THE PROTOLANGUAGE DEBATE: BRIDGING THE GAP?

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Synthetic and holistic theories of protolanguage are typically seen as being in opposition. In this paper I 1) evaluate a recent critique of holistic protolanguage 2) sketch how the differences between these two theories can be reconciled, 3) consider a more fundamental problem with the concept of protolanguage.

1. Introduction

Humans have language. It is hypothesised that the common ancestor of chimpanzees and humans did not. Evolutionary linguists therefore have to explain how the gap between a non-linguistic ancestor and our linguistic species was bridged. It has become common to invoke the concept of a *protolanguage* as a stable intermediary stage in the evolution of language: "[t]he hypothesis of a protolanguage helps to bridge the otherwise threatening evolutionary gap between a wholly alingual state and the full possession of language as we know it" (Bickerton, 1995, p51).

What was protolanguage like? Under the *synthetic* account, advanced by Bickerton (see, e.g., Bickerton, 1990, 1995), protolanguage had symbols which could be used to convey atomic meanings, and these proto-words could be strung together in ad-hoc sequences. Language developed from such a protolanguage through the *synthesis* of these words into more and more complex, formally-structured utterances.

Under the (competing) *holistic* account, (see, e.g., Wray, 1998), protolanguage was a system in which individual signals, lacking in internal morphological structure, conveyed entire complex propositions, rather than semantic atoms. The transition from a holistic protolanguage to language was by a process of *analysis*, by which holistic utterances were broken down to yield words and complex structures.

Recent times have seen a number of critiques of holistic theories of protolanguage (most notably Bickerton, 2003; Tallerman, 2004, 2005). I will (briefly) review some of these criticisms in section 2. This review suggests that these com-

peting theories actually have rather different targets of explanation, and the apparent conflict between them can potentially be resolved. Such a unified account is sketched in section 3. However, this reconciliation highlights a more fundamental problem with theories which appeal to protolanguage as an intermediary stage in the evolution of language, namely that such theories are in danger of merely labelling the gap between alingual and lingual states, rather than bridging it.

2. Some criticisms of holistic protolanguage, and some responses

Bickerton (2003) and Tallerman (2004, 2005) highlight a number of potential problems with holistic protolanguage. The most thorough critical evaluation is Tallerman (2005), which provides a series of roughly 30 criticisms. I will outline and evaluate five of these here. The reader should appreciate that this is only a partial presentation and examination of Tallerman's arguments, the fuller consideration which her paper deserves requiring a rather longer treatment than this.

2.1. *Problems with learnability*

A first line of attack on holistic protolanguage is that it is not a viable communication system in its own right. I will focus on two such criticisms here. The suggestion in both cases is that *Homo erectus* (the species linked to protolanguage by Bickerton, Tallerman, and Wray) could not plausibly have learned a sufficient number of utterances to make a holistic protolanguage work.

2.1.1. *Argument 1: limited inventory size*

Tallerman's first argument to this effect is that *Homo erectus* would simply have a limited capacity for learning holistic utterances:

> "How many holistic utterances is it reasonable to assume that the hominid could learn over the course of a lifetime (of maybe 25 years)? . . . [For human infants] a reasonable estimate of learning rate is an average of 9–10 words a day from 18 months onwards. Assuming that the input was a set of holistic utterances, could this feat conceivably have been matched, even approached, by the smaller-brained erectus. . . ? I submit not."(p16–17)[a]

Is this a valid criticism? Firstly, as Tallerman herself acknowledges, it is unclear how many utterances are required to create a viable protolanguage, holistic or otherwise. This makes it difficult to evaluate how damaging this type of criticism actually is. Would a holistic protolanguage require, say, 1000 utterances to

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[a]Unattributed citations refer to Tallerman (2005). Page numbers refer to the in press version of this paper, available online at http://dx.doi.org/doi:10.1016/j.lingua.2005.05.004.

work? Or is less than 1000 actually sufficient? Or less than 100? How does this correspond to the numbers required for a synthetic protolanguage?

