



The Case Against Linguistic Palaeontology

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Abstract

The method of linguistic palaeontology (or palaeolinguistics) has a controversial status within archaeology. According to its defenders, it promises the ability to see into the social and material cultures of prehistoric societies and uncover facts about peoples beyond the reach of archaeology. Its critics see it as essentially flawed and unscientific. Using a particular case-study, the Indo-European homeland problem, this paper attempts to discern the kinds of inference which proponents of linguistic palaeontology make and whether they can be warranted. I conclude that, while the case for linguistic palaeontology has often been overstated, so has the case against it.

Keywords Linguistic palaeontology · Wörter und Sachen · Proto-Indo-European · Historical linguistics

In 1710, the same year he published his proof that we were living in the best of all possible worlds, Gottfried Wilhelm Leibniz wrote an article on linguistics in which he argued not only that the Germanic and Celtic languages were descended from a common ancestor but that this common ancestor shared a further ancestor language with all Turkish, Slavic, Finnish and Greek languages. While he was wrong to lump the Finnish and Turkish languages in here, the overall idea was both correct and in direct conflict with the Biblical account of linguistic descent.¹ If he had a bit more data to hand he might have arranged it into a table like the following:

Icelandic	Irish	Latin	English	Czech	Sanskrit	Tocharian B
faðir	Athair	Pater	Father	Otec	Pitár-	Pacer
móðir	Mathir	Mater	Mother	Matka	Mátár-	Macer
bróðir	Brathir	Frater	Brother	Bratr	Bhrātar-	Procer
systir	Siur	Soror	Sister	Sestra	Svásar-	Ser
hundur	Cu	Kanis	Hound	Pes	Śván-	Ku
hestur	Each	Equus	Horse (eoh)	Hérák	Aśva	Yakwe

For Leibniz, the interest of this research lay not merely with determining genetic relations between languages but with the descent of peoples. ‘Languages in general, being the most ancient monuments of the human species... that serve best for determining the origin of peoples’ (Leibniz 1996 [1765], p. 285). Over two centuries later, the linguist Mary Hass echoed this sentiment when she described a reconstructed proto-language as ‘a glorious artefact, one which is far more precious than anything an archaeologist can ever hope to unearth’ (Haas 1966, p. 124).

The practice of making inferences about the cultures of language users on the evidence of reconstructed languages is called linguistic palaeontology (henceforth LP).² These inferences may concern the material culture and geographic location of speakers as well as their social relations, mythology, and beliefs (the notion of ‘archaeological culture’ is used to capture both material culture and behaviour). This

¹ Leibniz was not the first to make this observation. Marcus Zuerius van Boxhorn had proposed a common ancestor language called Scythian over half a century earlier and he was wrong to lump the Turkish and Finnish languages in this group as they each derive from their own language families, the Uralic and Turkic families respectively but he was largely correct when he wrote that: ‘It seems in fact that all languages from the Indus River to the Baltic Sea have a single origin’.

² There are numerous terms for this. While the term palaeolinguistics is becoming more common within the linguistic literature (Crowley and Bowern 2010, p. 316), linguistic palaeontology is the term found in the relevant archaeological literature. Cultural reconstruction is also a common term but suggests a focus on cultural artefacts over environmental features like salmon. *Wörter und Sachen* may actually be the most accurate term but no longer appears to be in use.

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method will be familiar to philosophers from Nietzsche's *Genealogy of Morals*, a book which hoped to tackle the question: 'What light does linguistics, and especially the study of etymology, throw on the history of the evolution of moral concepts' (Nietzsche 1967, p. 63).³

The primary aim of this paper isn't to solve philosophical problems through historical linguistics or to solve archaeological problems with philosophical methods, I doubt that can be done, but to encourage greater dialogue between the fields on the issue of LP. In this paper, I will be defending the rather weak-sounding claim that there is nothing *in principle* so unscientific about linguistic palaeontology such that it should be excluded from the considerations of archaeologists. While this claim may appear trivial, it is by no means uncontested. Linguistic palaeontology, according to Raimo Anttila, 'has rarely enjoyed particularly high repute' (Anttila 1989, p. 373).⁴ I will argue in this paper that both the defenders and critics of LP tend to overstate their case.

In the following sections, I will provide a brief overview of the comparative method in linguistics and its connection to linguistic palaeontology. I will then consider a case-study in which linguistic palaeontology has been utilised in a debate within archaeology: the Indo-European Homeland problem. The purpose of this discussion is to present some of the methods used within LP and examine the problems they face. I will argue that these are problems of evidence but not principle. That is, they are not problems which suggest that LP should be ignored by archaeologists, in fact, they highlight the need for greater engagement between the fields (and philosophy). In the final section, I will discuss the theoretical status of linguistic reconstructions and argue that reconstructions can serve as models for dead languages.

For the sake of clarity, I will define some terms now. A pair of expressions are called cognates when we can reliably assume that they descended from a shared ancestor. These descendant-words are known as reflexes of that ancestor. An archaeological horizon is a common set of traits or artefacts that identifies a culture at a time. And a proto-lexicon is a set of lexical items which can reliably be attributed to the speakers of the proto-language.

³ Nietzsche was a contemporary of Hermann Paul in Leipzig and a few years ahead of Brugmann, two leading members of the Neogrammarian movement.

⁴ While this quote has been invoked in the past by critics of LP, it's worth noting the line which follows it: 'But in any case it remains true that linguistic palaeontology very neatly provides assumptions to be tested' (Anttila 1989).

