Agriculture and Language Dispersals

Limitations, Refinements, and an Andean Exception?

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Among the grandest and most controversial proposals for a holistic, cross-disciplinary prehistory for humanity is the hypothesis that it was the adoption of agriculture that lay behind the dispersals of the world's greatest language families. Conspicuous by its absence from this debate, however, is one of humanity's rare independent hearths of agriculture and pristine civilization development: the Central Andes. Here we look to this region's little-known language prehistory, particularly the initial expansions of its two major indigenous language families, Quechua and Aymara. We then set these linguistic scenarios alongside archaeological evidence on where, when, and how agriculture originated here. The different time depths of these processes appear to preclude any simplistic cause-and-effect relationship between the two. Yet we go on to identify significant idiosyncrasies in the origins and development of food production in the Andes, which call for a number of refinements to the basic agriculture–language dispersal hypothesis. These are framed within a generalizing principle able to reconcile the appealing explanatory power of the hypothesis at great time depths with a reining in of any claims to unique and universal applicability in more recent times. The Andean case ends up transformed—an exception that more proves the rule than refutes it.

In the search for the origins of human populations, each of a number of disciplines opens up its own partial window on our past, but the views from genetics, archaeology, and linguistics by no means neatly converge into a single, coherent prehistory. There is, though, one proposal that seeks to coordinate at least the archaeological and linguistic pictures at one crucial stage. This is the simple—in the view of its critics, simplistic—argument that it was the adoption of agriculture that provided the empowering force behind many of the greatest expansions of human languages. Among its first proponents was Renfrew (1987, 1989), who included demography/subsistence as the first of four models to explain language replacements. Diamond and Bellwood (2003) provide a recent review of the apparent strengths and weaknesses of this proposal as applied to nine cases of language dispersal across the world. The controversy that surrounds it, meanwhile, is neatly summed up by Golla, Malhi, and Bettinger's (2003) response that the hypothesis "more frequently distorts than illuminates the histories of the speakers of the world's languages."

In this article we attempt a fresh approach to the hypothesis and its attendant controversies. We do so first by clarifying a number of principles from linguistics that are indispensable to an understanding of how and why languages diverge at all. We then focus on one of only a handful of regions across the globe where agriculture and, ultimately, a pristine civilization arose independently: the Central Andes. For despite this region's undoubted potential to inform the debate, it has hitherto been essentially overlooked. The omission is all the more curious since the Andes clearly constitute an exceptional case for the archaeology–language dispersal hypothesis and a case study that turns out to be highly illuminating for both sides.

First Principles and Perspectives from Linguistics

Underlying much of the controversy are a number of issues of interdisciplinary misunderstanding. Many of the leading authors involved are not linguists, so it seems worth briefly restating the fundamentals of comparative/historical linguistics, as relevant to this debate (for a fuller survey, see Heggarty 2007). We illustrate these with examples from the best known of all the great language families, also the subject of one of

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the most controversial of all claims for an agriculture-led dispersal: Indo-European.

How Can Linguistics Inform Prehistory? Language Relatedness, Expansions, Driving Forces, and Time Depths

First, how is it that linguistics has a place in the study of human prehistory at all? The answer is simple. If we can look to languages—or to be more specific, patterns of language divergence—as informative of history, then this is only because those patterns were molded in the first place by what one might call the forces of history. It is a basic axiom of linguistics—and one somewhat contrary to popular perception—that both in principle and in practice all human languages are, to all intents and purposes, equal in their communicative utility. Whether and how certain languages expand and diverge at the expense of others that become marginalized and extinct therefore has essentially nothing to do with any intrinsic linguistic qualities of their vocabularies, grammars, or sound systems. It is entirely a function of demographic, social, cultural, and political forces, created by and acting on the communities that speak those languages. Among these forces are the size, density, and growth of a population; the degree and nature of its contact with or isolation from other populations; and its relative sociocultural or political power and/or prestige. The relationship here is one of cause and effect: real-world driving forces mold language divergence patterns. These patterns are thus a reflection—and a valuable surviving record, a linguistic history—of how those forces operated on given populations over time. Here we shall refer to this mixed bag of factors collectively as simply the "forces of history," admittedly a rather vague term but one we intend here in the sense of those forces that have the potential to determine language expansions.

That agriculture has a potential role as a motor for language dispersal is thanks precisely to its relationship to many of these forces. Much of the controversy attending the hypothesis, meanwhile, revolves around it being far from the only potential motor—hence the risk of overgeneralizing its impact to language expansions in which other demographic and/or sociocultural factors were actually more important.

Second, how is it that languages come to diverge from each other through time? It is a law of natural, living languages that they all inevitably change over time—often at very different rates but never at a rate of zero. As long as the population that speaks a language retains a high enough level of contact and interaction as a coherent unit, then each incipient change will end up being either adopted or rejected by the entire speech community, and their speech will continue as a coherent single language, despite changing constantly through time. However, if speakers of a language expand across so wide a territory that effective contact between the different areas within it becomes too limited, then different changes to the original common language can arise separately in those different areas. This is the simple process by which languages diverge: what was a single original language begins to distil first into different accents, then dialects, and eventually quite different languages, albeit all within the same language family. This is, by definition, what it means to state that two (or more) languages are related: that their lineages, whatever changes they have gone through, can both (or all) be traced back ultimately to the same single common ancestor or protolanguage. Any language family is, by definition, a result of divergence, that is, in the real world, a geographical expansion out of an original single-ancestor homeland.

Beyond determining this simple yes/no datum of whether any two particular languages are or are not related to each other, where the answer is yes, historical linguistics can also provide more refined data on how closely. A broad family such as Indo-European spans various levels of relatedness between its various member languages, for which a family tree provides a helpful first analogy. Tracing back from the present, we can first identify modern Romanian, Italian, Spanish, and French, for instance, as sister languages within the Romance family, in that all derived from Latin (alias Proto-Romance). One linguistic generation further back, Latin itself once had a number of sister languages within a wider Italic family. Another step back, one common interpretation sees a further intervening stage, a common Italo-Celtic branch. Finally, another generation or so further back, one arrives at the ultimate ancestor, Proto-Indo-European.

Beyond Proto-Indo-European, the trail runs cold. Despite speculative claims to the contrary, the so-called Nostratic hypothesis, the linguistic consensus is firm that no deeper relationship of Indo-European with any other known languages can reliably be demonstrated by any valid methodology (Campbell 1999). We return to this question of the validity of supposed macrofamilies in more detail below.

The fact that a broad language family may include a series of generations through time, as Indo-European does, brings us to the third form of data that linguistics can provide, critical for attempting to match up its scenarios with those of other disciplines. It is certainly the case that there is a basic, natural relationship between amount of language change or divergence and length of time elapsed. But it is by no means a strictly linear relationship, because of two complications in particular. First, as discussed below, the rate of change is far from even. Second, not all change is divergent: certain changes can occur in parallel or even cause related languages to reconverge somewhat. Nonetheless, as language change continues inexorably though time, the languages within a family tend to grow further and further apart. The degree of diversity across a language family—how much the languages within it have changed relative to each other—can therefore stand as a rough indication of the time span elapsed since its single ancestor language first began to diverge. This can be complemented by another rough linguistic proxy for time elapsed: an analysis of how much the modern descendant languages have changed relative to their common ancestor language, to

the extent that linguists have been able to reconstruct it in detail.

Methods have been proposed that try to derive from this approximate relation between time elapsed and degree of language divergence a precise linguistic dating mechanism: to determine the absolute time depth that corresponds to the divergence observed for any given language family. These methods include the so-called glottochronology (Swadesh 1955) and a new method proposed by Gray and Atkinson (2003) that enlists phylogenetic analysis techniques drawn from the biological sciences to try to estimate the time depth of language family trees. Unfortunately, neither method enjoys any status as a recognized, orthodox method of historical linguistics. Even if glottochronological results are still widely cited, few linguists take them at chronological face value. They are read not so much as literal dates than as a useful, at least standardized, expression of rough measures of divergence and genealogical depth within a language family (the underlying raw lexicostatistical data that the dates were extrapolated from in the first place). The root problem is, of course, that language change and divergence are acutely susceptible to forces of history and these are themselves highly unstable through time. There is thus no reason to expect language change and divergence to follow any remotely regular clock. Indeed, there is abundant evidence from known historical cases that they do not. (That said, the claim that rates might average out sufficiently in practice, for large numbers of related languages over long periods, remains more open to discussion.)

Notwithstanding this wide variation in rates, the fundamental principle that change and divergence increase cumulatively with the passage of time still stands. So while absolute dates remain in dispute, there is a strong measure of agreement in linguistics on the relative time depths of language families that stand at different extremes of the scale of degrees of interfamily diversity: from the most diverse families, such as Indo-European to the most compact, such as Quechua, the main surviving language family of the Andes. Along with this goes fair confidence, at least in broad order-of-magnitude time windows commensurate with a family's degree of diversity (for additional information, see CA+ online suppl. A, "Linguistic Dating Methods").

Figure 1 offers a broad comparison of the respective time depths of some of the major Old and New World language families while still recognizing explicitly the inherent uncertainty of absolute datings. We do this by including both the short and the long chronologies that have been variously proposed for given families, showing the large divergence between them. The respective time depths are based on the spread of estimates that emerge from the various approaches to dating the divergence of language families. Glottochronological results generally fall toward the short end of the scale, while the long chronologies fit in with a widespread perception that glottochronology typically significantly underestimates real time depths (for additional information, see suppl. A, "Long and Short Language Family Chronologies").

In number of speakers, Quechua is, in fact, the greatest surviving indigenous language family of the whole New World; alongside it, the only other language widely spoken in the Andes is Aymara (for additional information, see suppl. A, "Nomenclature for Languages of the Andes"). The map in figure 2 shows the modern-day distributions of each. For discussion of the time depths of the Quechua, Aymara, and Uro families, on which the chronologies shown in our figure 1 are largely based, see Cerrón-Palomino (respectively, 2000: 282, 286–287; 2003:329–333; 2007:90–94).

Observers from outside linguistics might be forgiven for feeling a little bemused by certain apparent contradictions in linguists' approaches to dating. A detailed attempt to clear a path through them can be found in work by Heggarty (2007: 321–325). In any case, for our purposes here, the issue is not so much the very debatable absolute dates but the big picture. As is apparent from figure 1, there is great variation in the relative genealogical depths to which different language families attest. The crucial point is this: linguists' confidence in relative degrees of divergence is much higher than in any proposed absolute datings. Whatever the debate on their absolute datings, the differences in scale in figure 1 are irrefutable truths on any of a host of linguistic criteria. The internal diversity within either Quechua or Aymara is far, far less than that within Indo-European or Afro-Asiatic. This forms our first key observation in this article: the greatest Old World language families, such as Indo-European, first began to expand at time depths that are orders of magnitude older than the Quechua or Aymara dispersals. Proposed absolute datings range from 6,000 to 9,500 years ago for Indo-European but from just 1,200 to ca. 2,500 years ago for Quechua. Whatever the uncertainties within these ranges, there is still no danger of mistaking the two.

Ultimate and Proximate Factors in Language Dispersals

It is above all in this respect that our case study region of the Andes stands out as a striking exception to the linguistic patterns that the Old World has led us to consider normal. And it is this contrast that puts the agriculture–language dispersal hypothesis into a valuable new perspective that can illuminate if not defuse much of the controversy that surrounds it.

The starting point is our recognition that agriculture is far from the only player among the many demographic and sociocultural forces that have the potential to precipitate a language expansion. In principle, any given language dispersal might be attributable to any such force or any combination of them. It is precisely this that the interdisciplinary approach to prehistory seeks to elucidate by applying the data from different disciplines to work out how strongly and in which contexts these various forces have acted through time—and it may thus explain observed language dispersals (and much else besides).

Yet in all the interdisciplinary research, the poor relation

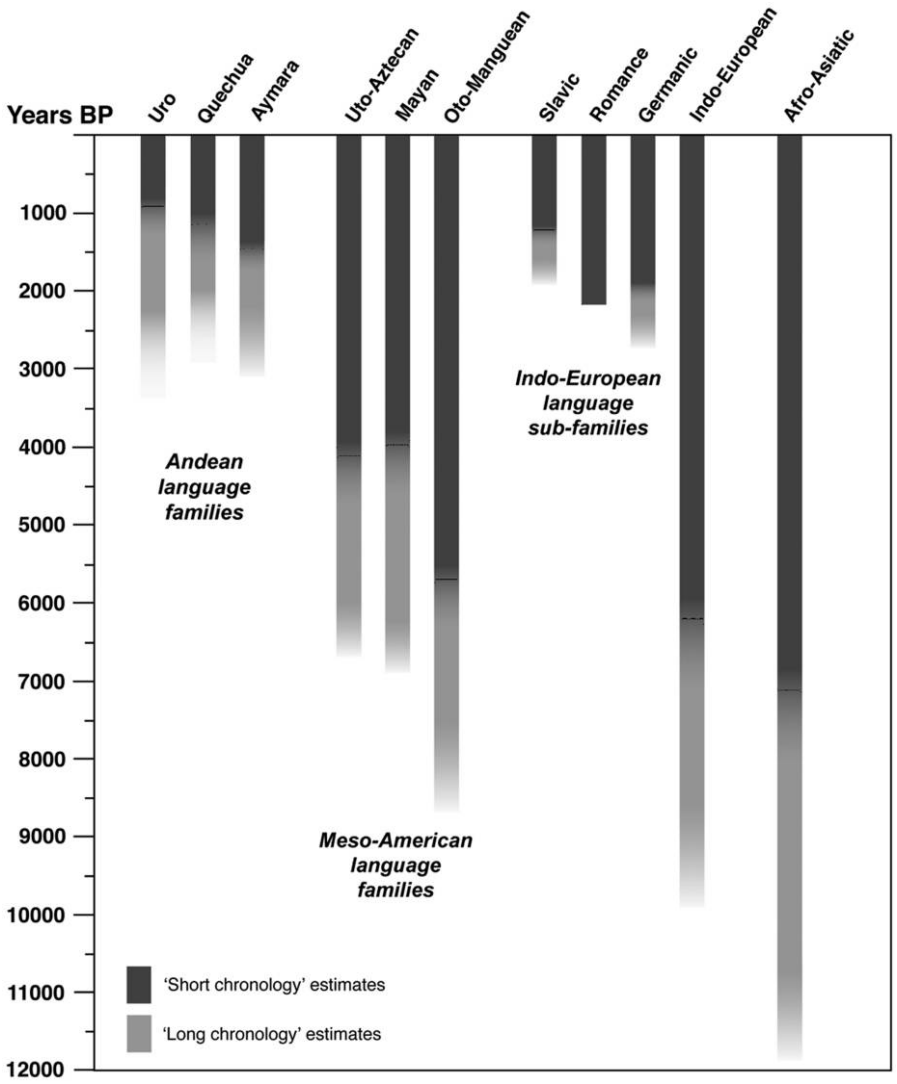

Figure 1. Time-depth estimates of major Andean versus Meso-American and Old World language families.

has remained the study of precisely how and why such forces sometimes succeed and sometimes fail in driving language expansions, a lacuna all too often filled by vague assumptions and appeals to one or another overall model (for a survey of proposals, see Renfrew 1987). Much has been made of the concept of elite dominance especially. But simply appealing to this model hardly takes us forward, when we are left having to explain how and why an elite's language is adopted in some cases, such as the English conquest of Ireland, but conspicuously not in others, such as the Norman conquest of England—for all the loanwords, this paper is not written in (Norman) French.