Secondly, why not assume that the capacity of *Homo erectus* to memorise signals is approximately the same as that of modern humans, e.g. on the order of 10^4 items (although, again, we can't say if this would be enough to make holistic protolanguage viable). Tallerman discounts this possibility because of the (relatively) small brain size of *Homo erectus*.[b] In order for this to be a factor, however, we need to know what, if any, relationship exists between brain size and maximum inventory size. Jackendoff (2002, p241–242), for example, speculates that there is *no* link between brain size and capacity for lexical memorisation. Much work remains to be done if Tallerman's hunch is to be vindicated and this criticism established as significant.

2.1.2. *Argument 2: holistic signals are harder to learn*

A further factor suggested by Tallerman as reducing the maximum inventory size of a holistic protolanguage, and possibly forcing it below the (unknown) viability threshold, is that holistic lexical items are harder to learn than their synthetic counterparts:

> "whereas lexical vocabulary can be stored by pairing a *concept* with the arbitrary sound string used to denote it, holistic utterances must be stored by memorizing each complex *propositional event* and learning which unanalysable string is appropriate at each event. This task is harder"(p17).

The simple response to this argument is "why?". Why is it harder to memorise an association between a signal and an atomic concept (a predicate or argument, say) rather than a proposition involving both a predicate and an argument? Is it twice as hard to memorise the latter? Or does difficulty of learning increase exponentially with number of semantic atoms attached to lexical items? How does this putative increase in difficulty compare with the difficulty of identifying the individual semantic contribution of words in a synthetically-constructed protolanguage utterance? Tallerman offers no insight on the basis for this claim, or on any tradeoff between the two alternative tasks, or means in which it might be investigated. Without further support, this criticism seems mainly a matter of assumption.

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[b]She actually offers several objections, the full quote being "could this feat conceivably have been matched, even approached, by the smaller-brained erectus, *lacking any linguistic cues, no fixed phonemic inventory, and with only the vaguest idea of the intended meaning of the holistic string*?" The proposed deficiencies are all outcomes of earlier argumentation in Tallerman (2005), and are themselves open to dispute. Given the limited scope of this paper, this argumentation will be omitted.

2.2. *Problems with analysis*

Analysis, also sometimes referred to as segmentation or fractionation, is the process by which holistic utterances are broken down into component words plus rules which govern their combination. Wray (1998) describes a scenario under which chance co-occurrences of meaning and surface form between holistic utterances lead protolanguage learners/users to segment out words, leaving behind a residual template. The accumulation of such analyses over time eventually leads to a system of words and grammatical structures. Computational models have shown that a similar process can, in principle, lead to a transition from holistic protolanguage to compositionally-structured linguistic systems (see, e.g., Kirby, 2002).[c]

Tallerman provides two arguments suggesting that a holistic protolanguage is not a plausible precursor to language — that the transition from a holistic protolanguage to language via a process of analysis would not be possible.

2.2.1. *Argument 1: The problem of counterexamples*

Tallerman states the problem as follows, classing it as "major":

> "logically, similar substrings must often occur in two (or more) utterances which do *not* share any common elements of meaning at least as many times as they occur in two utterances which *do* share semantic elements. ... The holistic scenario is, therefore, weakened by the existence of at least as many counterexamples as there could be pieces of confirming evidence for each putative word." (p19–20)

Were this accepted, we might indeed doubt any account requiring transition via analysis from holistic protolanguage to language. There are, however, two problems.

Firstly, it is not a *logical necessity* that counter-examples outnumber confirming cases for any possible segmentation — this is certainly a possibility, but we can trivially construct a case where there are *no* counter-examples to a particular segmentation. The number of counter-examples to a segmentation depends on the set of utterances under consideration, and cannot be deduced *a priori*.

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[c]A frequent criticism of these models is that, typically, learners are provided with meaning-signal pairs during learning: "If the problem space were not limited in this way, the simulations simply wouldn't work — the agents would never converge on a workable system. But such unrealistic initial conditions are unlikely to have applied to our remote ancestors" (Bickerton, 2003, p86). Such comments reveal two regrettable, though common, errors. Firstly, this modelling decision does not embody an (unrealistic) assumption about "initial conditions", but rather an idealisation which allows another aspect of the process to be addressed and understood. Secondly, the fact that the analysis process works in models which make this idealisation does *not* demonstrate that analysis would not work if this idealisation was relaxed — in order to make this point, such a model must be shown not to work. This has not been done, to my knowledge.

