signal, as illustrated by the scattered mosaic of major and minor families, as well as many language isolates, across Amazonia.

Given the general scepticism among historical linguists as to farming/language dispersals (e.g. Campbell and Poser 2008: 337–350), the final word here had best stress one overarching archaeological perspective. For all the qualifications, the transition to the Neolithic and agriculture still ranks as the single most defining, cardinal change in the development of human societies. In the light of the basic cause-and-effect relationship that links our disciplines (section 1), it would seem curious indeed if it had left no linguistic impact.

## 6.6 Ecology: climate and environment

The question of subsistence leads on to a final set of factors: environmental ones of geography, ecology and climate. The roles that such factors could or could not have played in shaping the human past – in which circumstances and for which clear reasons – have long been central questions for prehistorians. The lessons need to be applied to interpretations from linguistic patterns too, especially to avoid mistaking apparent statistical correlation for causation (note Cysouw *et al.* 2012: 657). Environment is generally more logically and judiciously seen not as a primary driver in itself, but a background context within which more direct causes and explanations apply, and acquire their particular relevance. Among these are the many factors surveyed in the preceding sections, such as subsistence technologies, especially for extreme climates, and innovations in seafaring. Direct relationships between environmental drivers and language expansions might most plausibly be entertained where they are known or

assumed to have permitted or even driven population movements. These may include both abrupt one-off environmental events and much slower, longer-term processes. Most of the major climatic processes that shaped human (re)settlements of the globe, however – such as the openings of the land-bridge and ice-free corridors through Beringia, or the retreat of the ice sheets after the Late Glacial Maximum – have operated at timescales at or beyond the limits on reconstructing specific language families and thus their expansion episodes.

### 6.7 Generalisations in demography and through time

After all of the above illustrations of how significant demography seems to be in known language expansions in pre-Modern times, the reader might be forgiven for expecting that our genetic and linguistic lineages should match rather well after all. In practice, however, it is often observed that they do not. Many such mismatches may result from the very changed circumstances of the Modern Era, of course, which certainly have provoked massive language shift. But even before then it seems that language shift, rather than demographic replacement, has also been a significant mechanism for language dispersal.

Even within a fundamentally demographic model of language replacement it can be perfectly explicable, even expected, that linguistic and genetic ancestries will by no means show a perfect correlation. The mechanism of demic diffusion by a demographic ‘wave of advance’ – first proposed by Ammerman and Cavalli-Sforza (1984) and invoked by Renfrew (1987: ch. 6) for the farming/language dispersal hypothesis – can also accommodate smaller hunter-gatherer populations on the advancing wave front progressively switching to farming, just as their minority languages yielded to the expanding ‘farming language’. In the Indo-European case, then, what the hypothesis predicts is that the proportion of genes derived from Anatolia should cumulatively *decline* westwards across Europe.

In other disputed cases, demic diffusion is less appropriate, as for example across discontinuous territories such as the countless islands of South East Asia. Indeed, while there is consensus that the Austronesian languages spread out of a homeland on Taiwan, genetic data do not clearly support a majority genetic input from there. Notwithstanding all the counter-examples to the elite dominance model (section 6.3), then, in what other circumstances might a language be able to spread even when not spoken by the majority of a population?

Crucially, human population prehistory has not been a series of uniquely two-way encounters and ‘language contests’ that always pitted a conquering elite against a conquered majority speaking just a single language. Where an incoming group, even if relatively small, comes to be dominant instead over a fragmented patchwork of many minor local tongues, in speaker numbers the elite’s language may rank as at least first among equals, i.e. as one among a multiplicity of small language populations, with no one major rival. New Guinea presents just such a patchwork of acute linguistic diversity, hinting that a similar scenario may have held more widely across neighbouring Island South-East Asia too, before the Austronesian expansion overwrote it there.