Despite the obvious challenge in discerning some structure among all these forces and assessing their relative significance at different times, there is one simple, principled way to make a start. We propose drawing one general distinction between "ultimate" and "proximate" factors, taking up two terms that already have established senses in a number of disciplines but not yet in historical linguistics. We shall apply them here specifically to distinguish among the range of factors that may drive language expansions. That is, at opposing ends of the scale, we can contrast ultimate or deep-time factors, among them the far-reaching impact of agriculture, with more proximate factors, such as the "guns, germs, and steel" of Diamond's (1997) title, and historical contingency.

In identifying any given factor as ultimate or proximate, we do, in practice, largely coincide with Diamond, but our use of these terms nevertheless differs in principle from how they are generally read in the biological sciences and elsewhere, in two main respects. First, we apply them here specifically to those forces capable of propelling language dispersals. Second, and more substantially, we neither assume nor imply

Figure 2. The two major language families of the Andes: present-day distribution.

any necessary cause-and-effect relationship from the ultimate to the proximate. If one follows Diamond, one might wish to trace proximate factors back, in any case, to foundation in ultimate ones, seeing "guns, germs and steel" as possible in the first place only in populations that had already become agriculturalists, even if many millennia before. In our own approach, to be precise, ultimate factors are seen as potentially necessary but not sufficient conditions for particular proximate factors to arise; the former may enable but do not cause the latter. Our scale of factors from the ultimate to the proximate is thus more a simply cumulative one, through time. We do not therefore assume that any proximate technology—such as the outrigger canoe, so critical in Oceania—can and must come about only through the ultimate causation of agriculture. Historical contingencies, too, rank at the proximate end of our scale, without any assumption that these can usefully or meaningfully be imputed to more ultimate causes.

Clearly, to differentiate between ultimate and proximate factors is to pin labels on the ends of what is really a continuum; our case study in the Andes will duly illustrate how

difficult it can sometimes be to draw the line between the two. Still, every continuum has its extremes, at which the labels certainly are valid, and generalization along the scale can be instructive. Indeed, we propose here a significant generalization, in the form of a default correlation between the scale of different time depths of language family dispersals and the continuum of forces that drive them. The most ancient language expansions will be most plausibly attributable to factors at the ultimate end of the scale: shallower language families, born of more recent expansions, to more proximate factors. Thus, the relative role of agriculture diminishes along the continuum as progressive developments in technology and social organization make for increasingly sharp relative advantages of one human society over another, especially either side of the 1492 watershed.

This correlation is proposed as a general principle whose precise application will vary considerably in context, given different cultural trajectories in different parts of the globe. (Again, our Andean case study will provide a valuable perspective.) We expect this correlation to be relatively uncontroversial; in fact, it might seem self-evident, even based on a somewhat circular definition. Nevertheless, it is worth stating explicitly since it allows us to see a way through much of the controversy that has attended the agriculture–language dispersal hypothesis.

For certainly, where critics have most decried the attempt to push the hypothesis too far is, as a rule, precisely where its most enthusiastic proponents have sought to invoke it to explain relatively recent dispersals of rather shallow language families. And rightly so, for it is in just such cases that the validity and the supposedly unique explanatory power of the hypothesis are most open to objection.

A case in point is the one that Diamond and Bellwood (2003:598) describe as "the colonizations of previously uninhabited Polynesia and Micronesia by Neolithic populations speaking Austronesian languages." Their critics retort, as per Golla, Malhi, and Bettinger's (2003) response, that this expansion "was driven by the development of sophisticated seafaring technology, not farming success," that is, a much more proximate factor than agriculture. It is one of our key points here, of course, as per our concept of a continuum of factors capable of driving language expansions, that to see the question in entirely exclusive terms either way is likely to be unhelpful. Nonetheless, this case is certainly one in which agriculture alone can hardly be seen as a sufficient condition. More curiously still, it is self-contradictory for Diamond and Bellwood (2003:598) to claim as one of their "two clearest examples" of "concordance of evidence" in "attesting the replacement of local hunter-gatherers by expanding farmers" this case of human expansion into "previously uninhabited" territory, where, by definition, there were no hunter-gatherers in the first place.

Similarly, the agriculture–language dispersal hypothesis is hardly particularly illuminating for the spread of European languages across the globe during the periods of colonialism and imperialism. It is not agriculture that can explain why it was specifically English, Spanish, and Portuguese that spread to become the languages spoken by most people in the Americas today, rather than French, German, or, indeed, Hindi or Chinese, whose speakers had no less an agricultural advantage at the time. By the same token, it is questionable for Diamond (1997, table 18.2) to attribute to food production the spread across Siberia of Russian (again, why not Chinese?), as late as 1480–1638. By this late period, as in the European colonization of the Americas, much more proximate factors and historical contingencies were indisputably at play. It is only these that can meaningfully account for which particular languages spread (and which did not), in which regions and at which times.

It is true that on the grandest scale one might, à la Diamond, seek to trace these proximate causes further and further back to ultimate ones. Nonetheless, even the ultimate factor of agriculture versus hunting/gathering cannot be applied to the European language expansions over large parts of the New World, for these were already highly developed agriculturally themselves. Advocates of the hypothesis typically retreat here to the position that it was not agriculture per se but rather some relative subsistence superiority of the Old World over the New World agricultural packages that drove European dominance, but we see little evidence for this either. The people of the Americas have continued to thrive on native agriculture (maize, potatoes, beans, tomatoes, etc.) and live no more off European crops than Europeans live off theirs. As Hemming (1970:59) dryly remarks, "The world's annual potato harvest is worth many times the value of all the treasures and precious metals taken from the Inca Empire by its conquerors." It is hard now to conceive of Mediterranean or Far Eastern cuisines without the American tomato or chili pepper. Other than for livestock, the Columbian exchange of agricultural packages (Crosby 1972) by no means flowed asymmetrically from the Old World to the New, rather than vice versa. This agricultural story can therefore hardly be invoked to account for a linguistic one that stands in radical contrast to it: not an exchange but an utterly one-sided domination of the languages of the New World by those of the Old.

What does explain this straightforwardly, of course, is a panoply of well-known demographic, social, political, cultural, and technological factors and historical happenstances. Whatever ultimate causes one might try to trace these back to on the grandest scale, as a practical, direct explanation for the expansion of any particular language one must, in the first instance, invoke any evident proximate factors and historical contingencies.

Advocates of the hypothesis are quick to object that they by no means claim it has unique, universal applicability (e.g., Bellwood 2005:2). The impression among their critics, however, is that they do indeed err in precisely that direction. The expansions of European languages to the New World and across Siberia and the seaborne spread of Malayo-Polynesian

are cases in point: to see these as driven primarily by agriculture rather than by a host of other relevant factors is greatly to overstate the case. This tendency on the part of its advocates to overegg how uniquely powerful agriculture is as an explanation for language dispersals and how widely it applies is unfortunate, for it does a disservice to what remains a significant insight. Much of the attraction in how we propose to link the time-depth scale of language dispersals to the ultimate-proximate scale of driving forces is precisely that it helps rescue the agriculture–language dispersal principle by limiting the number of apparent exceptions to it. It does so by the radical but necessary step of recognizing explicitly the clear limits to any aspiration to universal utility and unique explanatory power at the shallower, more recent end of the language dispersal continuum.

Do We Need an Agriculture–Language Dispersal Hypothesis at All?

As we discard this bathwater, though, we should take care not to throw out the agriculture baby. For the fact remains that no other force of history created by humans is remotely comparable in magnitude. As Smith (1995:16) observes, the cumulative transformations wrought on our planet's ecology by agriculture are broadly comparable to those of the Cretaceous-Tertiary extinction event some 65 million years ago, which saw the extinction of some two-thirds of Earth's species. So while agriculture is certainly not the only force capable of promoting a language dispersal, to relegate it to the rank of just one among many is to miss a critical point of scale, particularly at greater time depths (see also Bellwood 2005: 10).

Language dispersals, too, must not be taken for granted. They do not just happen; they happen only for very good reasons in a real demographic and sociocultural context. If we aspire to a plausible account of our origins, then it is inadequate for us not to propose explanations on appropriate scales—and all the more inadequate the bigger the language family we are called on to explain.

Indo-European is very much a case in point. As its name suggests, this single, broad language family came not only to dominate Europe but also to extend across the whole of northern India and parts of the Middle East in between. Moreover, the divergence of this family, estimated to have begun at least 6 or perhaps even 10 millennia ago (see below), was largely played out long before the more proximate factors that have so transformed the world in historical times.

It is all too easily passed over just how remarkable a linguistic pattern Indo-European represents. From Bangladesh to the Algarve, scores of languages are spoken that all derive from what the linguistics unfailingly tells us was once just a single ancestor language, indeed one sufficiently homogenous that it must originally have been spoken only in some narrowly circumscribed geographical area. An expansion so vast, out of such humble beginnings, demands accounting for. The explanations are necessarily to be found somewhere among the various forces that are susceptible of driving language expansions, and for a dispersal of the scale of Indo-European across both time and space, we must seek a proportionate motor.

It is hardly surprising, then, that among the rival theories as to where and when the Proto-Indo-European ancestor language was originally spoken, one of the two main contenders duly seeks to trace it back specifically to a very early agricultural homeland: in eastern Anatolia, as far back as 9500 BP. The coming of agriculture is precisely the driving force that Renfrew (1987) posits as primarily responsible for the initial language dispersal.

This proposal, among the earliest claims to be made for an agriculture-led language dispersal, has met with particular controversy. The main rival hypothesis identifies speakers of Proto-Indo-European instead with the so-called Kurgan culture of the Pontic-Caspian steppe (Gimbutas 1970; Mallory 1989), thus assigning the start of Indo-European expansion to a much later time frame of ca. 6500 BP, a date more happily coincident with the entrenched view of the family's time depth traditional among specialists in its linguistics. The controversy has tended to center on this issue of chronology, though without much advancing us since all proposed absolute dating methods have met with serious methodological objections.

Many archaeologists, meanwhile, are bemused to hear specialists in Proto-Indo-European still invoking so enthusiastically a linguistic correlate to Gimbutas's and Mallory's Kurgan construct. For in their discipline, the whole concept of a Kurgan steppe culture, let alone one able to effect such an overwhelming linguistic impact over such a vast area, is viewed as suspiciously nebulous on all manner of archaeological grounds (see, e.g., Levine et al. 1999). As for Renfrew's hypothesis, archaeologists stress a number of complications and objections to any straightforward vision of the "coming" of agriculture and to subsuming such complex and protracted demographic and linguistic processes under just one overriding, ultimate explanation.

Seeking to clear a path through some of these complications, we can start by again invoking the distinction between ultimate and proximate factors. Unquestionably, proximate factors have radically redrawn the linguistic map of the Indo-European sphere. Not least over much of Europe, they have over the past 3 millennia effected a sequence of dramatic language expansions: first of the Celtic subfamily of Indo-European, then of Romance, then Germanic, and then Slavic, each overlying the previous ones to greater or lesser extents. Certainly, each of these major subfamilies owes its particular recent and present ranges not to the first coming of agriculture but to events many millennia later, toward or within historical times, above all the establishment of the Roman Empire and the Great Migrations that accompanied its fall.

That said, all these proximate overlays cannot deny—nor should they be allowed to mask—the linguistic impact that more ultimate forces had in earlier times. Indeed, for all the

turbulence of these four major language expansions in Europe, it is as if they have all simply overlain in different shades a picture whose basal color has remained a resolutely stable Indo-European "hue" that goes back to the much earlier ultimate factor expansion, from which each of the local, proximate overlays was necessarily mixed. That all four were of Indo-European languages seems less convincingly attributable to chance than to the strength of the ultimate factor responsible for the linguistic picture on the broadest level: from much earlier times, languages derived from a single ancestor were projected across a great span of Eurasia. For advocates of the Anatolian hypothesis, it remains its abiding attraction that it seeks to match this extraordinary linguistic pattern with the most fundamental alteration of human society that was the transition from hunting and gathering to agriculture.

The details of the Indo-European debate lie beyond the scope of this article. Here we aspire only to contribute to the weighing up of plausibilities between the rival theories from the new perspectives on the agriculture–language dispersal hypothesis offered by our case study in a radically different context: the Central Andes.

An Overview: Old World versus New?

Both in individual cases and as a general principle, the debate remains very much open as to whether the agriculture–language dispersal equation is a valid, fundamental insight into human prehistory or a simplistic and unproven generalization. From our generalization to correlate our two continua as set out above, it follows that where the agriculture–language dispersal hypothesis might in principle aspire to real value and explanatory power is not at proximate time depths but for the deepest language families, those that have been diverging since the earliest dates. For if the advent of agriculture remains the single most significant transformation of human society and ecology, on which all subsequent transformations rest, then it is only logical that at this deep-time end of the scale of language expansions its impact must weigh heavy. It is this that confers on agriculture a potential role uniquely commensurate with the very largest—and particularly the very earliest—language expansions that we can trace.

On a cursory overview, the odds can appear to be on the side of agriculture, at least to judge from Diamond's (1997, table 18.2) summary of the ultimate driving forces behind 10 of the largest language dispersals across the globe. Our generalization appears to escape Diamond, however, precisely because he insists on imputing even the most recent language dispersal in his table, that of Russian across Siberia, to food production. Once discarded, for the reasons already discussed, then of nine remaining cases, seven are attributed to food production. These seven are also all the major and oldest language expansions, precisely in line with our proposed generalization above. Conversely, just two expansions, specifically the most recent and both relatively minor ones in demographic terms, are imputed to horse-based pastoralism: Hungarian and Altaic (which many linguists do not in fact recognize, though the association with horse-based pastoralism does at least hold separately for the Turkic and Mongolic language families). Indo-European is left diplomatically undecided between the two, though as the most expansive and among the oldest of all these language dispersals, it would be decidedly bucking a trend if it could satisfactorily be explained by horse-based pastoralism, which otherwise accounts for language dispersals only at the other most recent extreme of the scale.