1 The Comparative Method

LP relies upon a reconstruction of elements of a proto-language which researchers can then use to draw inferences. These reconstructions are produced through the comparison of similar expressions in different languages in order to make inferences about their common ancestors. This *comparative method* is not merely based on the fact that certain words appear similar but that the similarities between them are systematic.⁵ It is the *systematicity* within the data that enables historical linguists to make inferences and frame hypotheses about the past of the language. Appeals to the wider context can help linguists sift out pseudo-cognates. For example, the Irish and Vietnamese words for 'cow' are *bó* and *bò* respectively but historical evidence suggests that it's implausible that these share a common ancestor. In contrast, the English *cow* and German *Kuh* are much more likely to share an ancestor. This data can be gathered into lists of cognates. While semantic intuitions guide this work, they are second to phonological features. On the basis of this data, rules of change can be conditionally proposed and proto-phonemes can be reconstructed. These are the basic phonological units of the proto-language.

The ultimate goal at this stage is the discovery of sound laws—though the term 'law' can be misleading. To prefigure later discussion, some linguists will be instrumentalists about these laws and simply identify them with regular or systematic correspondences in sound without making any further commitments. Alternatively, you can identify the laws with posited historical events that are particular to certain languages (see Greenberg 1979 for discussion of this point).⁶ Whichever interpretation one adopts, these 'laws' enable linguists to arrange languages into groups and families (e.g. P-Celtic and Q-Celtic, Slavic and Romance). To take one example, Grimm's Law of regular sound changes in Germanic from Proto-Indo-European ('PIE') has three parts: voiceless stops become voiceless fricatives, as we saw in the table above, voiced stops become voiceless stops and voiced aspirated stops become voiced stops or fricatives. Similar 'laws' have been discovered across language families

⁵ For a more detailed account of the method, see Crowley and Bowern (2010) esp. Ch. 5, and Anttila (1989).

⁶ According to the Neogrammarian slogan 'sound laws admit of no exception', a claim which has come under heavy criticism in recent years. 'The view that the sound laws are exceptionless is an illusion' (Barðdal 2015, p. 347) (see Hill 2014 for a defence of the claim that the laws are exceptionless). 'By "sound laws" they meant merely "sound changes", but they referred to them as "laws" because they linked linguistics with the rigorous sciences which dealt in laws and law-like statements' (Campbell 1998, p. 15). General laws do not exist or, at least, are not within the range of our ability to observe; one has to work with individual events' (Anttila 1989, p. 24).

(the middle step in Grimm's Law is mirrored by the Bantu shift from voiceless to voiced stops known as Dahl's Law).

Once a law or systematic correspondence has been discovered, it can then be reversed to reconstruct the state of the language before the shift occurred. This enables linguists to frame hypotheses about the phonological forms of the proto-language. One of the reasons that sound laws have the reputation they do is the success of particular predictions. A classic example of this is the reconstruction of the Proto-Germanic **gastiz* (guest) from later reflexes (see Anthony 2007, p. 31). The final step involves the reconstruction of the proto-lexicon. The number of words reconstructed will depend upon the number of reliably identifiable cognates discovered. There is some debate about how many cognates are required to reconstruct an entry in the lexicon and where those cognates are found will be an important consideration (for example, you shouldn't claim to have reconstructed a word in Proto-Indo-European if you have only found cognates in Romance languages). Reconstructed words are typically marked with a * to indicate their theoretical status (often what is reconstructed is not a whole word but a root). The endpoint of this method should be a list of words that one can reliably attribute to the proto-language. When the shift is made from phonological reconstruction to drawing inferences about the material and social culture of prehistorical societies, one moves from historical linguistics to a discipline much more closely aligned to archaeology.

This outline, while cursory, should provide some indication of how the history of languages can be reconstructed, however, it is important not to read too much into the resultant divisions between languages. Contrary to the popular image of a language as a tree, splitting discretely into distinct branches, the reality is often more complex. The tree-model of language divergence, as Bloomfield pointed out, does not reflect not a discovery but merely states the comparative method.⁷

The next section will exhibit one well-documented case-study which has been a source of disagreement between archaeologists and historical linguists, the origins of the Indo-European language family. Our purpose here is to highlight certain features of the method of LP.

⁷ Heggarty cautions that '[e]xcessive adherence to the tree idealisation has arguably been the single biggest spanner in the works that has frustrated and confused attempts to fit our languages more plausibly and coherently into population (pre)history as seen by other disciplines' (Heggarty 2015, p. 604).

2 The Proto-Indo-European Homeland Problem

There are two dominant theories of the origins of the Indo-European languages; the Pontic-Caspian-Steppe hypothesis and Anatolian hypothesis. The first of these locates the original speakers of Proto-Indo-European on somewhere on the Russian steppes, north of the Caspian Sea and posits a point of separation at around 3500–4500 BCE. This connects the Proto-Indo-European language with the Yamnaya horizon and in doing so suggests a bridge between linguistic and archaeological data. The Yamnaya horizon is an archaeological culture associated with kurgan cemeteries (tumulus burial mounds), corded-ware pots, and a system of nomadic pastoralism. This last point is of importance for the Steppe (and any other) hypothesis as some motivation is required for other cultures to adopt the Indo-European languages. Language displacement is often associated with some kind of social and technological advance and a prominent element of the Steppe hypothesis is the idea that the Proto-Indo-European language spread with nomadic pastoralism. Without some material basis in the archaeological data, it is difficult to construct a plausible narrative for widespread social change. Influential defenders of this claim are Marija Gimbutas and David Anthony.

The alternative theory locates the Indo-European homeland (*Urheimat*) in modern-day Turkey and suggests a point of divergence at around 7000–6000 BCE (the Early Neolithic). In this case, the socio-technological development spurring the spread of Proto-Indo-European is the development of agriculture. The displacement of earlier non-Indo-European languages was facilitated by both the population increase enabled by farming and the prestige attached to farming cultures. An advantage of this hypothesis is that the spread of farming techniques has been well-documented by archaeologists. This theory was propounded by the archaeologist Colin Renfrew and is supported by Grey and Atkinson (2003) and Bouckaert et al. (2012).