In such circumstances the incomers’ language does not face an uphill struggle against drift to fixation in favour of a single numerically overwhelming native language. Rather, against any other single language it has rough parity in demographic strength, allowing its primacy in other respects to promote language shift toward it. One key element is the incomers’ ability to establish a new coherent unit or network, of far greater territorial range than any predecessors, and/or with more intense interaction across it. In linguistic terms, a new, wider speech community has been forged, within which the incomers’ language is unique in enjoying wide currency, prestige and utility right across it. *Lingua franca* status alone,

however, so often fails to guarantee *first-language* expansion and divergence into a family (section 6.2) that the incomers and their language must bring something more than trading utility, if native populations are to switch to it *en masse* as their new native tongue. In Island South-East Asia, the leading hypothesis sets the Austronesian language lineage within a far wider cultural complex brought from (pre-Han) Taiwan, including components of a new farming subsistence package and sophisticated seafaring technology, as well as a newly expanded trading network. Cases of cultural expansions of this nature, and not necessarily through military or demographic dominance, are far from rare in the archaeological record. This general “*primus inter pares*” vision (Heggarty and Renfrew 2014b: 556–557) may thus serve to model other major language dispersals through prehistory.

One final generalisation is a chronological one. Rolling back in time from the Modern Era towards the dawn of the Neolithic, and notwithstanding spectacular ups and downs, on a long view human societies tended overall to be of lesser demographic scale and complexity. Rather less acute too, then, was thus the scope for disparities between them in technologies and social organisation. Or in linguistic terms, ever fewer processes were yet in play that might so sharpen the disparities between human groups (and by extension their languages) as to propel just a privileged few of them to expand dramatically at the expense of their neighbours; ever fewer processes, that is, save for emerging contrasts in simple subsistence, and thus demography. The transition to farming and the Neolithic thus take on more plausibility for accounting for the *earliest* language family dispersals that we can detect. But over time, those early shapers of prehistory progressively gave way to the increasingly immediate impacts of many other phenomena arising and intensifying only since then (complex and highly populous state societies, major technological innovations, etc.). In more recent times, language expansions have come to be driven above all by these obvious proximate factors, very far removed now from any distant ultimate causation in the Neolithic (Heggarty and Beresford-Jones 2010: 165–169).

## 7 Prospects for a cross-disciplinary prehistory

One overall judgement emerges from this survey of how historical linguists have traditionally sought to set language (pre)histories into real-world contexts: all our traditional techniques and models are less reliable than the discipline has long liked to believe. The perspectives from (pre)history and archaeology only compound the doubts. The same technique can be open to opposing interpretations, and different techniques often contradict each other on the same language family. Convincing ‘proof’ is hard to come by indeed. To be realistic, the prehistorian’s task usually comes down to weighing up a balance of plausibilities in the light of the multiple perspectives from the various methods. Linguistics alone cannot come to the most plausible overall scenario for the prehistory of the populations involved. That can be assessed only in the light of the archaeological and genetic records, and the cause-and-effect relationship that links them all.

These are no grounds to finish on a low note, however. Those other disciplines continue to make spectacular leaps forward, and in historical linguistics itself, dropping the mask of many ‘old certainties’ only throws open the potential for great advances towards a sounder, truly cross-disciplinary understanding of prehistory. New methods such as Bayesian phylogenetics, provided they are intelligently applied, are already challenging established views and forcing them to be either justified, or abandoned.

Finally, there remain further aspects of the linguistic record hitherto largely untapped, but which also have much to teach us of prehistory. They lie beyond the confines of historical

linguistics proper, however, so this chapter will close just by signalling their potential (for more discussion, see Heggarty and Renfrew 2014a: 23, 41–42). For the global linguistic patchwork is not only made up of the language families that are the heart of traditional historical linguistics. Any major family attests to the expansion of just a single linguistic lineage, often exclusively over a large territory, and with significant *divergence* within it. Wherever those families do not reach, the linguistic picture can be conspicuously the converse, in two different senses: patterns of *convergence* into linguistic areas, and/or hotspots of acute diversity in language lineages. New avenues are at last being explored, but here too there is much that linguistics might gain from working in concert with the complementary perspectives of genetics and archaeology. Even for the world's largest language families, that synthesis still has far to run. For language convergence areas and diversity hotspots, we have barely even begun to unlock the cross-disciplinary potential, so as to round out the rich tale that our languages can tell us of our past.

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