Diamond's survey is limited to language expansions in the Old World. But what of the New World? For if the agriculture–language dispersal principle is so fundamental, then surely it might be expected to apply across the globe. And the New World is, of course, home to two of the handful of instances of independent origins of agriculture.

Before we look to agriculture there, however, we must from the outset discard the grandest claim surrounding the languages of the New World, the so-called Amerind hypothesis of Greenberg (1987), subsequently defended by Ruhlen (1994) within his protoworld proposal. In disciplines outside linguistics, much has been made of this claim that the great majority of the languages of the Americas can be grouped into a single, vast, and deep-time macrofamily, Amerind, rather akin to the Nostratic hypothesis mentioned above in the context of Indo-European. For the linguistic consensus, however, this Amerind construct is nothing but a castle in the air. Ringe (2000:155), for one, demonstrates how the claimed mass (or multilateral) comparison method it is founded on can be "refuted with simple textbook mathematics." Compounding this is Greenberg's approach to the actual language data: in those he cites for Quechua, for instance, "the number of erroneous forms probably exceeds that of the correct forms" (Adelaar 1989:253). So however beguiling it may be to imagine that Amerind might be some last faint linguistic vestige of the ancient first settlement of the Americas, the supposed pattern turns out to be no more than a face in the fire: tempting to believe in, perhaps, but for the linguistic mainstream more imagination than science. (For an attempt to clarify and defuse the cross-disciplinary misunderstandings and frustrations on the macrofamilies issue, see suppl. A, "Mass Comparison and Macrofamilies.")

Discarding Amerind, then, we can now survey the broad-scale linguistic picture in the two main hearths of agriculture in the New World, one of which duly appears not so different from that of the Old World. Meso-America is home to a concentration of some of the broadest and deepest language families of the Americas: Uto-Aztecan, Oto-Manguean, and Mayan. The genealogical depth and degree of divergence within each of these is doubtless rather less than in the greatest Old World families such as Indo-European and Afro-Asiatic (fig. 1). Nonetheless, they are at least of the same order of magnitude as the next rank of major Old World families and are usually estimated to date back to time windows of between 4 and 7 millennia BP (Kaufman and Golla 2000:52; for further

information, see suppl. A, "Long and Short Language Family Chronologies"). This sets the initial dispersals of each of these families roughly in line with assumed dates for the most significant stages in the domestication in southern Mexico of the major indigenous crops (Smith 1995). It is true that, geographically, these language families are much less expansive than their Old World peers, though for the agriculture–language dispersal hypothesis, this would simply follow from agriculture itself not spreading far afield out of Meso-America at those deep-time periods. Debates on chronology continue in both archaeology and linguistics, but the broad time frames do at least appear compatible with an association between the beginnings of agriculture and the start of one or more of the major language expansions in Meso-America.

Looking southward, however, the picture seems much less convincing. To start with, the Andean region, by any measure one of the major hearths of human agriculture, is conspicuous by its absence from the detailed debate so far. Not only do Diamond and Bellwood (2003:599) prefer to sidestep it but also the region is the subject of not a single article, even in collections such as Bellwood and Renfrew's (2002) or any of the four *Archaeology and Language* volumes edited by Blench and Spriggs (1997–1999). And while Bellwood (2005) does ably review the origins of agriculture in the Andes, he assigns little more than a paragraph to Andean linguistics, and the few works he cites pass over all the standard texts in the field, such as those by Cerrón-Palomino (2003 [1987], 2000), Torero (2002), and Adelaar with Muysken (2004). Ultimately, Bellwood (2005:237) finds himself unable to elicit support for his overall agriculture-linguistics hypothesis from the Andean case and concludes simply that "we can make no further progress as far as South America is concerned." One might be forgiven for finding this omission even slightly suspicious, given that the view from the Andes appears at first sight to pose a clear challenge to the agriculture–language dispersal hypothesis; we shall in fact suggest a more anodyne explanation.

The Andes and Agriculture: Clarifying Terms

Before we start, some clarification of our terms is in order. We use "Central Andes" here to denote that area of South America in which complex society attained both its earliest and ultimately its fullest expression: in short, civilization. While "Central Andes" is certainly common currency among Andeanists, defining this area more precisely in time and space has naturally provoked much debate and continual revision. Here we take the term to refer to all of Peru's western Pacific coast; the central Peruvian highlands, extending also into the Altiplano of northern Bolivia; and the adjoining eastern slopes of the Andes as they descend to the *ceja de selva*, the fringe of tropical rainforest that lies above the level of the seasonally flooded Amazon basin (see Lathrap 1970). Conveniently, this area also broadly matches what, on independent linguistic grounds, Adelaar with Muysken (2004) delimit as their "Inca Sphere"—albeit with some additions, notably Ecuador.

Of course, the areas where complex societies first arise rarely coincide precisely with those where domestication first began—a point we return to shortly in our review of the agriculture–language dispersal hypothesis. The Central Andes are a case in point: for this "Peruvian Culture Area" (Willey 1971) does not necessarily include all of those zones in which processes of plant domestication first began, which may well extend to lower altitudes farther east, for instance, into Amazonia proper. We do, however, mean these areas to be included in the term "Andes" when used alone without qualification, to be construed here in the broadest sense of the entire western portion of South America, including Ecuador, Colombia, the rest of Bolivia, Chile, and northwest Argentina.

The Andes: A Curious Omission

The impact on Europe of the discovery of the Americas (and of course vice versa) is widely appreciated. All too often overlooked, however, is the significance of the contribution that millennia of plant manipulation in the Andes still make to human subsistence today. When Harlan (1975:233) identifies the 15 staple food crops that "really feed the populations of the world," it turns out that no less than six of them originated or at least developed most of their diversity in the Andes. These are maize, potatoes, beans (of the *Phaseolus* species), manioc, sweet potato, and peanuts (as the source of groundnut oil). Other important South American domesticates include cotton, varieties of squash, chili peppers, cocoa, and even tobacco and coca.

Why is it, then, that this core agricultural region of the world has been so neglected? For Diamond and Bellwood (2003:599), the answer is that "linguistic relationships . . . in the Andes–Amazon are too uncertain to test [the hypothesis of an agriculture–language link] because of migrations and disease-caused language extinctions after European colonization." Yet these same limitations apply equally to Meso-America, and this has not prevented Diamond and Bellwood (2003), among others, from arguing that the region presents a fairly strong case for a link between agricultural origins and language dispersals.

In any case, Diamond and Bellwood's claim comes as news to specialists in the Andean languages themselves. It is true that expertise in the linguistics of the Andes is confined to a relatively small group of specialists, but, among them, confidence in the essential linguistic facts is of the same order as in Meso-America. What is different, when that expertise is brought to bear on the question, is simply the story that those data tell.

For Adelaar with Muysken (2004:22) the "most striking feature" of the linguistic panorama of the Andes (including our focus here, the Central Andes) is "the number of irreducible genetic units," that is, language families unrelated to any others. We would turn this somewhat differently, since

for our purposes here the issue is not so much how many (unrelated) families there are but how shallow their time depths are. For among the indigenous languages of the Central Andes, broad, deep language families are simply not to be found. Each of the main two surviving families, Quechua and Aymara, encompasses a degree of internal diversity that is distinctly limited by the standards of even the Meso-American families (see the comparisons in Kaufman and Golla 2000: 52), let alone the likes of Indo-European or Afro-Asiatic. The depth of Quechua or Aymara is comparable with just the very last generation of Indo-European. That is, diversity within each family is of the same order as that of Romance languages—indeed, if anything, slightly less (see fig. 1).

So while a precise absolute chronology remains beyond us, there is no doubt as to orders of magnitude of divergence relative to other language families for which we do have a dated historical context. On the one hand, this is more than enough to dismiss the popular myth that attributes all Quechua to the much later Inca expansion. On the other hand, and more important for our purposes here, not a single language family can be found in the Andes whose divergence can plausibly have begun more than 3 millennia ago (see Heggarty 2007:327–33; 2008).

Diamond and Bellwood's explanation for this state of affairs—that we simply lack the linguistic data to tell—seems to stem from a misinterpretation of what it means to say that language families of greater genealogical depth cannot be found in the Andes. First, one must beware the temptation instinctively to assume that just because we find families of great time depths over much of the Old World, then such linguistic patterns are somehow the default expectation for the Andes, too, and thus that if we cannot detect them there, then this can be only for lack of data. Such a vision would hardly be shared by most linguists experienced in the study of the world's linguistic diversity through time and across geographical space. On the contrary, from that perspective, it is the time depth and breadth of Indo-European that appear startling, especially when one recalls how small and tightly circumscribed a single language the whole family ultimately goes back to.

Second, the qualifications that necessarily accompany linguists' careful formulations of what we can say about language relatedness, the provisos of "as far back as we can tell" unfortunately lend themselves all too easily to misinterpretation. Pace Diamond and Bellwood (2003), it is not the case that our linguistic data in the Andes fail to pass the threshold of what is needed to make reliable judgments of relatedness. On the contrary, they do, hence the known families such as Quechua, Aymara, and Uro. Rather, it is that these language data show clearly that no claim for relationships at deeper levels even approaches the required threshold of linguistic evidence. This applies equally to all the other languages of the Central Andes that have survived into our linguistic record in historical times. As an indication of how radically different they are from each other, see the survey and measures of typological distance of Torero (2002, sec. 6). In short, that linguists cannot find broad, deep families of the order of those of the Old World or even Meso-America is not because we have insufficient data to tell. We have plenty, and they all point unfailingly to the conclusion that there were simply no such families there to be detected in the first place.

What may also contribute to the confusion is that in the early decades of Andean linguistics, coincidentally the heyday of now-discredited approaches such as glottochronology and then mass comparison, there was one proposal for a broader, deeper family entertained for a while by some enthusiasts. This was the so-called Quechumaran hypothesis, that at a much greater remove Quechua's and Aymara's ancestors in turn go back ultimately to a single common ancestor. "Notwithstanding the lack of proof," as Adelaar with Muysken (2004:35) mildly put it, this outdated idea is still occasionally aired among Americanist linguists who do not specialize in the languages of the Andes. Among those who do, not one signs up for it.

Granted, one cannot, in principle, exclude shared origins in an extremely remote past, beyond the time-depth limits to which our linguistic methods can "see," but this, too, should not be misinterpreted and is effectively valueless. We side firmly with Torero's (2002:154) salutary methodological rigor in insisting that a vague hedging of bets will not do, for the evidence is more than ample for explicitly dismissing any specific Quechumara construct, as per the latest findings of Heggarty (2005, 2008).

For all practical purposes, including any putative association with the origins of agriculture, Quechua and Aymara are not related. Certainly, had they been related at a time depth of the order of the three main Meso-American families, this could and would already have been demonstrated convincingly. That it cannot be at all is not for lack of the appropriate data on whether they were related but simply because they were not. Hypotheses for Quechua's and Aymara's origins (Cerrón-Palomino 2000, 2003; Torero 2002) therefore envisage separate homelands, though allowing for the periods of intense contact between the families, which can much better explain the nature of the parallels observed between them.

First Impressions: The Andes as a Counterexample

So the problem in the Andes is not in fact that the linguistic data are unclear but that they are less widely known, are often misinterpreted, and, above all, do not appear to tell the expected story for the agriculture–language dispersal equation. For the most striking feature of the linguistic panorama of the Central Andes is the glaring absence of any language family of remotely the same scale and time depth as the great language families of the Old World or even of those of Meso-America. And yet the Andes stand squarely alongside those regions as unquestionably in the first rank of the world's major independent hearths of agriculture. At first glance, then, the Andes do indeed seem to constitute a stark counterex-

ample to the proposal that the development of agriculture provides such a powerful motor to drive great language expansions. For if it does, then what happened to this force in the Central Andes, where it appears signally to have failed?

Refining the Agriculture–Language Dispersal Hypothesis

Not only does the case of the Andes require closer investigation but so does the nature of the agriculture–language dispersal hypothesis itself. Certainly, it is rather unfair to present it as crudely as we have so far, for even its proponents explicitly recognize that it requires some qualification. To follow Diamond and Bellwood (2003:598), we can start out from what might be termed the "strong form" of the basic cause-and-effect logic: "that farmers and their culture replace neighboring hunter-gatherers and the latter's culture" and therefore that human genes and languages dispersed together with prehistoric agriculture.

In this strong form, the hypothesis has been justifiably criticized; Diamond and Bellwood (2003) themselves acknowledge six complications. Several of these are targeted at the debates that surround claimed correlations between patterns in human genetics and dispersals of agriculture and languages. Indeed, it remains unclear to what extent and in which cases agriculture spread either at the demographic expense of hunter-gatherers or by hunter-gatherers picking up agriculture from neighboring populations.

Here we prefer to set issues of human genetics aside, as a complication and distraction from our focus. That we feel able to do so is because we take no particular a priori position on the above issues; indeed, we deliberately steer clear of any assumptions either way. We aspire only to isolating an agriculture-language relationship or, to be more precise, an agriculture–population–language dispersal equation. But "population" here means those who have either inherited or adopted agriculture and/or a given language, if not both together within a larger cultural package. As historical linguists are acutely aware, it must not be underestimated just how often languages are transmitted horizontally between populations, breaking the chain of vertical transmission through the generations. Consider the modern English-speaking population of the United States, for instance: only a relatively small proportion of their ancestors 5 centuries ago spoke what has become their own native tongue today.

Another issue on which we limit our assumptions is the direction of the cause-and-effect relations in the hypothesis. Certainly, it is language divergence that could be driven by agriculture, not vice versa. Beyond that, however, we make no particular assumptions as to the causal relationship between the agricultural threshold and population elements in our equation. That is, we do not speculate here on why agriculture might be adopted or on the precise mechanism(s) by which it is spread.

Yet even with all these assumptions removed and Diamond and Bellwood's (2003) six complications recognized, the strong formulation remains unsatisfactory on a number of levels. All these refinements still fail to advance significantly the debate on the Indo-European language dispersal, for example. For our case study here, too, it transpires that none of the complications that Diamond and Bellwood list are relevant to explaining why the Andes show no evident trace of a link between agriculture and language dispersals; rather, the Andean context calls for a number of quite different refinements.