Both of these hypotheses have the virtue of identifying a relatively central area from which the Indo-European languages could have spread as well as having archaeologically attested mechanisms for this spread (pastoral nomadism or agricultural expansion). However, since the Proto-Indo-Europeans are fundamentally a linguistic posit, this is not a debate that can be decided by archaeological evidence alone. At the very least, a broad interdisciplinary approach is required. Which disciplines are invited to the table, however, remains a contested issue. Recent genetic data appears to support the Steppe hypothesis by confirming migrations from the steppes (Haak et al. 2015).⁸ On the other hand,

⁸ Some archaeologists have disputed whether this migration could have carried the Indo-European languages due to the distance between the dates for the breakup of the language and breakup of

Bayesian phylogeographic methods support an Anatolian origin (Bouckaert et al. 2012).⁹

A chief point of contention between proponents of each theory is whether linguistic palaeontology is a legitimate methodology. Renfrew writes ‘[t]he main reason for the failure to locate such a homeland arises, I think, first from an unwise reliance on linguistic palaeontology in a rather uncritical way’ (Renfrew 1987, pp. 97–98). In contrast, Robert Beekes, a linguist rather than an archaeologist, summarises Renfrew’s position as follows: ‘His theory happens to be contradicted by the linguistic and historical data which we have at our disposal, which is the reason why he must reject all the conclusions which have been reached by linguistic palaeontology’ (Beekes 2011, p. 50). We have an instance here of a debate between archaeologists such as Renfrew and Anthony which hinges in large part on the legitimacy of linguistic palaeontology as a method.

The claims made for linguistic palaeontology can appear quite extreme. For example, Anthony writes: ‘The proto-lexicon contains much more, including clusters of words, suggesting that the speakers of PIE inherited their rights and duties through the father’s bloodline only (patrilineal descent); probably lived with the husband’s family after marriage (patrilocal residence); recognized the authority of chiefs who acted as patrons and givers of hospitality for their clients; likely had formally instituted warrior bands; practiced ritual sacrifices of cattle and horses; drove wagons; recognized a male sky deity; probably avoided speaking the name of the bear for ritual reasons; and recognized two senses of the sacred (“that which is imbued with holiness” and “that which is forbidden”). Many of these practices and beliefs are simply unrecoverable through archaeology’ (Anthony 2007, p. 15).¹⁰

Gimbutas, appealing to mythology and material culture as well as linguistics, goes further when discussing the cultural

change enforced by Indo-European expansion: ‘These changes were expressed as the transition from matrilineal to patrilineal order, from a learned theocracy to a militant patriarchy, from a sexually balanced egalitarian society to a male-dominated hierarchy, and from a chthonic goddess religion to the IE sky-oriented pantheon of gods’ (Gimbutas 1993, p. 219). Others have argued that a ‘democratic revolution’ in the centre of the Indo-European linguistic sphere accounts for the absence of cognates for ‘king’, **rēǵs* in certain languages (discussed in Mallory 1976, p. 53).

These are clearly very strong conclusions to derive from linguistic evidence and it is understandable that critics would be suspicious. Rather than try to justify the strongest claims about the cultural reconstruction of prehistorical societies, I will confine my focus here to weaker claims which bare more directly on archaeological data; claims about the homeland of speakers of the proto-language. It is the homeland question that can have the greatest relevance for archaeology because it is through identifying a homeland that researchers are able to connect a language with a particular material culture. This isn’t to deny that linguists may practice semantic reconstruction with other interests. But we don’t need to vindicate the strongest claims made by the proponents of linguistic palaeontology in order to assess its value. While this focus may not be expansive enough for either Anthony (or Nietzsche), it seems to be the most defensible use of the method and the most directly relevant to archaeologists.

Before we continue, certain familiar caveats are required. The acceptance of the claim that Indo-European languages share a common ancestor does not commit one to the idea that there ever existed a homogeneous material culture or a unified linguistic community that spoke it. These ideas might be motivated by further archaeological evidence but the linguistic evidence does not compel them. Similarly, talk of a ‘homeland’ must be shorn of nationalist connotations. The homogeneity enforced, often violently, by the modern nation-state shouldn’t be projected upon prehistoric communities. There is little reason to believe that speakers of a proto-language, in virtue of sharing reconstructed linguistic items, considered themselves to be a single political community or that they maintained rigid borders with other communities. Furthermore, when dating the spread of these languages, the presence of reconstructions, particularly of terms referring to technological advances, does not tell us that the language or community sprang into existence at the same point in time that the technology did. If future linguists managed to reconstruct the word ‘internet’, they would be wrong to infer that the English language came into existence around the turn of the second millennium. What such a reconstruction might indicate is that the word was in use prior to the division of the proto-language into various families. A linguistic homeland is an essentially temporal notion.

Footnote 8 (continued)

Yamnaya culture. There are further questions about the migration of Yamnaya culture due to differences between Yamnaya and corded-ware pottery and burial mounds (see Klejn et al. 2018 for more information).

⁹ Bouckaert et al. (2012) have argued that the Bayesian phylogeographic method comes down decisively on the side of the Anatolian hypothesis. There is unfortunately not the space to examine this claim in depth here but for a detailed response and criticism of the phylogeographic methodology, see Pereltsvaig and Lewis (2015).

¹⁰ Each of these claims is motivated by their own independent arguments. The taboo on naming the bear is allegedly evidenced by the fact that the Indo-European term for bear, **h₂éftkos*, was not carried on into Germanic, Baltic and Slavic languages, all of which adopted what appear to be taboo terms for the animal; ‘the brown one’, ‘the ice fisher’ and ‘the honey eater’ respectively. It is also worth noting that Gimbutas’ theory is based on much more than linguistic analysis.

My aim is to make a distinction between practical and principled concerns with linguistic palaeontology. Practical objections concern the details of specific reconstructions and can be responded to (though may not be unanimously resolved) within a shared theoretical framework. Some of these will be discussed in the following. Principled objections are directed to the fundamental assumptions of the framework. I will argue that some of these do successfully undermine *a particular way of thinking about LP* but shouldn't lead us to throw the baby out with the bathwater.

