

## REVIEW ARTICLES

# Can we know how language began?

Andresen, Julie Tetel. 2014. *Linguistics and Evolution: A developmental approach*. Cambridge: Cambridge University Press. pp. vi + 308. Paperback. ISBN: 9781107650114

Hurford, James R. 2014. *The Origins of Language: A slim guide*. New York: Oxford University Press. Paperback. pp. 173. ISBN: 978019870188

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Beginnings always fascinate. When historical linguistics got going seriously in the nineteenth century, although the bulk of the research was about extrapolating the historical record back through the short, late segment of linguistic prehistory that can be inferred from it, there were also linguists who thought about the question of how languages first arose in languageless societies. The leading interpreter of the new German discipline of linguistics to British audiences was Max Müller, holder of successive Oxford language chairs from 1850 until his effective retirement in 1875. Perhaps the best-known aspect of Müller's teaching was the catchy set of names he coined for alternative ideas about the origin of language: there was the bow-wow theory which held that the first words were imitations of natural sounds, the yo-he-ho theory which held that speech evolved out of the sounds that accompany physical effort, the pooh-pooh theory which took language to have originated in expressive exclamations, and several more. (Not all these names were invented by Müller, but the idea of naming language-origin theories in this style began with him.)

Julie Andresen (pp. 155–6) takes Müller to have used such names in order to “stigmatize” or “disparage” the theories, and some reviewers did take offence at the time. But Müller apologized in a footnote in his second edition, explaining that the names were chosen because they were clearer and briefer than terms such as “onomatopoetic” or “interjectional” (Müller 1862, 304, n327). To my mind they are apt, memorable, and pleasingly unpompous names.

But this line of thought soon came to an end. One thing that every undergraduate studying linguistics is taught about was the decision in 1866 by the newly-founded Paris Linguistic Society to exclude discussion of the origin of language

from its meetings. The topic was seen as just too speculative to be open to concrete scientific progress. And this cannot have been an idiosyncratic, unnecessary decision, because seven years later the British counterpart, the Philological Society, adopted the same rule. These bans were effective in moulding the way the discipline developed. For much of the twentieth century, the topic received very little attention.

Things have changed in recent decades. The earliest indication of this which I noticed was a 1975 conference on this once-banned subject (Harnad et al. 1976). (One might add from the same period Stam 1976, which examined theories of the origin of language put forward in the eighteenth and early nineteenth centuries.) And what began as a trickle has grown into a steady stream. Recently, Tallerman and Gibson (2012) divided the field systematically between sixty-odd scholars, each of whom was asked to summarize the state of knowledge in his or her respective sub-area; not all those writers are specialists solely in the origin of language, but for many of them it is a fairly central concern. And now the two most venerable British university presses have each brought out a shorter book that aims to present the topic in a digestible format to newcomers. Of the two books on which this article focuses, the one by James Hurford — who was once described by Steven Pinker (1994, 295) as “the world’s only computational evolutionary linguist”, though by now there are others — is essentially a brief summary of two much longer books by the same author, Hurford (2007) and (2012), and it aims to steer a judiciously balanced path between opposed views on matters of controversy. Andresen’s is more of a campaigning volume, pushing a point of view which Julie Andresen feels has been unjustly ignored by mainstream linguistics. As she puts it in her opening sentence, “What is needed for a twenty-first-century linguistics is an understanding of language that is inspired not by Descartes but by Darwin”.

(By opposing Darwin to Descartes, Andresen presumably intends to contrast a view of language as a static system expressing fixed innate ideas with a view that sees it as a changing, developing activity — “*linguaging*”, in Andresen’s usage, rather than “*language*” or “*languages*” as nouns. But I wonder whether Andresen realizes how Cartesian or even Chomskyan Darwin could be when he discussed human cognition: “As far as concerns infants of from ten to eleven months old, and deaf-mutes, it seems to me incredible, that they should be able to connect certain sounds with certain general ideas as quickly as they do, unless such ideas were already formed in their minds” (Darwin 1874, 136).)

Perhaps the single most central issue for any theory about the origin of language is the one which Andresen (p. 104) expresses as “did human language develop out of animal communication systems in a continuous line of development or was there a break?” This question relates to the Chomskyan idea that the structure

of all human languages is largely encoded in our genes rather than learned from experience, but these are two distinct issues. If languages are purely learned rather than innate systems, then surely they must have emerged from a process of gradual development, but among those who believe that language structure is innate, while Noam Chomsky holds that it was created in one of our ancestors through a sudden genetic saltation, Steven Pinker believes that the relevant genetic properties emerged from a process of gradual Darwinian evolution. Hurford and Andresen both make clear that they believe in continuity. Neither seems to believe in innate grammar, indeed Andresen's closing paragraph suggests that much of her motive in writing her book was to reject Chomsky's account of language; but Andresen and Hurford would both like to say that, with respect to language acquisition, "nature v. nurture" is somehow a false opposition.