So to continue clearing a way through the controversies surrounding the hypothesis, we propose the following more qualified formulation:

1. The transition from hunting and gathering to the control and cultivation of food sources (ultimately, agriculture) is in no sense an event but a protracted coevolutionary process between humans and parts of their environment (Rindos 1980). This gradual progress can, however, be punctuated by evolutionary jumps through occasional mutations that are then selected through human agency. From this perspective, domestication is defined as "genetic change in population because of interaction with humans that leads to a dependence relation," and agriculture, the outcome of this process, is "the mutual dependence of crop plant[s] and humans" (Benz and Staller 2006:665).

2. This process is a gradual one of trial and error, as different components come together in different ways to form and reform various agricultural packages. Nevertheless, at some point(s) along it, an intensification threshold is crossed, at which a particular package coalesces that confers real net advantages on sociocultural and demographic levels, not least in that far greater population densities can be supported.

3. Crucially though, to confer significant linguistic impact in propelling a major dispersal of language, the agricultural package must also confer geographic expansiveness, as well as driving demographic growth through intensification. That is, the package must form what Jones (2007:144; italics added) calls a "*mobile* food chain": one that can relatively easily be adapted and propagated successfully to surrounding regions. Typically, this all-round "intensification and expansiveness" combination is conferred by the inclusion in the package of two key components. Above all, it needs the robust, high-yield, storable starch source of a true cereal—which, in many contexts, also requires the ability to control growing seasons through agricultural technologies such as terracing and irrigation. Additionally, it calls for a source of protein: ideally, large domesticated animals. Significant linguistic impact is likely only when this expansive intensification threshold is crossed.

4. This threshold may, of course, be crossed far from where domestication processes first began. Moreover, the level at which significance thresholds are set will vary considerably from one regional context to the next, as our Andean case study will show.

5. Once some group(s) of people cross this expansive in-

tensification threshold, they and/or their (agri-)culture are likely, through a process that itself may or may not have considerable time depth, inexorably to expand geographically. The reasons for this include the higher food yields and more complex sedentary societies that are associated with agriculture (Diamond and Bellwood 2003).

6. As this expansion proceeds, the native tongue of these agriculturalists spreads, too. Yet the further it expands and the weaker the contacts right across this growing territory, the more this speech will, over time, naturally diverge from one area to the next. Their original single common language fragments into a series of accents, dialects, and, eventually, a family of quite different, albeit ultimately related, languages.

7. The resulting large-scale linguistic picture, even after many millennia, is of a great span of contiguous territory across which are spoken languages that belong to a single, broad family, that is, all ultimately derived from the same common ancestral tongue.

A New Look at the Andes

Idiosyncrasies of the Andean Context for the Development of Agriculture

The refinements proposed above are potentially necessary in all instances but nowhere more so than in the Andes. For on closer inspection, the origins and development of agriculture there are seen to be characterized by a number of crucial idiosyncrasies that have a direct bearing on any attempt to apply the agriculture–language dispersal principle to the region.

We begin with an overview of the archaeological story of how agriculture developed here. Although significant uncertainties remain, we aim here to make some broad, state-of-the-art observations that do make it possible to draw meaningful contrasts between agricultural origins in South America and elsewhere.

1. As originally proposed by Sauer (1952), agriculture arose largely independently in Meso-America and South America. In each region, so-called vicarious species underwent parallel domestications from separate wild ancestors—remarkably, in no less than 12 genera, including beans (*Phaseolus* spp.) and squash (*Cucurbita* spp.), the two other members, alongside maize, of the famous New World food crop triad (Smith 1995). The potato and domesticated camelids did not spread to Meso-America in pre-Hispanic times. Diamond (2002) has argued cogently that this lack of shared domesticates may go back to the impediments to adaptation imposed by the tropical lowlands that lie between the temperate highland areas of South and Meso-America and, ultimately, to the north-south versus east-west axes of the continents.

There is, of course, some evidence that some lowland roots and tubers—such as manioc, sweet potatoes, and arrowroot—were exchanged between the tropical forest regions of the Americas from very early dates (see, e.g., Piperno et al. 2000). Nonetheless, these are more exceptions that prove the rule that, in general, interchange between the two foci of complex society in the New World was severely restricted by biogeography. South American domesticates do not appear to have formed a significant part of the subsistence foundation of any Meso-American complex society and vice versa—but for one crucial exception, maize, to which we return shortly. And whatever the causes and minor exceptions, this general independence between the New World hearths stands in marked contrast to the spread of a package of seven major plant and animal domesticates from the Fertile Crescent across so much of the Old World (Smith 1995:51).

2. In fact, the Andes themselves did not constitute a single hearth of domestication, even in the disparate sense in which such geographical areas of origin are now understood. Vavilov (1992 [1940]) originally reasoned that centers of origin of agriculture worldwide were those regions that host the greatest genetic diversity among modern crop landraces (reminiscent of a parallel rule of thumb proposed for searching for language family homelands; see Heggarty 2007:326). Flaws in this assumption are now appreciated: plant domesticates can originate in one location and develop their diversity elsewhere (Smith 1995:6). Nevertheless, it remains noteworthy that this methodology identified three separate subregions of western South America that display conspicuous landrace diversity. Indeed, following Harlan (1971:472), many have observed that the Andes are more of a noncenter of plant domestication: a large region within which different plants were domesticated in different areas and over different timescales (see Pearsall 1992, 2008:106, fig 7.1). Scrupulously, of course, this is true of any of the so-called hearths of agriculture, but the point here is one of scale. The Andes form the most geographically diverse region on Earth. An east-west transect from the desert Pacific coast to the Amazon jungle across their several ranges, second in altitude only to the Himalayas, encompasses a diversity of ecological zones that scholars have struggled to classify satisfactorily ever since the days of Von Humboldt. Of Holdridge's (1967) 103 world "life-zones," no less than 84 are to be found here. The Andes are also unique among Alpine regions in that they span the tropical latitudes and can therefore support cultivation and dense populations even at great altitudes. The origins of agriculture were thus played out across some 4,000 m of altitude variation with extraordinary, tropical environmental diversity across "horizontally condensed" space (Shimada 1985:xi). Unsurprisingly, then, domestication processes generated at least three quite distinct complexes of taxa, assigned various labels by various scholars; Harlan (1975:76) terms them "high elevation" (potato, other tubers, quinoa, and camelids), "midelevation" (various beans, peanut, coca, yam bean, etc.), and "lowland" (squash, cotton, various chili peppers, sweet potato, tobacco, manioc, and a range of fruits).

3. The deep submarine trench along the Pacific coastline of the Andes generates nutrient upwellings that support one of the world's richest marine ecosystems. These marine re-

sources originally formed the subsistence basis of early sedentary societies along the Peruvian coast. Indeed, since Moseley (1975), some have argued that they also, perhaps uniquely worldwide, laid the foundations of the first complex societies. Exactly how the region's mixed marine-agricultural economies evolved as societies began to form during the Preceramic period is still a matter of some debate (see, e.g., Dillehay, Bonavia, and Kaulicke 2004; Haas and Creamer 2006; Quilter and Stocker 1983). Nonetheless, it remains perfectly clear that marine resources continued to form a major component of human subsistence on the coast and the primary source of protein long after the development of agriculture.

4. As is widely known, the New World offered precious few meat-producing herd animals suitable for domestication (Diamond 2002). In the Andes, camelid domestication did at least provide the basis for some forms of pastoralism, but these were limited by Old World standards, not least in that they never provided a basis for truly nomadic lifestyles (Rabey 1989). Camelids could never be used as traction for preparing fields, processing crops, or transporting humans or for milk and derived products. Pastoralist societies here always remained intimately bound to their agricultural neighbors (see, e.g., Lane 2006).

5. Last but certainly not least, the cultivation and manipulation of cereals are so significant to the story of agriculture worldwide that they practically define the term. And yet in the Andes, agriculture began without the domestication of a single important grass or grain (Pearsall 1992:190). Until the introduction of maize, apparently from Meso-America (Matsuoka et al. 2002), the major carbohydrate sources were tubers and the so-called pseudocereals (Chenopodiaceae-Amaranthaceae).

Consequences for the Agriculture–Language Dispersal Equation in the Andes?

What consequences do these five idiosyncrasies entail for the agricultural threshold–population–language equation in the Andes? Together they determined an atypical relationship between incipient agriculture and the development of complex societies, as follows:

1. The very beginnings of domestication lie, almost by definition, beyond our ability to discern them. The first hints in South and Meso-America appear to go back to time depths of the same order of magnitude as in the Near East, at around the Pleistocene-Holocene transition (see, e.g., Dillehay et al. 2007; Piperno and Fritz 1994). In the New World cases, recent plant microfossil data suggest, as Sauer (1952) originally speculated, that some of these processes likely began in the tropical lowlands, that is, beyond the Central Andean area proper (Piperno and Pearsall 1998).

2. Across the tremendous ecological diversity of the Andes, complexes of plants (and animals) evolved under human manipulation in radically distinct ecological niches, isolated from each other at very different altitude levels. These inevitably took further time to undergo the transformations necessary for them to be successfully exploited in other niches. Moreover, the potential for such transfers varied from one cultivar to the next. On a larger scale, interactions with Meso-America were also severely limited by biogeography.

3. Of course, just because complexes of plant taxa were manipulated by humans in separate ecological niches does not preclude very early interchange of products and people between them. Transhumance between ecologically complementary niches is duly documented in the Andes long before agriculture and may well have played a decisive role in its eventual emergence. Likewise, various mechanisms of trade in obsidian and chert operated over deep time depths across the Andes and likely extended also to seeds, tubers, and other materials less visible in the archaeological record. Once the protracted transition to sedentism was complete, however, that record indicates only limited contacts between the now permanent communities (Dillehay, Bonavia, and Kaulicke 2004:29). In due course, ecological complementarity came to be articulated in various other ways, most famously through Murra's vertical archipelago model (invoked in part to explain, in conspicuous contrast to Meso-America, the absence of markets from the ethnohistorical and archaeological record for much of the Andes). The result was widespread "discontinuous territoriality": distinct ethnic groups occupied mosaics of territory encompassing diverse, ecologically complementary regions (Shimada 1985:xix). These changes took place in the Central Andes over the long period from ca. 11000 to ca. 4500 BP, known as the Preceramic (or in the alternative chronology, within the Archaic period).

4. The progressive domestication of plants proceeds hand in hand with that of the landscape itself, through the construction of terracing, irrigation works, raised and sunken fields, and so forth: "anthropogenic geomorphology," as Doolittle (2001:6) so aptly calls it. In the Andean context, this landscape modification was truly monumental: as Cieza de León first put it in 1553 (cited in Denevan 2001:135): "In the building of irrigation canals, I doubt that there has ever been a people or nation in the world who constructed and conducted them over such rough or difficult terrain." Again, the acute impediments of Andean topography ensured that landscape modification, too, was an extended process here, wrought and reworked only over great time depth (see, e.g., Denevan 2001).

5. Regardless of where domestication began, the archaeological evidence is clear that the first large-scale sedentary populations with monumental architecture, commonly taken as indicative of so-called complex society, arose in a number of valleys along the central Peruvian coast during the later phases of the long Preceramic period, based on exploiting the rich marine ecosystem. Perhaps the earliest of these developments began in four neighboring valleys on the central coast, collectively known as the Norte Chico. Over the course of the third millennium BC, an extraordinary agglomeration

of monumental sites arose here, typified by the famous site of Caral.

Extremely arid conditions prevail along this coastline. Its organic archaeological record therefore enjoys preservation conditions almost unique worldwide, giving us an especially rich insight into the subsistence bases of these original societies. For despite this arid climate, life and human settlement flourished in the oases along the many rivers that rise in the Andean highlands and flow westward to the Pacific across the desert coastal strip. By the end of the Preceramic period, floodplain (and, in due course, irrigation) agriculture of crops such as beans, squash, and various fruit trees had come to form an increasingly significant part of human subsistence (see, e.g., Dillehay, Bonavia, and Kaulicke 2004:24). Settlements shifted slightly inland to the alluvial floodplains, and certain coastal valleys seem to have supported considerably higher population densities than neighboring territories.

Be that as it may, theirs were not mobile subsistence packages that could expand into their highland hinterlands: agriculture was still a complement rather than an alternative to marine resources, so this subsistence regime necessarily remained anchored to the Pacific coastal strip. Moreover, not only did marine resources continue to be important but also, broadly speaking, they were available equally to all societies along the coast, while their individual river valley systems remained more or less isolated from one another by the expanses of desert that lay between them. In short, we argue that the idiosyncrasies of the Andean origins of agriculture have implications not for its deep-time origins but for both the rate and the discontinuous pattern of its subsequent spread.

All of these various factors combined in the Andes to dilute the relative impact that early agriculture would have on population density. In effect, they raised the intensification threshold that agricultural productivity had to reach before it could confer a truly significant relative advantage in subsistence resources on any one society and one that was expansive so that it could expand into its neighbors' territories and begin appreciably to outpopulate them—hence the delay before this higher level was reached in the Andes, in comparison to Meso-America or the Old World.

Finally, perhaps the most important factor in crossing intensification thresholds is that of successful cereal agriculture. The origins of maize, the paramount New World cereal crop, and the history of its apparent spread into South America are, as Iltis (2006:23) puts it, "still a somewhat contentious story . . . that has occupied botanists, geneticists, anthropologists, and archaeologists for more than 100 years" (see, e.g., Benz and Staller 2006). Yet while these are crucial debates in themselves, the details impinge only tangentially on the point being made here. For whatever the time depth of its origin and the route(s) by which it reached South America, it is not until the Early Horizon, around 2500 BP, that maize became a ubiquitous and abundant feature in the uniquely well-preserved archaeobotanical record from the desert coast of Peru (Pearsall 1992, 2008). As Burger (1992:209) puts it, "Perhaps the most conspicuous change in the [coastal refuse deposits from the Early Horizon] is the increase in the quantity of maize compared to other cultigens."

For the preceding Initial period from ca. 3500 to 3000 BP, maize remains are "still uncommon" (Pearsall 1992:191), while the claims for maize further back in time have long been regarded as controversial. Pearsall (2008:114, table 7.1) conservatively records no instances of Preceramic maize in her botanical database for the coast of Peru. Bonavia and Grobman (2000), meanwhile, argue that maize is present in small amounts in a number of Late Preceramic sites in the Andes, and some maize remains have of course been reported from Caral, too. But even if one sets aside any uncertainties about the veracity of the data on maize in the Preceramic, it remains clear that at all of these sites, maize is conspicuous precisely for its scarcity. As Shady (2006:401) observes: "Despite the extensive excavations being carried out in Caral, little maize has been found." Indeed, she concludes that maize played no role in daily diet, and her inventory of food plant remains found at Caral records but an infinitesimal proportion of maize, in stark contrast to the "huge quantities of fish and mollusks" (Shady 2008:49).