To assess things, we need an account of the *standard model* of linguistic palaeontology. The claim that 'a community's culture is reflected in its language' (Bostoen 2007, p. 175), is intuitively plausible but how this can be turned into a scientific methodology is our focus here.¹¹ Specifically we must ask, what is the form of inference made by linguistic palaeontologists? The bluntest formulation suggests we can infer what objects existed in an environment from semantic reconstructions:

Reconstruction entails Referent: 'It is fair to assume that the things which the reconstructed words represent also actually existed' (Beekes 2011, p. 35).¹²

If this were the form of inference, it wouldn't be warranted. We can't infer directly from the existence of a reconstructed meaning to the existence of its referent. Some of the best-attested PIE expressions concern various gods while the present English language contains the words 'unicorn' and 'dragon'. Although the inference is commonly stated this way, it's best to take this as loose writing and not what proponents of LP actually believe. To handle mythological cases, some intermediary step is needed. A more plausible inference is:

Reconstruction entails Concept: 'If a word can be confidently reconstructed to the protolanguage (crucially, in both form and meaning), we may conclude that the concept it designates was known to the speakers of the protolanguage' (Epps 2015, p. 580).

¹¹ It's evident that not every word in English or Urdu can be traced back to Proto-Indo-European but there is reason to believe that the fact that a word can reflects the word's frequency of use (Pagel et al. 2007). The fundamental idea is that more frequently used words have a slower rate of change over time.

¹² This is a common formulation. 'Linguistic paleontology is based on the assumption that the things that the reconstructed words denote actually existed at the time when these words were used to denote them' (Pereltsvaig and Lewis 2015, p. 168). The authors do clarify in a footnote that they take mythological terms to denote concepts. A similar statement of this inference is: '[I]t supposes that if we can reconstruct a word back to the proto-language, then since they "had a word for it", the people who spoke it must have been familiar with whatever we assume that it then referred to' (Heggarty and Renfrew 2014a, p. 31).

This is much more plausible. However, I think it can do with a bit of revision as it can be jarring to say that words designate concepts since most of our talk is about objects in the world rather than our concepts themselves. Further, if you have a psychological theory of what concepts are, you might rightly be wary of the idea that we can uncover the psychological states of speakers of a proto-language from LP. It would be helpful to introduce the Fregean terminology which is commonly applied in the philosophy of language. According to Frege, a word expresses a *sense* which determines a *reference*. While the reference of an expression can be an object in the world, a sense is simply a mode of presentation of that reference; one referent can be picked up by multiple senses. For example, it is conceivable that the terms **kʷékʷlos* and **rot-eh₂* were used to refer to the same objects, wheels, but had different senses.

Now we can state the form of inference. Semantic reconstruction allows us to reconstruct the senses of expressions in the lexicon. If we can reconstruct the sense, then assuming that the expression doesn't explicitly concern some mythological being, we can infer that the speakers of the proto-language were acquainted with the object referred to, its *reference*. We can then use these referents to determine the homeland of the proto-language. The homeland will be a region in which these objects existed.

This method hasn't been confined to the question of the Indo-European homeland. Siebert (1967) placed the Algonquian homeland to the north of Lake Ontario based on shared names for biological species like 'seal' while others place it to the west of Lake Superior. Similarly, the reconstruction of a word for 'crocodile' is used to rule out Polynesia as an origin for Proto-Austronesian (Crowley and Bowern 2010, p. 312). The same methodology has also been applied to determine the origin of the Bantu languages.¹³ Much of what is known about the origins of the Romani people is determined by historical linguistics (Campbell 1998, pp. 73–74). The presence of loan words in the Romani languages tells the story of when the Romani left India and passed through Persian and the Byzantine empire into the west. For a critical account of the use of linguistic methods for Northern Fennoscandian prehistory, see Saarikivi and Lavento (2012).

¹³ See Vansina (1995), for discussion of the linguistic evidence concerning the Bantu Expansion; Blust (1993) discusses the Proto-Austronesian case. *The Lexicon of Proto-Oceanic* (Ross et al. 2017), should be noted for its incisive discussion of some of these issues.

3 Theory Internal Arguments

In the case of the Proto-Indo-Europeans, the most important reconstructions for determining the *Urheimat* are those for horse and wheel.¹⁴ The word for horse, **ekwo-s*, is attested in reflexes from Ireland to the Tarim Basin. The absence of horses from Italy, Greece and the Balkans appear to rule these regions out while the presence of domesticated horses on the Eurasian Steppe would lend support to this location. The case of wheels is even more significant for two reasons. Firstly, linguists have reconstructed five terms which appear to relate to wheeled transport, including two words for wheel, as well as words for axel, thill and the act of conveying something in a vehicle. Secondly, archaeological evidence suggests that wheeled vehicles weren't invented until 4000 BCE at the latest. This would appear to undermine the Anatolian hypothesis which places the spread of the Indo-European languages two millennia earlier.

Let's begin with practical questions. The main purpose of this discussion is to provide more detailed examples of the kind of reasoning involved when LP is applied to archaeology. The chief question about the term **ekwo-s* is whether it denoted wild or domestic horses. If it denotes domestic horses, then, it is argued, we can tie the PIE language to an archaeological culture possessing domestic horses (or a culture adjacent to one). As the earliest possible location for horse domestication is the Pontic-Caspian Steppe after 4800 BCE (Anthony 2007, p. 200) this would provide compelling support for the Steppe hypothesis.¹⁵ The problem though is that we have no guarantee that **ekwo-s* refers to a domesticated rather than a wild horse. As D'jakonov puts it: 'The Proto-Indo-European term for 'horse' shows only that horses *were known*' (cited in Bryant 2003, p. 116; emphasis added); it doesn't demonstrate that they were domesticated (Dolgopolsky 1993, p. 240). While horses were rarer in Anatolia, there is archaeological evidence that they were present.