Hurford argues this by reference (p.38) to simulation experiments in which intercommunicating agents modelled in software evolve language-like systems from a languageless initial state via a process of cultural (i.e. not genetic) transmission. Taken as a model of how language might have arisen among mankind, Hurford says that this "is not at odds with ideas of a 'language instinct' ". But this avoids the issue posed by Pinker's book of that name. Pinker (like Chomsky, at least until his recent writings) holds that we have not just an instinct to use language but an instinct to acquire and use some language having very specific, arbitrary properties defined by a theory of Universal Grammar. The main objection to Pinker's language-instinct idea is the observation that the languages of the world in fact share few or no universal properties (cf. Evans and Levinson 2009).

Andresen (p.51) seeks to eliminate the nature/nurture issue by reference to a concept, due to Susan Oyama, called constructivist interactionism, "in which the ontogeny of an organism constitutes a perpetual coming-into-being", so that "nature does not preexist ontogeny as an always-already-there-but-dormant essence waiting to be 'awakened' ". What Andresen says about constructivist interactionism occupies quite a large fraction of her book, but I find it very hard to follow. It is perhaps not fair to judge this aspect of Andresen's thesis without extensive study of her sources, and I have not done that. (It would not be easy to do, considering that one work on which she relies heavily is an unpublished document circulated by a Chilean natural history museum.) But she illustrates the idea with a concrete example, to do with teeth, which speaks for itself. On p.41 Andresen says "[Oyama] would say that the number and organization of human teeth are not innate; they are developmental *results*". How are these things not innate? Most adult humans have 32 teeth. A minority (to which I happen to belong) have two fewer in the upper jaw. The orthodontist who first discussed this when I was a boy told me that it was the consequence of a recognized genetic mutation. I suppose he could have been mistaken, though I doubt it, but one could surely not say

that his assertion was conceptually untenable? Some individuals may have only 30 teeth as a consequence of interaction with their environment (a couple of teeth get knocked out in a fight), but the claim was that in my case the difference from 32-tooth people is genetic rather than environmental — a matter of nature rather than nurture. Whether right or wrong, the statement that the number and organization of human teeth is controlled genetically makes perfectly good sense, and so does the claim that the detailed structure of human language is controlled genetically. If one disbelieves that claim, as I do, one must show that the evidence is against it, rather than asserting that there is no issue to be decided.

The rather muzzy quality of the authors' treatment of nature v. nurture carries over to some extent into their treatment of the continuity issue. Hurford's discussion of this makes heavy use of the prefix "proto-". He assumes (p. 37) that full language was preceded "by a 'protolanguage' stage", and that full human concepts were preceded by a stage of "proto-concepts" (pp. 61, 73). If there was indeed a continuous path of development from non-language to language, presumably there must have been intermediate stages to which these "proto" terms were applicable, but I am not sure they really add much to the basic idea of continuity between non-language and language. If other species have concepts or at least "proto-concepts", for which Hurford says the evidence is now convincing, why does no other species have a "protolanguage" that seems at least on the way towards full human language? (Hurford sees the communication systems found in some other species as different in kind from human language.) Indeed, what exactly is a proto-concept? On his pp. 63–4 Hurford discusses the cognitive processes which he envisages as underpinning a concept FLY in the brain of a frog: but everything before the final sentence relates to particulars (individual patterns of light and shade, motion, and so forth impinging on the frog's organs of sense on specific occasions), and then suddenly, as if by magic in that final sentence the universal term FLY appears. How human beings abstract universals (in the logician's sense) from particulars is a mystery, and saying that our remote ancestors, or other species today, do the same thing at a simpler level is probably true as far as it goes but does not really dispel the mystery.

There may be an analogy here with the problem of consciousness, much discussed by philosophers recently. Human beings, when awake, are vividly conscious of their surroundings. A camera or a heat-seeking missile, although they both possess organs for gathering data from the outside world and acting in response, are presumably not at all conscious — they are "just machines". Some suppose that this difference is merely a function of complexity, so that a purely physical system, once it reaches a certain level of complexity, will possess consciousness — perhaps some computers or advanced software systems already have proto-consciousness, or will have in the future. But the philosopher David Chalmers (1996, xi) says

that this misses the point: “Present-day scientific theories hardly touch the really difficult questions about consciousness. We do not just lack a detailed theory; we are entirely in the dark about how consciousness fits into the natural order”. (He goes on to suggest that this mystery will only be solvable if we accept that mind is a separate substance not reducible to physical matter.) Likewise I am not sure that positing simpler forms of language preceding the fullscale languages that we know today, although it may well be correct, does much to help us understand how our ancestors made the leap from not having language to having simple languages.