Inland, the story is less certain because of poor organic preservation. Nonetheless, the very limited data available do seem to suggest that here, too, it was not until the late Initial period that maize became "a significant and regular contributor to human diet . . . at some highland sites when complex societies such as Chavín de Huantar developed" (Tykot, Burger, and Van der Merwe 2006:196; see also Chávez and Thompson 2006; Chepstow-Lusty et al. 2003).

In sum, we have a story that, while still full of doubt in its detail, is plain in its broad meaning: maize may well appear in small amounts during the Preceramic, but it is not until the Early Horizon at the earliest that it became a staple food crop contributing significant starch intake to human diet as part of an agricultural package in the Central Andes. How come?

More than any other domesticated plant, maize has undergone quite extraordinary morphological change as a result of its long coevolution with humans—a process still ongoing. Notwithstanding its apparent origin in Meso-America, maize eventually developed more diversity in the Central Andes (Grobman et al. 1961). This diversity is testament to the time depth of maize evolution there, the intensity of human selection it underwent, the diversity of the ecological contexts to which it was adapted, and the range of economic and ritual roles that it acquired. For the botanist Bird (1980, 1984), moreover, most of this tremendous Andean maize diversity had been achieved by around 3000 BP: the dawn of the Early Horizon.

It is perhaps no coincidence, then, that this is also the period for which the archaeological record also represents the first clear evidence of some form of pan-Andean integration, originating in Central Peru: the Early Horizon. Indeed, for

Collier (1962), improved maize varieties were the subsistence platform on which the Chavín Early Horizon was founded. Throughout Andean prehistory, maize underwent a series of morphological changes whereby earlier varieties with pop and flint kernels developed into many-rowed flour-kernel maize (see, e.g., Bird 1980, 1984; Bird, Browman, and Durbin 1984; Grobman et al. 1961). Our archaeological record of maize is still incomplete, but at least on the coast, small forms of Central Andean many-rowed maize had become the dominant maize type by the first century BC, toward the end of the Early Horizon, with a distribution from "La Libertad south to at least Ica" (Bird, Browman, and Durbin 1984:203), which, we note, is strikingly coincident with the extent of the Early Horizon along the coast. While we do not agree with Bird, Browman, and Durbin's (1984) unorthodox vision of the linguistics, we do take up their observations on maize morphologies, to postulate that cumulative changes to many-rowed flour maize with higher starch contents may, by the Early Horizon, have tipped the agricultural package of which they were a part across an expansive intensification threshold.

And it is of course the Early Horizon that finally brings us into the date ranges consistent with the relatively shallow genealogical depth of both the Quechua and Aymara language families. For on the basis of their own independent language data, specialists in Andean linguistics envisage a time frame of precisely this order as the earliest plausible period in which one might set the first stages in the divergence of either of these language families. Indeed, as is apparent from figure 3, even this requires a "long-chronology" view of their expansion histories, that is, toward the earliest end of the date range generally entertained (Heggarty 2008). The linguistic geography of their successive expansions, too, is eminently compatible with the Early Horizon heartland. It is widely agreed that both families' expansions doubtless started out from homelands somewhere in Central Peru, even if the debate on more precise candidate locations within this region remains open (Cerrón-Palomino 2000:290; Heggarty 2008:52–53; Torero 2002:46).

An Agriculture-Defined Linguistic Scenario for the Andes?

So on closer inspection, the archaeological detail in the Andean context points to an interpretation very different from the first impression of a counterexample to the agriculture–language expansion equation. For rather than just a single hearth for the coming of agriculture, we now have a more detailed agricultural scenario in which multiple, independent domestication processes are played out over long periods of relative isolation from each other. This sits much more easily alongside our linguistic picture of multiple, unrelated language families. Far from a contrast to contradict any link between agriculture and language expansions, then, these two patterns exhibit so strong a parallel as to immediately suggest just such a link. More precisely, the fundamental principle that it is extralinguistic forces that determine patterns of language divergence suggests that the link is one of cause and effect.

Our refinements and Andean idiosyncrasies duly allow us to posit a scenario in which the striking linguistic pattern in the Andes might after all plausibly be attributed to agriculture in the specific trajectory it followed there. Recognition should be accorded here to the first ventures in this direction by Torero (2002:38–41, 123–124), who speculates that the absence of any deep, overarching family might be a linguistic echo of the tortuous topography of the Andes and the consequent isolation of the first hearths of agriculture from each other. We take this much further, however, by introducing other significant idiosyncrasies and complexities of the Andean context. Together, these can be built into a holistic explanation for the broadest patterns in the comparative/historical linguistics of the Central Andes, as they relate to human cultural developments there. Finally, we can recruit the case of the Andes as one that allows an instructive contrast to be drawn with other parts of a world, to contribute to the agriculture–language dispersal debate on a much wider level, as we shall shortly attempt in our conclusions.

The detailed linguistic scenario that might best fit the archaeological one, then, would run as follows: We start by observing that we have no good grounds at all to make that other tacit assumption behind an Amerind scenario, where the first settlers of the Americas (however homogeneous or diverse they were genetically) were all speakers of the same single language. So, back at these earliest stages of the peopling of the Andes, speakers of all lineages of the future Andean languages dispersed across the region as hunter-gatherers and thereafter lived in groups in relatively little contact with each other. This effective isolation continued over the millennia leading up to, through, and even after the protracted processes by which various of these groups independently came to develop agriculture.

So even if the linguistic ancestors of some of those groups had originally spoken the same language when they first arrived in the continent (including any putative "Pre-Proto-Quechumara"), over these many millennia of relative isolation, divergence between their speech in the different regions would have proceeded apace, obscuring by now all obvious traces of any ultimate common origin. This holds whatever the antiquity of human settlement in the Americas, given how quickly language change accumulates. Original correspondences in sound and meaning are incessantly whittled away until eventually so little is left as to be indistinguishable from the random background level of chance similarities statistically inevitable between any two languages. Most mainstream linguists (e.g., Kaufman and Golla 2000:47) consider 10 millennia or fewer (before first attestation) to be the effective ceiling on how far linguistic reconstruction can see into the mists of time to discern language relatedness. A first settlement even as late as Clovis takes us beyond this level, a fortiori, for any earlier settlement. To imagine that at this level

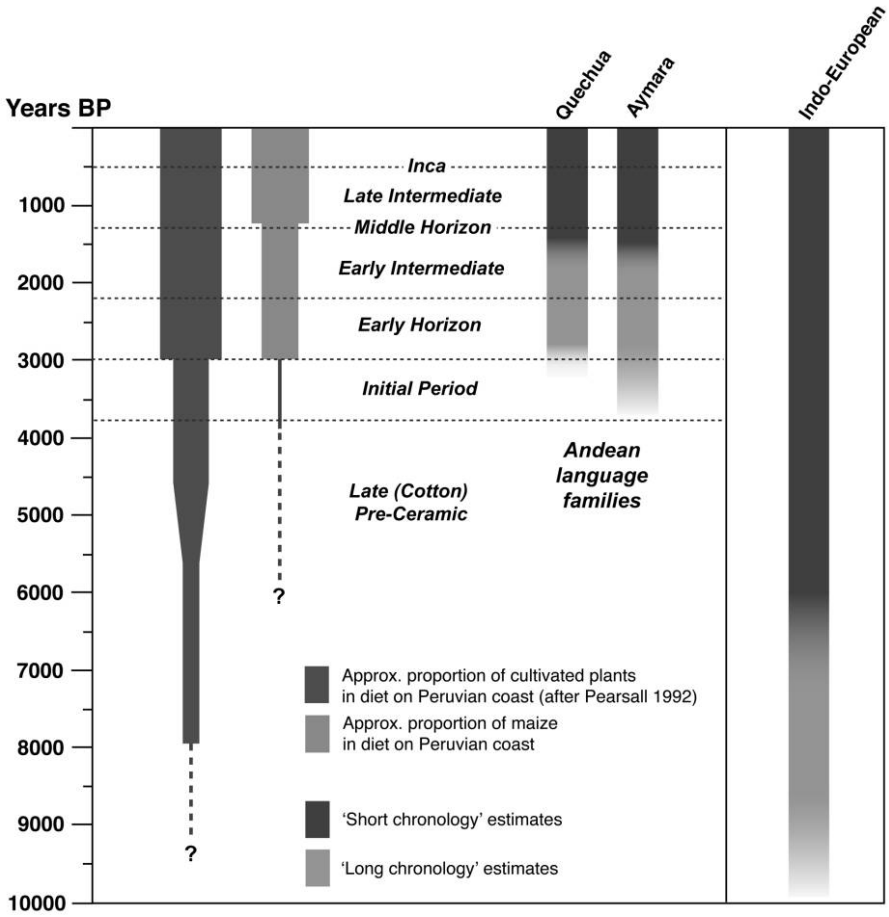

Figure 3. Language dispersals and phases of agricultural development in the Andes: time depths compared.

we see a mass comparison Amerind pattern in the language data, then, is indeed to look for faces in the fire.

This is not to deny that there were movements of or contacts between populations over the *longue durée* of the Preceramic. As observed above, the archaeological record indicates that there were. So, too, does linguistics: very many structural correspondences exist between the language families of the Andes, not least Quechua and Aymara. Critically though, these are not of the sort that indicate that languages have diverged from a common origin but that they have been converging, through contact, from originally different, unrelated linguistic starting points.

Countless languages across the Andes and Amazonia originally unrelated to each other have gradually come to share in certain general, abstract, structural characteristics, though without any one language recognizable as the source (see, e.g., Torero's [2002:539] typological criteria). Such areal features are of precisely the type that typically denote chains of localized interactions—over prolonged timescales and across extensive territories—between small-scale groups speaking a mosaic of different languages and not expansions of single languages into broad families, which would leave quite the opposite linguistic signal. That is to say, they reliably denote only areal proximity, not relatedness (Adelaar with Muysken 2004:34–36; Cerrón-Palomino 1995; Heggarty 2006:185–188; Torero 2002:154).

Only at some stage during the Late Preceramic, between 5000 and 2000 BP, did societies in the Andes reach such a scale that interaction between larger groups became significant. This, too, may have a correlate in a linguistic pattern, in offering one possible explanation for why the contacts between Quechua and Aymara in particular are of a stronger order than simply sharing in general areal characteristics and extend far beyond this to a host of identifiable borrowings and specific structural calques throughout both families. Andean linguists have traditionally argued that these likely date back to an intense interference between the Quechua and Aymara lineages while each was still at the stage of its respective ancestor language, that is, before either began to diverge into a family (Cerrón-Palomino 2000:337). Even with this type of linguistic effect, however, we remain in the realm of interactions and contact that bring about a degree of con-

vergence between originally unrelated languages. We still have no linguistic evidence of the contrasting process of language expansion and subsequent divergence: that is to say, no evidence of speakers of any one language enjoying so significant an advantage as to propel their speech to a wide-scale expansion at the expense of that of their neighbors.

Both the archaeological and linguistic records for the Andes show plenty of evidence, then, for widespread movements, contacts, trade, exchange, and so forth over the deep time depths of the Preceramic period. Crucially though, none of these interactions are of the sort that might drive a major language expansion on the scales we seek for Quechua and Aymara. The shallow time depths of language families in the Andes indicate that it was not until around 2–3 millennia BP that some population—whether Aymara- or Quechua-speaking (see Beresford-Jones and Heggarty 2010)—at last came on some sociopolitical and/or demographic force(s) powerful enough to carry their language(s) across great expanses of the Central Andes (at the expense of a disparate constellation of minor languages that had fragmented over the many millennia since humans had first populated the region). At this relatively recent time depth, one obvious candidate for a role in driving language expansion(s) is the crossing of an expansionist agricultural intensity threshold that came unusually late in the Andes. This coincidence in timing can be seen by setting the divergence time depths of these language families alongside the archaeological and particularly agricultural chronology, as shown in figure 3.

How Might Linguistics Inform Archaeology?

At this point, we turn to how the linguistic story might inform archaeological interpretation. For although an underlying coherence in the archaeological record for this period across much of Peru has been noted ever since Julio C. Tello in the 1920s, hence the term "Early Horizon," there remains great uncertainty as to its nature: the Chavín problem (as Willey [1951] called it; see also Kembel and Rick 2004:51). Some would deny the very existence of an Early Horizon (e.g., Pozorski and Pozorski 1987) or even the utility of the horizon concept itself (Rice 1993).

Meanwhile, there has been much ado about the far earlier agglomeration of monumental Preceramic sites in the Norte Chico during the third millennium BC. Some see these as the first evidence of urban or even state-level society: an ancient font of "political centralization and the beginnings of a fundamental Andean religion" with "a 'provocative' effect on other outside areas" (Haas and Creamer 2004:46, 36; see also Shady 2003, 2008). Others remain cautious.

Given how speculative all interpretations of these deep-time periods are, it is all the more valuable that linguistics can bring to the debate a fresh line of evidence. We reiterate our founding principle: great language dispersals like those of Quechua and Aymara must have been driven by processes of commensurate scale. In a lucid defense of the Early Horizon, Burger (1993:74) concludes that "the Chavin horizon is not a stylistic chimera as some have contended, but a real pattern"—a pattern, we would add, that in both time depth and scale is echoed in the earliest language dispersals in the Central Andes. By contrast, the Norte Chico Preceramic complex, for all its undoubted significance, shows no such correspondence, either in chronology or in scale.

First, viable estimates for the time depths of the language families of the Andes do not even approach the antiquity of the Late Preceramic. It is true that suggestions have been ventured (Haas and Creamer 2006; Shady 2003), that the language of Caral may have been of the lineage that ultimately became Quechua. But scholars from disciplines outside linguistics should beware certain fine but strict distinctions in linguistic terminology here. For at the time depth of Caral, linguists make a point of referring not to Proto- but only to Pre-Proto-Quechua (Cerrón-Palomino 2003:22) or, the alternative, Paleo-Quechua (Torero 2002:45). These terms, by definition, denote stages of the lineage long before expansion and divergence: "a remote antecedent of the modern Quechua language family," as Torero (2002:44) puts it. In any case, identifying the Quechua lineage with Caral is very far from orthodoxy among Andean linguists and is rather disingenuous, given that there is simply no linguistic evidence that permits us to do so. As Cerrón-Palomino (2003:22) rightly observes, such claims are "premature, if not to say speculative" (for further information, see suppl. A, "The Language of Caral?"). There is no suggestion in linguistics, then, that Caral—even if it did speak this language lineage—was responsible for its expansion; rather, the very terminology used firmly asserts the contrary.