What kind of evidence might decide this? The lack of reconstructions for a wider equestrian semantic field has been cited as evidence that **ekwo-s* originally denoted a wild horse. If horses played the central role in Proto-Indo-European society suggested by the Steppe hypothesis, one might expect reconstructions for foal, mare, gelding, stallion, steed and so forth. The absence of these expressions is used

as evidence to fix the sense of **ekwo-s* and thereby its reference.¹⁶ While cognate terms for 'horse-breaking' have been found in Celtic, Latin, Greek and Indic, the term has been applied to animals other than horses and so it may only have been applied to horses at a later date (Mallory and Adams 2006, p. 136). Can archaeological evidence help us fix the sense of **ekwo-s*? As you'd expect, this depends on where you look. It would be circular to assume that **ekwo-s* must denote a domestic horse on account of its place of origin, the Steppes, since the claim that it denotes a domestic horse is one of the arguments for the Steppe hypothesis.¹⁷ Further cultural considerations, however, can offer hints. The Indic *asvamedha*, Roman *Equus October* and some Irish inauguration ceremonies involved the ritual dismemberment of a horse (and in cases, bathing in its blood) which suggests but doesn't entail a shared ceremonial tradition.

The main objection against the *wheel* case is that technological terms have a tendency to spread as loan words. The words for computer in Yoruba and Albanian are *kómputa* and *kompjuter* respectively. It would be wrong to infer from this that each of those languages derives from a single common ancestor which possessed a proto-form of 'computer'. What is much more likely is that the term spread with the technology. The same can be said for the adoption of the word *mouse* to denote the hand-held pointing device. Both *mouse* and *computer* had meanings before they were transferred to a new technology (it's important to remember that entirely new words are rare). As a new technology spreads across cultures and linguistic boundaries, the terms for it spread as well (though not always). In any case, critics observe, even if the term wheel was present in Proto-Indo-European, there is no guarantee that it denoted the wheels on vehicles rather than potter's wheels or even the sun disk.

A final concern that must be addressed is morphology. Technically, we could reconstruct a word for 'mare' in Proto-Indo-European by deriving the feminine form of **h₂éḱqueha-*. This doesn't itself tell us that speakers of PIE had a concept of 'mare' as the productivity of morphology does not tell us about the reality of concept possession. The following excerpt captures this problem: 'The creation of agent nouns in the different Indo-European languages is so

¹⁴ For reasons of space, I will overlook the much-discussed reconstructions of plough, copper, and wool (for a detailed discussion, see Mallory and Adams 2006). While each of these are taken to support the Steppe hypotheses my aim here is merely to demonstrate the kind of argumentation involved in linguistic palaeontology and two cases suffice for this.

¹⁵ Similarly, the absence of horses in Greece and Italy before the bronze age is taken as evidence that PIE could not have originated in these areas.

¹⁶ This method follows Blust's first principle of semantic reconstruction: 'the meaning of a lexical item in an attested or a reconstructed language is determined by contrast within a field of lexical items of partially similar meaning' (Blust 1993, p. 38) which in turn stems from a Saussurian principle that the value of terms results from the presence of other terms (a kind of semantic holism).

¹⁷ Bryant (2003, pp. 116–117) charges Diebold (1976) with this kind of circularity. Diebold claims that we know that **ekwos* denotes the domesticated horse *Equus caballus* from archeological evidence, however, the reason we know that this archeological evidence is relevant archeological evidence is because of the linguistic evidence which locates PIE on the Steppe in the first place.

productive that there are few words for occupations that can be attributed to Proto-Indo-European with any degree of certainty. The lack of reconstructable occupational terms may also suggest that Proto-Indo-European society was not one with much occupational specialization' (Mallory and Adams 2006, p. 283). In other words, we can say that the reason for the lack of agent nouns is a result of the productive morphology of Indo-European languages or that Indo-European society lacked the division of labour which would produce such names. The first explanation is linguistic, the second is social, and it is not clear how we should choose between them.

4 Principled Objections

One might think that these issues can be resolved by simply providing more detail and that a bit more evidence might tip the scales one way or another. There is other data that I am omitting and which appears to support the claim that the terms for wheeled transport are neither loan words nor independently developed but must have been inherited from Proto-Indo-European (see Ringe and Anthony 2015). However, some opponents of linguistic palaeontology go beyond highlighting framework-internal objections, rejecting the framework as a whole. Probably the most articulate critic of LP, Paul Heggarty, writes:

'Linguistic Palaeontologists sometimes try to object that the *mouse* example [mentioned above] is not pertinent and insist on a case-by-case scrutiny of their reconstructions, but this misses the point. It is not the details of a reconstructed Proto-Indo-European form that are in question, any more than they are for the Proto-Germanic form of *mouse*. What *is* in question is the series of assumptions that linguistic palaeontologists make: about what a proto-form does or does not necessarily entail for the culture and date of the protolanguage—by virtue, in their view of the simple fact that they have been able to reconstruct it; and about how and when that form acquired the meaning that they now read for it' (Heggarty 2006, pp. 190–191).¹⁸

The suggestion here is that the problem with the method isn't simply a lack of data but something deeper. To assess this, we need to look at what the assumptions referred to are.

¹⁸ There are other principled objections which a philosopher might raise but which no archaeologist that I am aware of has raised. For example, one might be concerned that the terms of current languages are fundamentally incommensurable with those of prehistoric peoples such that we could never give an adequate gloss on items in a protolanguage. We might occupy such a radically different 'conceptual scheme' or 'form of life' that we could never truly understand their speech. The correct response to this charge is to point out that a gloss is not a translation. Our aim isn't to speak to prehistorical peoples but to geographically locate them.