We certainly know a great deal more today than the nineteenth century did about the ancestry of *Homo sapiens*, the communication systems of some other species, cognitive processes in neighbouring species such as chimpanzees, and so forth; and Hurford’s book in particular is a good guide to that material. But the fact is that the sum total of this new knowledge does not seem to take the question of the origin of language all that far beyond where it stood in Max Müller’s day.

Consider, for instance, the idea that the first speech grew out of singing (the “la-la theory”). It is a possibility, certainly. It has been advocated by some linguists of the past, such as Otto Jespersen (quoted in Mithen 2012, 296). Hurford (p. 130) says that Charles Darwin believed it, though he gives no reference either there or in his longer *Origin of Grammar* book (2012, 53); if Hurford is thinking of Darwin (1874, 132–3), to me that passage reads rather less specifically. Hurford is cautiously negative about the idea:

A striking fact about birdsong generally is that the number of notes making up songs is greater than the number of songs. This is just the reverse of the productivity of human language, in which the number of words is always far less than the number of sentences that can be made from them.

As it happens I share Hurford’s scepticism about the la-la theory. But on the other hand, Steven Mithen, one of the contributors to Tallerman and Gibson (2012), supports it, finding it “astonishing that music has received minimal attention in the literature on the origin of language” (Mithen 2012, 296). Birdsong grammar may be quite unlike human grammar, as Hurford says, but then birds are not close evolutionary cousins of Man. My own sceptical reaction might be based on nothing more substantial than the fact that I am not a very musical person myself. Is it plausible that we could ever find hard evidence one way or the other? For all our modern research findings in various fields, I am not clear that we are entitled to say much more, about the la-la theory or about various alternative ideas on the origin of language, than “it could be right, it could be wrong”.

Julie Andresen is less interested in hard evidence than in analysing the terminology and metaphors which have been fashionable in linguistics at various periods, with the idea that examining these will unblock intellectual dead-ends

that we have become stuck in without realizing it — as she puts it (p.260), she “use[s] historiography as a kind of talk therapy for the discipline”. In principle this could be a worthwhile move. It does require considerable familiarity with changing patterns of discourse in the subject, though. I was surprised, for instance, to find Andresen saying (p.20) that “Philology is more traditionally known as historical or comparative linguistics”: actually, it was the other way round. The latter terms are used nowadays for roughly what was earlier called philology (though the correspondence is not precise — “philology” included literary and cultural connotations which are not present with “historical/comparative linguistics”). As late as 1969, when I became (so I was told) the first person in the more than 800-year history of Oxford University to be appointed to a post with “linguistics” in the job title, some of my senior colleagues were puzzled and asked whether I ought not rather to be called a philologist.

(For a linguist Andresen sometimes seems insensitive to language. To describe people as “in a bit of a conceptual and descriptive pretzel” (p.24), or to say that the “effects of our languaging” are “deeply rhizomed in our synaptic tangles” (p.96), are not the kind of metaphors that help to clarify difficult concepts. Andresen is unaware that the French word *liquide* has an *e* (p.215); she supposes that the singular of (*Homo sapiens*) is “sapien” (p.225).)

One index of the sparsity of solid findings in the area covered by these books is the frequency with which the authors make factual mistakes, perhaps through striving to give their books more substance by going beyond the narrow range of knowledge that really is well established. Hurford’s book is far more reliable than Andresen’s, yet despite the fact that Hurford’s book is essentially a brief digest of his two much longer earlier volumes (which could have made it relatively easy to avoid topics where the facts are uncertain), even Hurford makes a number of statements that seem wrong. On p.77, for instance, he says that “Philip Lieberman has persuasively argued that the ultimate cause of the human lowered larynx is its function in producing different vowels”. It is true that back in 1972, when Lieberman along with many other linguists was dazzled by the novel idea that the detailed structure of language might be genetically preprogrammed, he argued that differences between Neanderthal and modern human larynx positions were to be explained in terms of evolutionary pressure to produce distinct vowel sounds (Lieberman 1972). But Hurford’s wording implies that Lieberman still thinks this and that it remains plausible, yet the idea has been so heavily criticized that I wonder whether anyone still believes it. (See e.g. John Ohala (1994, 338) on the fact that only men, not women, have this distinctive larynx position.) Lieberman himself has comprehensively recanted his earlier belief in language as a driver of human genetic evolution (Lieberman 2000). Or, consider the passage on Hurford’s p.147 where he correctly points out that nineteenth-century linguists