As we have made clear from the start, there is no language family in the Andes and certainly not Quechua whose dispersal can be set remotely so far back in time as the Preceramic. Whatever else the Norte Chico might represent of potentially great significance, there is no trace whatever in the linguistics that it drove any language expansion. The same applies, a fortiori, for any other postulated deeper-time antecedents of Central Andean civilization in the tropical lowlands, be they in Ecuador or Peru.

Nor do the precocious developments in the Norte Chico satisfy our criterion of scale. In due course, it may be demonstrated that Caral was an urban center and capital of a pristine state in the Norte Chico (though, e.g., note the circumspection of Dillehay, Bonavia, and Kaulicke 2004). Chronological refinements may come to support its provocative effects along the central coast and into its immediate hinterland. No matter—for these would hardly make for a horizon. Preceramic societies of certain central coast valleys may well have supported considerably higher population densities than their neighbors, but for as long as they relied on a combination of marine and agricultural resources, they remained anchored to the coast. In fact, the entire prehistory of the Central Andes seems to present no instances of coastal societies expanding to dominate their highland hinterlands over

any significant territory. As Julio C. Tello long since observed, major expansions clearly visible in the archaeological record all proceeded the other way around, spreading out of the highlands.

And so to our final observation to reiterate here: that whatever the intrigue over its first appearance, maize is conspicuously scarce before the Early Horizon. In the 1960s, largely by analogy with its status as the preeminent crop during the Inca Late Horizon some 2 millennia later, scholars frequently speculated that maize might have provided a critical stimulus for the Chavín Early Horizon (Collier 1962:171; and for a critical review, see Burger and van der Merwe 1990). Today, however, most archaeologists see the Early Horizon rather as the expression of what was little more than a proselytizing cult, radiating out from (or in toward) the monumental site of Chavín de Huántar (Burger 1992; Kembel and Rick 2004).

Here again, the synthesis between archaeological and linguistic data proves instructive: for whatever our interpretation of the Early Horizon, we propose that it should include one or more forces capable of driving a major language expansion. This leads us in turn to question the long fashionable archaeological model for the Chavín Early Horizon, which "places the vital motor of cultural change in ideology rather than in the material realm," as Willey (1999:86) puts it. For we see few, if any, historical precedents for major language dispersals being accounted for by religious proselytizing. The oft-cited counterexample is, in fact, a chimera: the language of the Arabs spread not wherever Islam did but only to those areas where it was driven by their military conquests (for more information, see suppl. A, "Language and Religious Dispersals?").

Religious ideology doubtless was an important trapping of the Chavín Early Horizon and is certainly conspicuous in the archaeological record, yet for all that, it signally fails as a "vital motor" to drive major language expansion. Prototypically, this requires, as Bellwood (2005) proposes in detail, demographic growth and the movement of substantial populations of speakers. Perhaps archaeology has been misled by the all-too-visible representation of the Early Horizon's cult ideology in its material culture record—in an apparent upending of Hawkes's (1954) (in)famous "ladder of inference." Putting ideology aside, we would draw attention to the correspondences in broad scale and timing among the first language family expansions in the Andes, the "real pattern" (Burger 1993:74) that the archaeological record of the Early Horizon represents, and the first point in time for which we can unequivocally assert that maize became ubiquitous in the archaeobotanical record. Our synthesis of linguistic and archaeological data here suggests that Collier's (1962) original formulation deserves revisiting (for additional information, see suppl. A, "Maize and Tubers in the Andes"). Interestingly, as early as Isbell (1974), it was suggested that the expansion of Quechua was associated with that of maize agriculture, even if his specific proposal won no real support (Cerrón-Palomino 2003:336–337).

We do not, of course, advocate a simplistic argument that the spread of the major language families of the Andes is to be sought uniquely in agriculture. Certainly, we wish to suggest that it was the incorporation of maize as a cereal crop at significant levels that added the last, vital piece in the coalescence of the full Andean agricultural package. But by this late stage in the development of human societies in the Andes, other factors were no doubt also crucial to the crossing of an intensity threshold, not least the adoptions of agricultural technologies such as irrigation and terracing, allowing control of the growing season. As Benz and Staller (2006:670) note, "Maize does not appear to have displaced native cultigens in the early periods because C_4 cultigens were not specifically adapted to these environments without an intensive labor investment in the development of hydraulic technology." Moreover, this domestication of the Andean landscape was itself a process over great time depth. Floodplain agriculture on the coast, for instance, gradually came to employ a range of techniques across the "continuum of hydrostatic manipulations" (Nabhan 1979:246) from floodwater farming to, at length, canal irrigation.

Tracking all of these developments in time is notoriously difficult, but we hypothesize that it was only at some point around the start of the Early Horizon that these various, gradual processes intersected to tip Andean agriculture across an expansive intensification threshold (see Pearsall 2008). In any case, the Early Horizon is far from simply an expansion of maize cultivation: by this time, more proximate forces, too, were certainly at play, perhaps to do with a degree of social organization of the labor source that made possible the large-scale manipulation of the landscape in the first place.

All this makes for an Andean exception to patterns typical in the Old World on yet another level that needs to be borne in mind if we are to attain a more refined understanding of the relationship between agriculture and language expansions. The contrast with the other hearths of agriculture across the globe, such as the Fertile Crescent, is that domestication processes there, however local and independent they may have been initially, eventually did coalesce into what became, in effect, a single, mobile agricultural package, based on cereals and including significant animal protein. Even if this coalescence process took millennia, it was nonetheless complete by what was still a relatively very early date, by the standards of the Andes. The issue is not when the first settled agricultural societies arose, then, but when a predominantly agricultural package finally coalesced, with a cereal as a core component, into a form that could spread across great distances—which, in the Andes, also meant far inland, free from dependence on the maritime resources of the coast.

To be sure, the time depths of the origins of agriculture are roughly equivalent in the Andes and the Levant. But the sundry idiosyncrasies that we have identified in how agriculture subsequently developed in the Central Andes together delayed by many millennia the effective coalescence of a mobile agricultural package that could breach a linguistically sig-

nificant intensity threshold. Therein lies the determiner of the contrasting linguistic fates.

Crucially, in the Fertile Crescent this coalescence occurred long before so-called state-level polities developed to a scale at which they could have their own linguistic impact. Proximate fortune would eventually favor just one of the patchwork of languages originally spoken in Iron Age Italy, for instance, and it happened to be that of Latium. Yet for all its might, Rome could make little difference to the broadest-scale linguistic picture, for most of the potential rival languages it eclipsed were also Indo-European, in any case. In other words, by the stage that such proximate factors came into play, in Europe the ultimate ones had already laid down the Indo-European base color long ago.

Not so in the Andes, which stand out as an exception on both sides of the equation: in the startling polychromy of the linguistic picture and in how long it took for the initial independent domestication processes to come together into an agricultural package sufficient to propel some population across an expansive intensity threshold—which tropical latitudes, topography, and ecology conspired to set unusually high here. Only around the beginning of the Early Horizon, we argue, was this threshold finally crossed, thanks above all to the full incorporation of a cereal crop at last. We suggest also that this has been unduly overlooked hitherto precisely because it emerges so late in the trajectory of societal development in the Andes that its impact is therefore hard to tell apart from that of more proximate political and technological factors. Certainly, the visible archaeological record of the Early Horizon is characterized as much by cultural influences as by any expansion of a maize-based agricultural package.

In Andean linguistics, uncertainties still surround the various proposed scenarios for the precise homelands and expansion sequences of the Aymara and Quechua families; our own detailed proposal is set out in future work (Beresford-Jones and Heggarty 2010, forthcoming).

Nonetheless, there is at least reasonable consensus on locations for both homelands somewhere in central Peru and on the order of magnitude of the timescales involved (Heggarty 2007:333–337; 2008:41–43). The overall linguistic scenario we have presented above is compatible with this consensus on both counts. For our purposes here, what matters is only that the shallow time depth of the main Andean language families points to the Early Horizon as the earliest plausible candidate for having driven either of them. For on the broader scale we are considering here, the key question to answer is not where or even when precisely Quechua and Aymara began to expand but why no major expansions occurred much earlier to have left any significant linguistic trace—as happened in all other hearths of agriculture around the world and despite the advanced stages of societal complexity attained in the Preceramic, to which its monumental constructions stand in testimony.

The Andes in Global Perspective: The Exception . . . That Proves the Rule?

To be sure, the Andes stand out as a clear exception to the patterns observed in the world's other main hearths of agriculture. Yet it is an exception of the type that, if anything, more proves than undermines the "rule" that agriculture is a critical factor in linguistic expansions of the oldest and broadest scale. For the Andes constitute an exception not just on one side of that equation but on both. Specifically, where agriculture develops and spreads in a particular pattern that does not follow the usual Old World path of relatively soon constituting an effectively single center of domestication, the linguistics duly mirrors this with its own corresponding exception to the usual pattern of one or more broad, deep-time language families. It would seem a weak conclusion to attribute this to mere coincidence when there is an existing hypothesis that would account for it.

The Andes, the most overlooked of the world's independent centers of pristine civilization development, undoubtedly presents a number of complexities and idiosyncrasies. These can be only salutary for our purposes here, however, in that they require us to progress to a much more refined and qualified view of the basic agriculture–language dispersal hypothesis and to interpret it more sensitively in the different contexts in which human societies developed. These are lessons whose value extends far beyond the Andes.

The Andean case first underlines how essential the refinements that we propose are to the agriculture–language dispersal hypothesis. Once taken on board, however, they duly transform the interpretation of what, at first sight, can appear to be a contradiction of that hypothesis. For it transpires that our case study in the Andes in fact offers something of a reverse instance of it. The agricultural and linguistic prehistories actually turn out to be entirely consistent with the generalization we propose: that among the many possible forces that can drive language dispersals, the coming of agriculture takes on a uniquely powerful status specifically at the scale and time depth of the world's broadest and oldest language family expansions. For the case of the Andes exemplifies how the correlation survives in negative: in the absence of an effective single, ancient center of domestication, we duly observe a striking absence of the deep, broad language families that seem to attend the other hearths of agriculture across the globe.

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Comments

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Even though the hypothesis that agriculture was the ultimate driving force of language dispersal in many parts of the world is not universally accepted, Heggarty and Beresford-Jones show in a convincing way that an agriculture-based explanation may underlie the relatively recent dispersal of the major Andean language families, Quechua and Aymara. Not the invention of agriculture itself, which in the Andes is nearly as old as the beginnings of agriculture in the Old World and in Mesoamerica, but rather the introduction of a highly productive and efficient staple crop, viz., maize, marked the threshold for the sort of expansion that favors the spread of successful languages at the cost of previously existing linguistic diversity. In the Andes, this process would have started during the Early Horizon, most probably during its second phase, when the influence of Chavín, as a major cultural-religious and possibly also political center, could be felt throughout central and central-northern Peru. The assumption that bearers of new agricultural techniques consolidated their achievements by occupying newly conquered territories presupposes a large-scale assimilation of preexisting populations located in those territories. These local populations may have had a nonexpansive agricultural history and probably represented an average density of distinct linguistic lineages, which would eventually be driven to extinction by the pressure of the expanding languages. This scenario is compatible with the view traditionally held by Peruvian linguists and archaeologists that linguistic and cultural diversity developed over millennia in long-lasting Neolithic societies with limited contact.

In spite of the strong explanatory power of this model, some significant exceptions need to be mentioned. In some areas, such as northern Peru and the Ecuadorian highlands, much of the original linguistic diversity was preserved until relatively recent times. Small languages, such as Quingnam, Mochica, Culli, Sechura, Tallan, Cañar, Puruhá, Panzaleo, and Cara, were eventually replaced by Spanish or Quechua but not before colonial or postcolonial times. Their late disappearance cannot be linked to any agricultural revolution. By contrast, traces of supplanted languages are remarkably absent in the central and southern highlands of Peru, as well as in the Bolivian highlands. The only non-Quechua, non-Aymara languages that can be detected there are Puquina, possibly an intrusive language with origins in the Amazonian region, and Uru-Chipaya, spoken by an anthropologically and historically rather distinct population. How can we explain the enormous success of maize-driven language dispersal in the central and southern parts of the Andean region as opposed to its failure in the north?

The absence of language families older than 3,000 years, observed by the authors, is also open to question. It may very well be that there are no such families in the Central Andes, but they are found in the northern periphery of that area. The language families Barbacoan and Chibchan, the former located in the Ecuadorian-Colombian border region and the latter extending through Central America from Colombia to Honduras, are almost certainly older than 3,000 years (and considerably older in the case of Chibchan). Would they be the product of earlier maize-driven expansions, predating the moment that maize reached the Central Andes? Do they represent cases of previous language dispersals?

A remaining question, but not the least in importance, is the complex relationship of the two expansive Andean language families themselves. It is correctly observed that these two languages are not genetically related at any detectable level and that they must have had separate homelands. By consequence, the intense structural and phonological similarity that exists between the two language families can be explained only by a period of long and pervasive interaction, maybe of multilingualism. Such a situation of interaction was probably initiated either by a violent invasion or by a slow process of ethnic expansion. The invaders may have had their language remodeled by assimilation with groups speaking the language of the invaded. After this period of interaction, the two language groups (one of them thoroughly transformed) apparently became separated again, minimally to the extent that they were then able to generate separate expansions. The historical background and interpretation of the Aymara language dispersal in relation to the dispersal of Quechua is one of the major problems that remain to be solved for a proper understanding of the Andean past.

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While I agree generally with the conclusions of Heggarty and Beresford-Jones's article and appreciate the support given to the agriculture–language dispersal hypothesis, I find that Bellwood (2005) and Diamond and Bellwood (2003) are discussed from a rather negative perspective. This begins in the abstract, where reference is made to "the hypothesis that it was the adoption of agriculture that lay behind the dispersals of the

world's greatest language families." But we have not applied this hypothesis to Athabaskan, Pama-Nyungan, Khoisan, Turkic, Mongolian, Tai, Hmong-Mien, or Uralic. Furthermore, it was not the "adoption of agriculture" per se that was the main driver of expansion, as I made clear in my *First Farmers* (Bellwood 2005:277–278), but the increase in agricultural dependence to the point where it could underpin population growth and dispersal. Farmers did not head immediately for the horizon as soon as someone planted a seed.

The authors refer to a statement by Golla that Neolithic populations settled the Pacific because of seafaring technology, not agriculture. Of course, Pacific peoples migrated in boats, but hunter-gatherers did not settle the remote and small islands of Oceania. Polynesians carried their portmanteau biota of crops and domestic animals during their migrations, and equating these migrations with maritime technology alone is insufficient. Without food production they would not have survived on many of the smaller islands, certainly not as the large and vibrant populations described by eighteenth-century explorers. Even if the first settlers had brief access to large populations of naive birds and marine mammals before their local extirpation, they did not give up their practices of food production.