As mentioned, there are two parts to linguistic palaeontology; the derivation of the meaning of a reconstructed term and the inference from the meaning [sense] to its referent with the ultimate aim of using this information to determine where the speakers of the language lived.

The first assumption is that we can determine the sense of expressions. There are two main arguments against this. The first goes as follows: the comparative method for phonological reconstruction is justified by the law-like nature of phonological change. No comparable laws exist for semantic change and therefore semantic reconstruction does not have the scientific credibility of phonological reconstruction. There may be general tendencies of words to undergo extension, narrowing, ellipses pejoration, and so forth, but these aren't systematic. For example, the fact that the Old English word for a wild animal, *deor*, narrowed in meaning to only apply to deer doesn't have many wider implications for the language. If we can't determine systematic laws of change, then we can't apply those laws to reconstruct earlier semantic contents. To do so is nothing more than a 'leap of semantic faith' (Heggarty and Renfrew 2014c, p. 1691).

This is a compelling argument if linguistic palaeontology is supposed to be carried out in the manner of the comparative method. That is, if the method of semantic construction involves the algorithmic application of rules to derive past semantic contents. There is no algorithm that can be applied to a cognate set that will give the sense of the original expression. However, it isn't clear that LP tries to use the comparative method at all. Rather, it is methodologically much more similar to archaeological interpretation in its attempts to discern the significance of (reconstructed) pre-historical artefacts.

The second argument is that semantic reconstruction is essentially vague and subjective: 'The very fact that the same technique can be invoked to construct cases for and against the same Steppe hypothesis reveals the widest objection to linguistic palaeontology in principle—its subjectivity' (Heggarty and Renfrew 2014c, p. 1691).¹⁹

I think we can break this criticism down into two components. The first is the claim that different meanings can be reconstructed for the same term; the second is that there is no *objective* method for deciding between two hypotheses. The first of these, the existence of opposing claims, is not a serious problem. The fact that multiple hypotheses can be formulated within a framework doesn't entail that the framework is 'subjective'. In fact, this is exactly what we should expect within any progressive (as opposed to degenerating) research programme. If it wasn't possible to

¹⁹ 'A great deal of this "cultural reconstruction" is insecure and subjective, reading into linguistic reconstructions far more than they really say' (Heggarty and Renfrew 2014b, p. 538).

formulate different hypothetical meanings for terms in the proto-lexicon, that would be a cause of concern. The method of semantic reconstruction should provide multiple hypotheses to work with and assess against the available data and we should accept disagreement among practitioners are an element of normal science.

The second part, however, suggests that this is deeper and that there may be no objective method for deciding between these hypotheses. For example, in the cases above, we considered multiple ways in which evidence might affect how we assess the plausibility of a particular reconstruction but it is unclear how we can assign a concrete value to the weight of this evidence. It is intuitively plausible that it is less likely that a word has independently emerged in multiple, independent branches of a language family. We should probably lend greater credence to a proposed shift in meaning if it follows one of the general trends of semantic drift identified (e.g. narrowing, metaphor, metonymy) than if it didn't but how much exactly this should influence our thinking is unclear. As critics note, words for technological developments are more likely to spread horizontally across languages than words for non-technological developments. In the case of the horse and wheel, the alternative hypotheses for their spread are that these terms independently emerged in different lineages or that they spread as loan words at some point after the spread of the Indo-European languages. In the case of horse, there is good reason to believe that the term wasn't derived independently. In the case of wheel, if we reject the hypothesis that it spread along with the rest of the reconstructed Proto-Indo-European expressions from the Steppes, it could have either developed independently or may have been a loan word which began to spread around 2000 years after the Indo-European languages first began to diverge. These hypotheses are possibilities but we don't have a method for assigning objective probabilities to them.

Some linguists have attempted to do this. Within the framework of Dyen and Aberle, ‘The differences of “likelihood” spoken of here are based on “judgements of differences” in probability. These are substitutes for true probability measures but are presumably reliable or are presumed reliable when they constitute expert opinion’ (Dyen 1985, p. 360). The subjectivity of the method can be best understood as the claim that we don't have an objective assignment of prior probabilities to a set of hypothetical reconstructions. This doesn't entail that we can't use any probabilities and rationally update them in response to evidence. What it does mean is that the probabilities assigned must be understood subjectively, as statements of the credence assigned to particular hypotheses by experts in the field rather than as objective probabilities. One may reasonably hope that as more data is collected which indicates the relative prevalence of different forms of semantic drift, models can become more precise.

At present, the inferences made in semantic reconstruction are neither deductive nor inductive. It isn't possible to deduce the original sense of an expression from a set of reflexes which may have shifted in meaning (e.g. *laks* means ‘fish’ in Tocharian B) nor is it possible to perform an inductive inference on the basis of the senses of the cognate set. The inferences performed are *abductive*; they are inferences to the best explanation. The best explanation for the current senses of a set of cognates is that they descended from a common ancestor which had a sense which would explain the current attested meanings. Reference to semantic reconstruction is a misnomer if reconstruction is understood in the sense of phonological reconstruction. If senses are not reconstructed from cognates but posited as the best explanation of why cognates possess the contents they do, then problems that linguistic palaeontology faces are problems of abduction in general. What counts as the best explanation may not always be clear. Yet normal criteria like simplicity, generality, and coherence can apply and are regularly applied in practice.