What if *in practice* we find that, in any holistic system of a reasonable size, counter-examples tend to outnumber supporting cases? Does that mean that all possible segmentations will be blocked, and the analysis process will never get started? This depends how the analysing learner/user deals with counter-examples. One possibility, as suggested by Tallerman, is only to segment if the evidence for a given segmentation outweighs the evidence against. An alternative approach is to segment at the earliest opportunity, on the basis of local pairwise comparison (as in Kirby, 2002), in which case the number of counter-examples to a given segmentation is irrelevant.

What do human language learners do — do they weigh up the number of possible counter-examples to an apparent regularity, or do they work on purely local comparison, or do they do something more sophisticated? Tallerman offers no comment on this, nor on a more directly relevant question: what did *Homo erectus* do? Until that question can be answered (and assuming an answer is possible), we cannot use the possibility of counter-examples to argue that analysis of a holistic protolanguage is impossible.

2.2.2. *Argument 2: The problem of surface instability*

Tallerman's second criticism of the analysis process is to argue that (premise 1) the analysis process requires *consistency of expression* (forms which are underlyingly the same are recognisably the same in surface form), and (premise 2) holistic protolanguage could not plausibly exhibit consistency of expression.

Tallerman offers several persuasive arguments in support of premise 2: synchronic consistency is unlikely due to factors such as allophonic variation, and allomorphic variation in any emerging semi-analysed system; diachronic inconsistency will inevitably arise as a consequence of processes of sound change. To summarise, "variation cannot help but exist because once hominids have a vocal tract in anything approaching its modern form, then specific phonetic tendencies appear spontaneously." (p9). Premise 2 therefore seems secure. What about premise 1 — does analysis really require synchronic and diachronic consistency of expression? Tallerman's three arguments here are considerably weaker.

Her first argument is that chance similarities cannot occur in a system which does not exhibit consistency of expression: "if the emerging stems aren't consistently audible in a fixed form, how can the chance similarities ...ever arise?" (p12). This is simply incorrect: chance similarities can of course occur in such a changing system, just as they can in a system where stems are audible in a fixed form. To give a concrete example, chance similarities between the lottery draw and the numbers on your lottery ticket are possible even if you change your numbers every week.

The second argument is that inconsistency in surface form may somehow obscure the intended *meaning* of a holistic utterance: "it's even harder for the speakers to decide on an agreed holistic message for any given string, because any given

string is constantly being eroded, assimilated, and so on" (p12). This suggestion needs more support. Why does sound change inhibit the acquisition or negotiation of meaning for an utterance? Is a similar process known to occur in attested instances of language change, such that words which undergo sound change have an increased likelihood of undergoing subsequent semantic change? Given the current lack of support for this claim, we may have to remain sceptical.

The third argument has to do with the damage done by sound change: "How, then, could the fractionation have proceeded successfully over ...hundreds of thousands of years, when the material the speakers were working on was continually slipping out of their grasp, changing the validity of any hypothesis formed by one generation and demolishing the emerging system?" (p11). This is an interesting question — can analysis proceed when an emerging regularity may be obscured by sound change? There are, however, grounds to think that this final argument is also incorrect. In attested language change, paradigms which have been damaged by sound change can be repaired by analogical levelling (see, e.g., Trask, 1996, for examples). Kirby (2001) uses a computational model to demonstrates that analysis can, in principle, still work despite destructive sound change. Tallerman's premise 1 therefore seems rather shaky: analysis can derive structure from a holistic system despite synchronic and diachronic inconsistency of expression, and Tallerman's position that it cannot remains to be demonstrated convincingly.