The authors also deny the significance of food production in recent population diasporas. Indeed, one could suggest that British settlement in Australia occurred simply because the British had large ships and too many convicts. However, I would claim that agriculture in a broad sense (including commercial as well as subsistence crops) was extremely important in the recent European colonizations, including those of the Russians—to put these down to "more proximate factors and historical contingencies" is not entirely helpful. True, the Cossack settlers were not Neolithic, and neither were the first white Australian freed convicts and migrant sheep farmers (Blainey 1966). But all these peoples, like the Anglo-Saxon migrants who carried the roots of the English language to Britain, wanted agricultural or pastoral land as an ultimate outcome and did not migrate for fun. New Spain was not conquered just for God or its gold; land and agricultural labor from conquered members of the native population were also significant magnets (Diaz 1963). The Romans used certain provinces for land grants to their retired (and Latin-speaking) soldiers and officials; this is exactly why Romance languages exist today close to the heart of the former Empire (including Romania) but not in Britain, Germany, Egypt, or Morocco. While agriculture did not cause these migrations in any monolithic way, I see it as counterproductive to give it a completely diminished role. Land had immense value in these societies.

Moving now to the Andes, I was not able in my *First Farmers* to go into great detail about Andean linguistics, partly due to lack of sources available to me. I am quite aware that Quechua and Aymara are internally not very diverse, and this is why I did not equate them with the late Preceramic in *First Farmers*.

The unfolding perspective on agriculture offered by the authors is basically the same as I proposed in *First Farmers* and in many papers since (e.g., Bellwood and Oxenham 2008). It was not just the very beginnings of plant cultivation and animal management that started linguistic and cultural expansions but their later intensifications. As in eastern North America, part of this intensification certainly developed in the Andes with the arrival of maize, during the Initial period and Early Horizon. Thus, the combination of maize, Chavin, and Quechua and Aymara expansion is perfectly convincing to me, as to Heggarty and Beresford-Jones.

The authors also suggest that the Andes took much longer than the Middle East to coalesce into one package capable of encouraging population dispersal. But the Natufian to Middle Prepottery Neolithic B trajectory in the Levant occupied roughly 2,000 years. A similar time lag occurred in central China from initial cultivation to large-scale population dispersal, at least southward (Zhang and Hung 2008). Development from the Caral Preceramic to Chavin occupied 2,000 years. These time spans all look rather similar to me. I have suggested similar scenarios for the Eastern Woodlands, with their Siouan and Iroquoian language families (Bellwood 2005: 248–249), again situations where local noncereal crops preceded maize.

The authors conclude with the statement that "to be sure, the Andes stand out as a clear exception to the patterns observed in the world's other main hearths of agriculture." As will by now be clear, I do not entirely agree. Even so, the historical conclusions presented in this paper do not surprise me, and I am gratified that the basic agriculture and language hypothesis still stands, at least in the eyes of these authors.

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Heggarty and Beresford-Jones have produced a clear-headed and incisive review of a subject made complex by its interdisciplinary nature, methodological nuances, and the excessive shortage of hard evidence from archaeology and historical linguistics. Their evaluation of an Andean "agriculture–population–language dispersal equation" rightly stresses the need to understand the historical relationships between language change and spread, population movement, and material culture within a larger cultural package. However, the place of languages within this package and whether Andean languages moved with populations (and agriculture) are not known.

The authors posit that the dispersal of Quechua is related to a maize-based "expansionist agricultural intensity threshold" that was reached during the Early Horizon. They also argue that "great language dispersals . . . must have been driven by processes of commensurate scale," implying that

pan-Andean cultural changes, including an increase in maize production, during the Early Horizon provided such a scale. However, a closer examination of the coincidence of Quechua and of the archaeological features of this horizon reveals no concrete evidence that they are linked. Further, the archaeological sampling for a widespread increase in maize cultivation during this period is presumed on the basis of slight increases at a few coastal sites. Increased production in the highlands, where Quechua most likely originated and spread, has not been sufficiently demonstrated.

Further, it is puzzling why mobile herding societies have been neglected in discussion of the spread of Andean languages. Is it because there is a pervasive view that pastoral societies had poor material cultures in comparison to agricultural societies and thus were marginal to the mainstream of cultural developments? The authors state that "pastoral societies . . . always remained intimately bound to their agricultural neighbors." This statement may be the case for parts of the northern and central Peruvian Andes, but is it not true for the puna and altiplano areas of southern Peru and the south-central Andes, respectively. While the economic foundations of early Andean states are conventionally sought in the development of agrarian production, the archaeology of pastoral societies has mainly concentrated on highland hinterlands. This generalization, however cruel, highlights the importance of relating herders to the spread of languages, particularly Aymara and Uro. Possibly fueling the expansion of these languages also may have been the synergy created by combining herding with cultivated crops. A critical development may have been a shift in the balance of risk management as pastoralists spread into previously unoccupied or less densely occupied areas of the Andes.

Another theme to reflect on is the long-term persistence of hunting-gathering alongside and beyond areas inhabited by farmers and pastoralists and what role it might have played in the spread of languages. On the basis of the archaeological record, it can be shown that hunters and gatherers continued to occupy portions of the Andes as late as 2000–1000 BP (e.g., Dillehay, forthcoming; Lavallee 2000). Expanding farmers and herders did not necessarily neglect or avoid hunting-gathering. The processes by which hunting-gathering gave way to agricultural and/or agropastoral food production remains much underresearched, leaving us to speculate what combined role they may have served in the spread of languages.

The likely unevenness with which food production and languages spread and the opportunities for hunter-gatherers, herders, agriculturalists, and agropastoralists to strike a variety of exchange-based connections with each other across multiple ecological zones, including the transportation of crops from one zone to another, may help to explain the spread of languages. I see the possibility for language replacement over a long period of time that would not necessarily involve major population movements and commensurate scalar processes but might be seen as gradual systemic demographic and social processes that were advantageous to societies practicing a variety of economies across different ecological zones. To assume too light a set of linkages among economic diversity, the specifics of Andean resource ecology, and social processes may be wrong.

Set in this context, it also seems that the spread of Quechua and Aymara depended on the nature of social learning and cultural transmission. This in turn relates to an understanding of the evolved cognitive basis of social learning, the definition and recognition of imitated food production systems, and the role that large-scale public events such as ceremonies at monumental sites during the Early Horizon and later periods possibly played in this process.

To conclude, although increased reliance on maize (and other crops) may be identifiable in a few early archaeological contexts, it does not follow that the demographic, social, and linguistic consequences that this entailed were always identical across time and space in the Andes. How resources and, more specifically, maize were combined together within the total economic pattern, previous community histories, and social organization and ideology—all of which can be considered as parts of a broader cultural package—all likely had a part in the spread of Andean languages.

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The "farming/language dispersal" hypothesis, primarily associated with Colin Renfrew and his colleagues at the McDonald Institute in Cambridge and Peter Bellwood in Canberra, maintains that the diffusion of farming was the cause of all significant language spreads of the past 10,000 years. After an initial flurry of interest, including prominent mention in Jared Diamond's 1997 best seller, *Guns, Germs, and Steel* (see also Diamond and Bellwood 2003), it has not fared well at the hands of its critics, particularly linguists (see my own contribution to the fray in Golla, Malhi, and Bettinger 2003). Heggarty and Beresford-Jones hope to rescue what they believe is the central insight of the beleaguered hypothesis by conceding that most linguistic expansions in recent millennia were likely due to more "proximate" technological and social factors. The origin and spread of farming from its various hearths, however, would still be the ultimate "remote factor" in language dispersal, maintaining its "uniquely powerful" explanatory status "specifically at the scale and time depth of the world's broadest and oldest language family expansions."

Ironically, this comes just as historical linguistics has scored a major triumph by demonstrating beyond a reasonable doubt the existence of Dene-Yeniseian, a language family with approximately the time depth of Indo-European that stretches from south-central Siberia to west Texas (Vajda, forthcoming). Anchored on the west by Ket and several other now-extinct languages scattered along the upper Yenisei River (Vajda 2006)

and on the east by the widespread Athabaskan family of western North America, Dene-Yeniseian (which also includes Eyak and Tlingit) is the first clearly proven linguistic relationship ever to span Bering Strait. It also presents Heggarty and Beresford-Jones with a serious problem, one that promises to grow more troublesome as linguists turn their attention to the long-suggested relationship of Yeniseian to Tibeto-Burman. Whatever is afoot here, it is not the diffusion of farming.

While Heggarty and Beresford-Jones concede that farming traditions have sometimes spread without an associated language expansion, as in the Andean situation they focus on in this paper, they emphasize that these were late developments, complicated by "proximate factors," and that the older and more diversified a language family is, the more likely it is to be linked to an early farming tradition. Strictly speaking, this may be true, and the correlation would seem to hold even in the New World (Blench 2008). But it is merely an artifact of the methods that are employed to determine the relatedness of languages. It is a frustrating but ineluctable fact, known to every competent historical linguist, that regular correspondences in phonology and morphology of the sort that allow us to affirm the existence of a language family grow too attenuated after 6,000–8,000 years to exclude alternative explanations of the similarities. Since most farming traditions diffused during the past 8,000 years, it is hardly surprising that many linguistic diffusions during this period were somehow connected with the movement of farming peoples. But the lack of comparative linguistic data sufficient to demonstrate the existence of older widespread language families, associated with the movements of prefarming populations, can hardly be taken as proof that such families did not exist.

There are, in fact, many good reasons to assume their existence. The best of these is that large, well-diversified language families apparently resulting from the rapid expansion of small populations of hunters and foragers are a salient component of the linguistic diversity of the nonfarming areas of northern and western North America. Modest, incremental innovations in tools, skill sets, and social practices apparently gave the speakers of Proto-Eskimo, Proto-Algonquian, and Proto-Athabaskan enough of a competitive edge to be able to displace or absorb the earlier occupants of vast swathes of the Arctic, the Subarctic, and the Intermontane West. Although considerably more recent in absolute date than nearly all of the agriculture-linked expansions elsewhere in the hemisphere (the Thule Eskimo expansion from Alaska to Greenland occurred after 1000 AD and the Athabaskan and Algonquian expansions within the past 2,000–2,500 years), it is difficult to see how they fit Heggarty and Beresford-Jones's model of "proximate" postagricultural population movements. Furthermore, like the wave on wave of Indo-European that crested across central and western Europe, there is comparative evidence of the existence of much older families occupying significant portions of the Eskimo, Algonquian, and Athabaskan territories—in the latter case, the self-same Dene-Yeniseian family alluded to above, which appears to have a time depth of at least 5,000 years.

I applaud Heggarty and Beresford-Jones for viewing the factors promoting language expansion and diversification as points on a historical continuum of growing socioeconomic complexity, but surely they err when they imagine the prefarming world to have been a static mosaic of highly localized speech varieties. It seems a much more likely scenario that the expansion of fully linguistic *Homo sapiens sapiens* was largely acted out in what Nichols (1992) has called "spread zones," areas of relatively scarce resources across which thin populations of hunters and foragers diffused rapidly and subsequently diversified into dialect-defined subunits.

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Heggarty and Beresford-Jones's paper provides an important overview of past attempts at wedding archaeology and linguistics and the link between the development of agriculture and the widespread dispersal of languages from a common stock. The strengths of the paper lie in the discussion of linguistics and archaeology in general and of the possible spread of Quechua and Aymara in the Andean region specifically. The authors' attempt to look at the relationship between the archaeological record and the spread of these two major linguistic groups is a welcome addition to the field.

Two issues need to be addressed for the paper to have lasting value. The first is the importance of maize before the Early Horizon in the Andes. Findings of maize in the archaeological record before the Early Horizon are indeed scarce, at least for macrobotanical remains—cobs, kernels, husks. There is significant maize at Caral (Shady 2006) and not necessarily in the "infinitesimal" quantities cited. In fact, the actual data on the amount and context of maize at Caral have not been published. To compare it with the "huge quantities of fish and mollusks" cited by Shady (2008) is misleading. Inland sites such as Caral in the Norte Chico do have large numbers of fish and mollusk remains, but these do not necessarily translate into large numbers of kilocalories. The primary fish found at these sites are anchovies and sardines, and while there may be millions of bones found, at a site occupied for almost 1,000 years, those bones do not translate into more than a condiment. Similarly with mollusks: Caral and other inland sites have lots of mollusk shells, but there have been no analyses showing the caloric contribution of these shells to the diet. Littoral sites, such as Aspero and Bermejo, have significant midden piles of mollusks, as might be expected at sites relying heavily on marine products, but such middens are absent at inland sites, such as Caral, Cabellete, and Upaca. These inland sites were full-time agricultural communities and relied on domesticated plant resources for their calories. How important maize was to this diet is

unknown at this time. In our own analysis (see Haas and Creamer 2006), we have found that *Zea mays* pollen is present in a large majority of samples analyzed. Maize starch grains are also being found in coprolites.

An issue related to the open question of maize before the Early Horizon is the importance of full-scale agriculture in the Late Archaic and Initial period. The authors want to see a correlation between agriculture and linguistic dispersal, but when that does not quite correlate with the archaeological record of pre–Early Horizon agriculture, they point to maize agriculture as the critical variable. They dismiss without cause the Late Archaic in the Norte Chico because it is supplemented by marine resources. They also fail to deal with the agriculturally based Late Archaic florescence at Kotosh and surrounding highland areas, which have no marine component. They do not acknowledge the intensive agriculture at Initial period centers such as Sechin Alto, Cardal, and numerous other sites along the coast. Yes, these sites have marine components to their diet, as did all subsequent coastal societies, but they were fully agricultural.

Our increasing knowledge of cultural elements of the Late Archaic and Initial period occupations on the coast and adjacent highlands also diminishes the seeming importance of the Early Horizon as a turning point in the unification of the Andes. Unlike the Middle and Late Horizons, the Early Horizon per se does not unify the entire Andean region but is found primarily in the northern parts of the coast and highlands. Furthermore, we now know that central elements of the "Horizon" appear much earlier than the 3500–3000-BP time frame. The Early Horizon art style and religious iconography, for example, all appear widespread in the Initial period on the coast long before the founding of the Early Horizon center of Chavín de Huántar. The architectural icons of Chavín de Huántar, with U-shaped monuments and sunken circular and rectangular plazas, appear in Norte Chico sites by 2600 BC, 1,500 years before they appear in the highlands. They are widespread on the coast throughout the Initial period and extend up into the southern highlands. If art, religion, and architecture do not make the Early Horizon a distinct horizon, what does? There is no evidence of Chavín hegemony outside the Marañon River basin. While there was warfare in the Early Horizon (see Brown-Vega 2009), there is no evidence of conquest or political consolidation. Given recent research, there is little in the archaeological record that confirms the Early Horizon as the temporal and cultural hallmark envisioned by Willey and others.