The final semantic issue is vagueness. It might be that *sense* is too fine-grained a notion to be appealed to in reconstruction. As Raimo Antilla observes, ‘it is surprisingly easy to devise abstract meanings to cover any two meanings’ (Antilla 1989, p. 369). If a set of cognates contains terms for sheep as well as goats, it is always an option to base the reconstructed sense on the union of these sets (e.g. small woolly animal). Perhaps the most famous instance of this problem can be seen in the now notorious *Lachsargument*. It was originally claimed that Indo-European term **laks* denoted the North Atlantic Salmon (*Salmo salar*) which is found in the Baltic and North Atlantic. This precluded the placement of the Indo-European homeland at any real distance from this region. In later work though, it was argued that **laks* actually denotes the Salmon Trout (*Salmo trutta*) which is much more widespread and would allow for homelands across Eurasia and that the term which originally denoted the Salmon Trout was only transferred to the North Atlantic Salmon by later tribes of Balts, Slavs and Germans. For either of these arguments to be convincing, one would have to accept that the claim that **laks* refers to *Salmo salar* entails that it does not refer to *Salmo trutta* and vice versa. In order to locate a homeland, we need to know which meanings a word excludes. In other words, we would have to accept that the Proto-Indo-Europeans would not have used this term to refer to both fish. The problem with this line of argument is that most English speakers would not be able to tell you whether the English word ‘salmon’ refers to *Salmo salar* or *Salmo trutta* exclusively.²⁰

²⁰ To reconstruct the meaning as the general term Salmonid (as is found in Mallory and Adams 2006) is to claim that speakers of proto-languages would not have considered the difference between the two kinds of salmon as worth marking lexically and that the Balts and

One might think matters are even worse. Familiar arguments for semantic externalism within the philosophy of language suggest that the reference of a natural kind term is not fixed by descriptive content alone (Putnam 1975; Kripke 1980). There are several ways this idea has been developed but the most influential is that a word must be embedded within a linguistic community which is causally related in an appropriate sense to the word's referents in order for that word to *refer* to those referents. However, if these arguments are correct, this merely highlights the need for archaeologists and historical linguists to work much more closely on these issues. In effect, what this would mean is that it is not possible to reconstruct the reference of an expression *without* an account of how the speakers of the language might be causally connected to the object referred to. Such an account would be the purview of archaeologists.

One thing left undisussed in the previous section is the role of arguments from absence. As one might expect, these are in some way the weakest arguments. For example, the absence of reconstructed words for 'olives' and 'vines' has been taken as an indication that the words did not exist in the proto-language and therefore that the original linguistic community did not originate near the Mediterranean. The natural challenge to this reasoning is that the absence of evidence is not evidence of absence. There is no word in the Proto-Indo-European lexicon for 'eye-lash' but we can presume its speakers had these. It takes some amount of luck for a word to be preserved in any form over thousands of years. The word needs to survive in multiple languages from different environments which may not contain the original referents. For example, one of the reasons why India is rejected as a possible point of origin of the Indo-European languages is the lack of cognates of words like 'elephant' in other languages. One might always choose to embrace the *modus tollens* of this argument and ask 'why should the Indic languages be held accountable for containing the names of exotica not evidenced in the western languages, and the western languages not have to account for their unique terms with no Indic cognates' (Bryant 2003, p. 113). It's plausible that the reason that Old Irish lacks a word for elephant was due to the lack of elephants in Ireland. For a word to be preserved in its original form, its original referent must be available to speakers of the daughter language.²¹ Despite these concerns,

Footnote 20 (continued)

Slavs did not change the meaning of the term when they applied it to *Salmo salar*.

²¹ Old Irish does have two reflexes of PIE terms for 'snake' despite the island's famous lack of snakes. One of these terms denotes eels and the other may have survived for religious purposes. This is an instance of a word acting like a parasite that will latch onto the bodies of different referents to survive. Alternatively, these developments could be understood as exaptation of traits, a change of function. These metaphors reflect different levels at which evolutionary think-

it would be foolhardy to reject this form of argument outright since it plays a vital role in archaeological reasoning (see Wallach 2019 for a range of examples). It would be odd to deny the historical linguist the ability to draw inferences from absence and grant it to the archaeologist, at the very least, further argument would be needed.

5 Formalism, Realism and Modelling in Reconstruction

There is a final principled objection a critic might raise which goes to the heart of the comparative method and undermines the similarities highlighted between the kinds of reasoning tolerated within archaeology and linguistic palaeontology. The argument proceeds as follows; the items reconstructed by the comparative method do not have the same ontological status as those discovered by archaeology. They are at most abstractions or names for correlations among existing objects, they are not discovered but created and therefore we are not in a position to apply the same methods of reasoning to them.²² There is a fundamental difference between drawing inferences from an artefact which has been dug from the ground and drawing inferences from a word that one has constructed.

This requires us to think about the kinds of theories we are building. How realistic are the reconstructions we are working with and what do we mean when we talk about their meaning?

The two main attitudes to linguistic reconstructions are *realist* and *formalist*. We have encountered both of these before above when we discussed whether sound laws are a statement of regular correspondences between languages or the postulation of historical events. There are strictly speaking several species of realism. The least plausible version treats the reconstructed language as a language like any other. If a linguist chose to learn the reconstructed form of a proto-language, they would be able to speak the same language as had been spoken in the past. The problem this faces is that the reconstruction lacks many of the properties

Footnote 21 (continued)

ing can be applied to understand cultural evolution. While this paper is focusing on more traditional methods of linguistic palaeontology, it would be remiss not to highlight the connections between the study of language evolution and the study of cultural evolution more widely within archaeology. Work in these fields have pointed towards general principles of evolutionary change (for example, Rogers and Ehrlich 2008) which may help constrain future work in LP as well as opening up new areas for inquiry.

²² In different forms, this argument appears in Pulgram (1959) and Tremblay (2005). For a valuable response to some of these issues, see Lass (1993, 2015).

of an actual language. It doesn't have any dialects so it could at most correspond to the idiolect of a single speaker. Even in this case, it could only correspond to a small portion of a speaker's language. If you were inclined towards a causal theory of either reference or word individuation, you might object that it doesn't stand in the appropriate relation to the original language to count as a part of the original language. One might wonder what the point of criticising such an implausible view is, however, failure to separate these strands of realism can lead to confusion. Pulgram 1959 argues that reconstructed Proto-Indo-European is not a language at all—a plausible thesis as it lacks a linguistic community or agreed syntax—but concludes from this that the reconstruction does not 'represent a reality'. This is a much stronger claim and depends on how one understands the relationship between a theory and its subject matter.