2.3. *Uniformity of process*

Tallerman mounts a more damaging criticism of holistic protolanguage in relation to uniformity of process:

> "We have a very good idea where [for example] grammatical morphemes come from in fully-fledged language: they are formed from lexical morphemes, specifically from nouns and verbs, via the bundle of processes known as *grammaticalization* ...The null hypothesis is that the same processes were at work in the earliest forms of language ...to propose a holistic strategy involving fractionation is to ignore the known processes by which words come into being in language"(p18)

This is potentially a serious problem for holistic protolanguage, and one which its proponents must address. One possible avenue of response is to attribute the apparent discontinuity to radically different *inputs* to a single mechanism. A recent trend has been to view children's' acquisition of syntax as the conservative extraction of regularities and generalisations from utterances which are initially under-analysed (see, e.g., Tomasello, 2003). These theories of acquisition are compatible with an account of analysis of holistic protolanguage. The difference in outcomes (segmentation and analysis versus synthesis and grammaticalisation) can then be attributed to differences in input — when presented with an input

which has undergone thousands of generations of analysis already, subsequent analysers are at least likely to proceed more rapidly and further than early analysers, and may proceed in a rather different direction altogether. This possibility, however, needs to be developed considerably if it is to constitute a valid response to Tallerman's criticism.

3. Bridging the gap?

What is Tallerman's own position on the nature of protolanguage, and its role in theories of language evolution? Firstly, for Tallerman protolanguage had nouns and verbs: "Once nouns and verbs come into being, well-understood linguistic processes will do the rest" (p18). Furthermore, a theory of protolanguage is not required to explain the origins of these categories: "Nouns and verbs more or less invent themselves, in the sense that the the protoconcepts must be in existence before hominids split from the (chimpanzee) genus *Pan*" (p18). Secondly, Tallerman suggests that protolanguage users had available to them pre-grammatical ordering and grouping principles, and that the origins of such principles do not require much explanation: "Given that it is well known that apes in language training experiments can spontaneously adopt ordering . . . and even parrots can be trained to pay attention to sequencing of symbols . . . , it would be very surprising if our hominid ancestors did not share that same skill"(p21).

Tallerman is therefore unconcerned with the origins of words (nouns and verbs, at least) and ordering constraints. Wray offers an explanation for the origins of such features, via the analysis of a holistic protolanguage. The two theories therefore seek to explain different aspects of linguistic structure and seem to be compatible, at least potentially. To give the bare bones of one possible unified account: a holistic protolanguage undergoes analysis to deliver up nouns, verbs, and some conventionalised ordering principles; the resulting synthetic protolanguage then feeds into known processes, such as grammaticalisation, to deliver fully modern language. Insisting on an account involving only one "true" protolanguage either risks assuming away part of the phenomenon to be explained (as Tallerman does), or ignoring known process acting in the formation of linguistic structure (as Wray does).

What follows if we relax this constraint, and allow room for *two* protolanguages, rather than merely one, as in the unified account sketched above? If we admit this first subdivision of "protolanguage" into two stages, is there any reason to reject further subdivisions, reflecting the development of phonological systems, emerging paradigmatic structure, evolution of function words, and so on (as in, e.g., Jackendoff, 2002)? The division into holistic and synthetic protolanguage is then rather a simplistic one — there are other alternative labellings of the stages, based on the presence or absence of other characteristic features of language which must be explained. In this case we might reserve the single term "protolanguage" to cover the series of stages, rather than reifying any one

particular stage as *the* protolanguage.

Of course, there is no requirement that these sub-stages be strictly segregated — for example, new segmentations delivered up by analysis might enter immediately into grammaticalisation processes, while other holistic utterances and parts of utterances are further broken down. In such a scenario, where different processes overlap temporally and interact, it makes little sense to see the process as consisting of a series of two or more discrete, stable steps.

Is there still a useful place for "protolanguage" in this more pluralistic conception of the evolution of language? If there is no single, steady state corresponding to protolanguage, but rather a continuous transition to language, with multiple aspects of linguistic structure being at different stages of development and entering into different interactions at any one time, then the concept of protolanguage is not really bridging the gap between alingual and lingual states, but rather labelling it.

Acknowledgements

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