The authors make a strong case for linguistics augmenting and adding to the archaeological understanding of Andean prehistory, and this is a valuable lesson to keep in mind. At the same time, given the vagaries of linguistic chronology, it seems unreasonable to stick to a 3500–3000-BP timetable for an Andean linguistic dispersal when this does not fit well with the contemporary archaeological record.

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Heggarty and Beresford-Jones's important paper takes a significant step toward its objective of "a holistic, cross-disciplinary prehistory," in bringing together the work of a historical linguist and a prehistoric archaeologist, both well established and both specializing in a single significant region, the Andes. Their examination of the farming–language dispersal hypothesis is a sophisticated one, both anthropologically and linguistically.

In the first place, it undertakes a general review of the farming–language dispersal hypothesis, formulating a more nuanced view than hitherto and offering some important general observation. Second, it moves toward a new outline for the origins of the linguistic geography of the Andes, specifically for the Quechua and Aymara language families, that promises to link their expansions or dispersals with demographic and ecological factors. Both are valuable contributions. It will, of course, ultimately be judged by the specific formulations toward which it is leading (Beresford-Jones and Heggarty 2010).

Its careful reexamination of the foundations of the farming–language dispersal hypothesis rightly stresses that "the transition from hunting and gathering to the control and cultivation of food sources (ultimately, agriculture) is in no sense an event but a protracted coevolutionary process between humans and parts of their environment." This is an important point sometimes missed by the critics—and perhaps some of the proponents—of the hypothesis. They go on to make the crucial observation: "This process is a gradual one of trial and error, as different components come together in different ways to form and reform various agricultural packages. Nevertheless, at some point(s) along it, an intensification threshold is crossed, at which a particular package coalesces that confers real net advantages on sociocultural and demographic levels, not least in that far greater population densities can be supported."

On this perception of the role of intensification and their consideration of the ecological diversity of the Andean region, their analysis is founded. The outcome is a more subtle view than the simple geographic spread of farming across an anisotropic plain, as outlined in the model of Ammerman and Cavalli-Sforza (1973). To say this is not a critique of that original formulation, a first-order model that can work well enough in a simpler context. But in the more complex environment of the Andes, with ecological zones varying with altitude, a more sophisticated approach is required.

There are some valuable insights along the way in this treatment but perhaps also a few oversimplifications. With regard to their methodology, one must welcome the bold statement that "real-world driving forces mold language di-

vergence patterns," a point developed by anthropologists in recent years (Nettle 1999; Renfrew 1990). But the analysis of causation among the "forces of history" seems less secure, where a distinction is drawn between ultimate and proximate factors. The assertion that seafaring technology is a much more proximate factor than agriculture is not universally valid, for instance, on the Pacific rim, where the population of Flores in Indonesia in paleolithic times must have been based on seafaring, as a necessary condition. I am not convinced that the a priori separation of causal factors into proximal and ultimate is the best starting point for the discussion.

A second problem is the estimation of time depth in historical linguistics, a familiar area of contention (Dixon 1997; Renfrew, McMahon, and Trask 2000). In a short article of this kind, the matter cannot be treated in detail, but what is offered here as so often rests to a large extent on the invocation of "linguistic consensus," and one can only agree that "observers from outside linguistics might be forgiven for feeling a little bemused by certain apparent contradictions in linguists' approaches to dating." It is in this area that more detailed considerations are likely to arise when the fully fledged historical analysis is presented (Beresford-Jones and Heggarty 2010), for which this paper lays the foundations.

Such criticisms are perhaps inevitable in this complex field. They should not obscure, however, the truly interdisciplinary quality of this paper, which moves beyond some hasty early evaluations of the farming–language dispersal hypothesis and seeks a more careful application in this interesting area. It illuminates not only Andean studies but also the manner in which the disciplines of prehistory and historical linguistics can fruitfully interact.

Reply

We extend our thanks to all our distinguished commentators. We begin with their reflections on the agriculture-language hypothesis per se: from Bellwood, a strong advocate, and from Golla, a critic. Both seem to wish us to start out from an a priori stance on the hypothesis and to see in our article a mission to argue either for it or against it in principle, invoking the Andean case to that end. Yet that is not what motivates this paper. Rather, we start out from the basic findings of historical linguistics in the Andes, echoed here by Adelaar. (Adelaar adds mention of other language families of interest but that lie largely outside our focus on the Central Andes and that are less significant demographically.) These findings cry out for explanation, to be sought in the wider context of human prehistory here. The most coherent scenario does involve a link with agriculture but hardly as straightforward as the strongest formulation of the controversial agriculture–language dispersal hypothesis would have it. This leads us to a number of necessary qualifications and refinements and a new and principled generalization. This, then, is a significant result emerging from our aim to explain Andean linguistic prehistory, not from a dogmatic intention to argue either for or against a general hypothesis in principle.

We are at pains to avoid the all-or-nothing visions that have characterized the agriculture-language debate hitherto. In particular, our proposed generalization stresses how much hangs on the different scales and time depths of language expansions. This seems lost on Golla and Bellwood, who continue a sterile debate to minimize or overegg, respectively, the role attributed to agriculture. Indeed, they often appear to be arguing against each other rather than our paper. Is what we actually say rather too reasonable for either to object to sufficiently? Certainly, their commentaries paint positions in extreme terms that we ourselves explicitly do not espouse.

We are careful to characterize the hypothesis as invoked for "many of the greatest expansions of human languages." Yet Golla states that it claims farming as "the cause of all significant language spreads of the past 10,000 years." Bellwood, meanwhile, lists language families to which the hypothesis has not been applied—again, above our heads, for we never suggested otherwise. Just as Golla turns our "many" into his "all," Bellwood misses our key qualification "greatest": for when set alongside the likes of Indo-European and Afro-Asiatic (our examples), Bellwood's families are hardly of equal rank: demographic minnows and most of them far less extensive geographically.

In any given language expansion, a number of factors are typically involved, of which agriculture may be one. Debate then hinges on the respective significance of agriculture as opposed to the others, which is why we introduce the concepts of sufficient conditions and limiting factors. Bellwood protests that we "deny the significance of food production in recent population diasporas" and that "equating [Austronesian] migrations with maritime technology alone is insufficient." Yet we do neither. The words "deny" and "alone" are his, not ours.

Oceania was settled by agriculturalists who also had the necessary seafaring technology. Pace both reviewers, to state these facts is not to take an extreme position on the hypothesis in either direction. Neither alone was a sufficient condition. Nonetheless, the limiting condition was not agriculture. Many other populations in the region had agriculture, without this enabling them to colonize the Pacific. That could be achieved only by those who developed the necessary seafaring prowess. Only this completes the explanation of why the region's languages are of the Austronesian family rather than of the many other agriculturalist language families of Asia.

The same goes for all the historical-era language expansions that Bellwood cites. Yes, agriculturalists are involved, but simply noting this does not elevate agriculture into necessarily the principal explanation, for it is not the limiting factor. We leave Bellwood to argue out with historians his unusual interpretation that the Spanish conquistadors were much motivated or empowered by farming. And while the intensity of

Roman colonization does in part explain the particular distribution of Romance languages, it hardly answers the much greater question of why those regions speak Romance at all, rather than, say, Celtic or Etruscan. These linguistic outcomes derive not from Bellwood's chimera of who had a desire for land but from who had the wherewithal (in social organization, technology, military prowess, etc.) to take and hold it. How many indigenous peoples did not want their own lands, whether to farm or to forage in, any more than the invading populations who took them?

For Bellwood to suggest that our position differs little from his own is a bit rich, then. He seems blind to the value of the generalization we set out clearly here. We are far from denying agriculture any role but do recognize that its relative significance—and its ability to explain and identify which language families expand where and when—diminish as "more proximate factors and historical contingencies" progressively take on their own defining roles. Bellwood is mistaken: to invoke them is indeed immensely "helpful," for there is nothing else, certainly not agriculture per se, that can explain the distributions of particular majority languages across both New and Old Worlds today.

Specifically on the Andes, Bellwood's claim that the region presents no exception to Old World timescales of agricultural intensification is in error, for the Late Preceramic (his "Caral") does not equate with his "initial cultivation," which began in the Andes many millennia before. (Haas here goes so far as to call Late Preceramic societies "fully agricultural.")

Golla's zeal to do down the hypothesis, meanwhile, tempts him into a rather disingenuous presentation. Readers from outside linguistics may have trouble distinguishing established facts from his own personal claims. His magic numbers for the time-depth ceiling on linguistic reconstruction, for instance, gloss over many inconvenient but critical qualifications equally well known to "every competent historical linguist."

Golla purports to welcome the proximate-ultimate distinction that we propose. Yet he goes on to claim that certain language families undermine the hypothesis precisely where we argue that it has most value, namely, for the greatest, oldest language expansions. His supposed counterexamples, however, turn out to be no such thing. His claim that Dene-Yeniseian is definitively established is overstated. A number of linguists remain sceptical, including leading Americanists such as Lyle Campbell (personal communication). And even if confirmed, while perhaps a considerable achievement in terms of the philological challenge overcome, it would certainly be neither "ironic" nor any "major triumph" in the debate over the agriculture–language dispersal hypothesis, for Dene is distributed primarily in the Arctic and sub-Arctic. All that a link with Yeniseian does is extend the family's distribution to a minor extra branch in the same ecozone—a more pertinent fact than that it lies on the other side of the Bering Strait. Dene-Yeniseian, then, has nothing to do with any hypothesis that contrasts hunting and gathering with intensified agriculture and everything to do with subsistence in environments in which agriculture is simply not possible at all. Self-evidently, the hypothesis is not imagined to apply here in any case.

Golla's other examples include Eskimo, and one might also cite Pama-Nyungan (if, unlike Dixon, one accepts it as a valid family), likewise spread across an environment unfit for agriculture for other reasons: the Australian desert. But it is anthropologically somewhat naive to derive worldwide analogies from such marginal environments to which hunter-gatherers have been confined only by the subsequent demographics of agriculture. Indeed, it is surely no accident that the largest hunter-gatherer language families are to be found precisely in such environments. (Moreover, to lump these together with the rich pastures of the Eurasian Steppe, Nichols's prototype "spread zone," only betrays that very concept as too broad to be helpful here.) Finally, impressive as the major hunter-gatherer language families may appear in Mercator projection, they are still less extensive territorially and much less so demographically than the greatest language expansions.

Turning to the Andes specifically, we quite agree with Dillehay that pastoralist societies doubtless played a significant role in cultural developments here, linguistic ones among them. We need only clarify, then, a few points on which his concerns are in fact at cross-purposes to ours here. First, we see the likely linguistic impact not in the divergence of Aymara or of Quechua, our focus here, but in the striking convergence by contact between them. Traditions of complementarity between herders and agriculturalists may well be instrumental in accounting for this convergence, especially the most recent phase in precisely those regions of the southern Andes that Dillehay identifies (see the chapters by Urton and Sillar in Beresford-Jones and Heggarty, forthcoming).

Second, this misunderstanding may go back to a difference in terminology. As we state, we follow the linguistic usage whereby "Aymara" (like "Quechua") refers to the language family as a whole. Dillehay seems instead to follow popular usage whereby "Aymara" refers only to its best-known surviving branch, that is, the southern Aymara spoken in the Altiplano. The Aymara expansion we discuss, then, is the first and greatest one that gave rise to the entire, broad family, at a time depth of up to 3 millennia, out of a homeland far to the northwest of the Altiplano. Dillehay refers only to the much more recent stage of the family's final, southernmost expansion.

Third, Dillehay thus assumes that we take Quechua as the language expansion identifiable with the Early Horizon—but we do not. As per figure 3, time-depth estimates hint that it is Aymara that more likely corresponds to this time depth. For other reasons, too, we would associate Quechua primarily with the Wari Middle Horizon instead (see Beresford-Jones and Heggarty, forthcoming).

Haas feels our arguments are undermined by recent claims for the importance of agriculture (in particular, maize) as far

back as the third millennium BC. We agree that agriculture was gradually increasing in significance through the long Preceramic period, but the critical point is the coming together of a particular package that could propel language expansions on great demographic and territorial scales. For all the undoubted complexity of the archaeological record of the Preceramic, we disagree with Haas's interpretation that it yet represents such a package: it has no significant cereal, nor it is mobile or demographically expansive (away from the coast).

On maize, as Dillehay notes, the record is still more full of gaps than data. Yet those data published (see Pearsall 2008: 106, fig. 7.1) do suggest a step-change in maize in Early Horizon contexts with respect to earlier periods. Nor does this evidence come uniquely from coastal contexts. Finucane's (2009:538) isotopic data from two highland sites "suggest that maize had become the single most important component of human diet in the Ayacucho Valley by ~800 BC."

Haas finds it "misleading" to suggest that maize had a very modest role in the Preceramic, compared to maritime resources. Yet this, too, is precisely what all published data indicate. Our citations of Shady (2003, 2006, 2008), and of Haas and Creamer (2004) themselves, all underline its scarcity: "little maize has been found," "no role in daily diet," "never a primary source of calories," etc. And if Shady's (2008: 49) "huge quantities of fish and mollusks" translate for Haas into "[no] more than a condiment," then what of just 15 maize cobs found in the same excavations (Shady 2006, 2008) over the millennium of Caral's occupation?

Finally, when Haas downplays the Chavín Early Horizon, arguing that pan-regional integration occurred much earlier, he strays from orthodoxy. For while Andean archaeologists recognize the precocious developments of the Preceramic, most see little evidence that they had much impact beyond their individual coastal river valleys. As Burger puts it (in Beresford-Jones and Heggarty, forthcoming), "these pre-Chavín cultures were quite provincial" and do not constitute a "first pan-regional transformation of the Central Andes"—for which we do indeed have to wait until "the first millennium BC." And for those who would wish to see the Late Preceramic as the driver of a major language dispersal, the inconvenient linguistic fact remains: there is no evidence of any such expansion at those remote time depths.

In sum, the Andes do indeed stand out as an exception among the world's pristine hearths of civilization, in how both the agricultural and linguistic stories played out there. Valuable lessons emerge for the hypothesis that seeks to link the two worldwide. We welcome Renfrew's own recognition of our refinements "sometimes missed by the critics—and perhaps some of the proponents—of the hypothesis."

—Paul Heggarty and David Beresford-Jones

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