The concerns about the uniformity of the reconstructed language motivate the chief alternative to realism, *formalism* (or formulaism): 'For the formulaists, a reconstructed sound is merely an ideal notation with no claim for perceptual reality; in fact, the reconstruction of parent forms is a logical, not a historical operation' (Antilla 1989, p. 341). The network of phonemes derived has not phonetic reality. According to the formalist, Pulgram, 'no reputable linguist pretends that Proto-Indo-European reconstructions represent a reality' (Pulgram 1959, p. 434).²³ Anything in linguistics that is timeless, non-dialectal, and non-phonetic, by definition does not represent a real language. Again, I suspect the case is being overstated in this argument. It is true that the comparative method produces a uniform language lacking dialectical variation and that actual natural languages have dialects. However, this merely shows that what is reconstructed is not the whole language and it certainly doesn't entail that it can't represent the target language.

A more plausible way to articulate realism is to claim that the reconstructed terms denote expressions of the proto-language. The reconstructed lexicon, on this view, is not a reconstructed *language* but serves the metalinguistic purpose of referring to expressions in the proto-language. The plausibility of this view hinges on what you think a proto-language is. I take it for granted that few would wish to identify the proto-language or any other language with a set of linguistic tokens, i.e. concrete physical objects/actions which occurred in the past. The natural assumption then would be to identify the proto-language with a set of linguistic types, i.e. abstract, mind-independent objects. This is

²³ The spirit of this claim has been more recently stated by Fox: 'What we are *not* entitled to do of course is to mistake our idealizations [of a uniform proto-language] for reality. It is all too easy to interpret our idealisation of reality as though it were reality itself, and to draw inappropriate conclusions on this basis' (Fox 1995, p. 140). Fox's position is an intermediary between the realist and the formalist.

the approach Linda Wetzel takes in her discussion of Indo-European. 'Since the languages referred to here are spoken by many people, they are types, not spatiotemporal particulars, and so is the tree that accounts for their development' (Wetzel 2009, p. 6).²⁴

This approach possesses both intuitive plausibility and appears to extend across the sciences. Biologists, for example, don't study particular animals but species. Correspondingly, reconstructed lexical items don't stand for particular tokens of lexical items but for the types of those tokens. The problem with this view as presented is that word types are typically taken to be abstract, atemporal objects and the question of the Urheimat is predicated on the idea that a language is a spatiotemporal particular. A proto-language is necessarily a historical object; it is the posited last point at which two historically attested cognates met. I don't think this is a radically damning problem but it does suggest that we need to develop an account of the relationship between a reconstructed language and a historical language which captures the temporal nature of the historical language and the ideal nature of the reconstruction. The realist position has some positive features but, if it is to help us connect historical linguistics and archaeology, it cannot be endorsed in its Platonist form. That is, we should not commit ourselves to the claim that words literally *are* abstract objects.

This brings us to the third possibility which is neither realist nor formalist. Reconstructions *model* the conceptual abilities of a historical community. 'Model' is a contested concept in the philosophy of science and I don't intend to come down strongly for any particular theory. The general definition I will use comes from Godfrey-Smith: 'models are idealized structures that we use to represent the world, via resemblance relations between the model and real-world target systems' (Godfrey-Smith 2006, pp. 725–726). While these properties are neither necessary nor sufficient to render something a model, it should be relatively clear how the comparative method manifests them.

The comparative method results in an idealised representation of the proto-language. It provides us with a collection of terms which may have existed at different points over a long period of time and may have possessed dialectical variants. Although this fact has been wielded as an argument for formalism, we should acknowledge that idealisation is a central part of any kind of scientific theory. However, it is important though to distinguish the idealisation inherent in the modelling process from the claim that one is theorising about ideal, abstract objects. The mistake underlying the Platonist approach to types is that it takes properties of the model, its abstractness and use of idealisation, and

²⁴ As I mentioned above, it's better to avoid claiming that the tree *accounts* for the development of a language but to acknowledge that it reflects the process by which the proto-language was discovered.

projects them onto the object modelled. We must simplify and idealise what we are speaking about if we are to model it effectively but this doesn't entail that we should understand a language as a set of abstract objects existing independently of human activities.²⁵ Languages exist within a particular geographic and temporal horizon even if our models of them are abstract objects.

6 Conclusion

In this paper, I have argued that the principled objections to the use of linguistic palaeontology by archaeologists are not overwhelming. This obviously doesn't give a free pass to every absurd reconstructed meaning but nor does it mean that the results of semantic reconstruction should be ignored. The purpose of this argument was to do some ground clearing for what will hopefully be a more active engagement between the philosophy of language and linguistic palaeontology (as well as archaeology). There are two reasons for this. The first is that analytic philosophy of language has spent over a century developing sophisticated tools for the discussion of semantic issues none of which has influenced the discourse in LP. This paper barely touches on the potential for co-operation. The second is more sombre. We are currently undergoing a massive language extinction event. Over half the world's languages face imminent death. It is sometimes said that when a language dies, a culture and a way of viewing the world goes with it. Whether this is true (in a sense) depends in part upon whether or not the loss of a language results in the loss of irretrievable information and the answer to this question is, in an important sense, the possibility of linguistic palaeontology.

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Compliance with Ethical Standards

Conflict of interest The author declares that he has no conflict of interest.

Ethical Approval This article does not contain any studies with human participants or animals performed by any of the authors.

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²⁵ Godfrey-Smith uses the term 'imagined concrete things' to describe 'things that are imaginary or hypothetical, but which would be concrete if they were real' (Godfrey-Smith 2006, p. 735). This nicely captures the nature of linguistic reconstructions.

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