believed in a “Golden Age of language structure, exemplified by the Classical languages” from which modern European languages with their impoverished inflexion systems have decayed, and claims that those linguists gave no explanation for this change of direction, first towards synthetic structure and then away — making them sound irrational. In fact, philosophically-minded linguists such as August Schleicher explained it in terms of Hegel’s philosophy of history, which was enormously influential in German intellectual life of the period: the change of direction was seen as an intellectual analogue of climbing a ladder and then letting it fall away once the high place was safely reached. (Cf. Hegel 1837, 62–3.) Hurford and I would doubtless agree in regarding this explanation as mistaken, but that is very different from saying that the nineteenth-century linguists had no explanation.

On p. 148 Hurford says that the particular case of inflexional simplification between Old and Middle English was a consequence of contact with Norman French. Did the average inhabitant of England after 1066 have enough exposure to French-speakers to make that plausible? The only explanation of this sort that I have seen argued at length attributed the development to contact not with French but, some centuries earlier, with Danish (Poussa 1982). While England was ruled by Danes these two languages were at least closely-enough mingled for core vocabulary, including pronouns, to have been borrowed from Danish into English (though as I understand it Patricia Poussa no longer maintains this hypothesis).

Hurford suggests on p. 140 that when sentence grammar first arose it was always of the SOV type, arguing that plenty of attested languages show that word-order, and languages sometimes change from SOV to another order, but no language ever changes from something else to SOV. I am no expert on word-order evolution but I find this claim surprising, at least, in the light of Chinese. Classical Chinese commonly placed direct objects after the verb, as English does, but in modern Mandarin a form *bǎ* (which originally mean something like “grasp”) has been grammaticalized into an object-marking prefix allowing direct object phrases to precede the verb. This looks to me like a move, admittedly incomplete, from SVO towards SOV.

However, these are peripheral issues and, if they are mistakes, they are on a smaller scale than Andresen’s. She tells us, for instance (p. 45) — referring specifically to sciences such as geology and botany which deal with mind-independent phenomena — that “Scientists as scientists are not interested in conserving or refuting a principle such as objective truth. They can and do proceed in investigating whatever phenomena they are investigating without having any particular interest in whether or not these phenomena are objectively real ...” Leaving aside the question whether it is meaningful to call objective truth a “principle” which might be “conserved” (confirmed?) or refuted, as a generalization about science this is breathtakingly wrongheaded. Famously, Ernst Mach before the First World War

believed that “atoms” were no more than fictions which facilitated calculations about observable realities, but this has never been a common scientific stance. Most scientists, I feel sure, would see little purpose in their work if they did not believe they were helping to uncover hidden but objective realities. Again, on p. 168 Andresen asserts that not just most, or the most important aspects, but *all* of what is “characteristically human about humans” is the result of our “linguaging behaviors”. What, even our near-hairlessness, commonly (e.g. by Aristotle) taken to be one of the distinguishing features of our species?

Perhaps a more serious error in the context of her book, Andresen (p. 152) believes that because there are far more human beings alive now than there were in the dawn of language, there must be “more languages and cultures today than there were 100,000+ years ago”. The figure seems to be Andresen’s guess for the time when language first emerged, and if a transition from a languageless to a language-using society was a datable kind of event then presumably there must have been a time when some community first made that transition, after which for a short while only one language existed. But in context Andresen seems to mean that after *all* human societies had made the transition, the total number of different languages would still have been small. That is very questionable. There were fewer individuals then than now, but it is generally agreed that speech-community sizes must also have been far lower than the modern average; Johanna Nichols (2012, 563) suggests a maximum of 500 speakers of a single language. We will probably never be in a position to plot the changing early ratios of total human population to mean language size, but I believe most experts picture the history of human language in terms of great early diversity making way eventually for the rapidly diminishing stock of human languages we find today — the opposite of Andresen’s assumption.

Andresen introduces a discussion of the Sapir–Whorf hypothesis (p. 199) by telling us that Eskimos have a hundred words for snow. Evidently she has missed Geoffrey Pullum’s famous demolition of that urban myth. And on p. 213 not only does she treat Whorf as authoritative on the lack by Hopi Indians of a concept of time — she has missed Ekkehart Malotki’s (1983) analysis of Hopi time ideas (based on better data than were available to Whorf) which makes those ideas much more “normal” by European standards than Whorf supposed — but she believes she can improve on Whorf’s exposition of his own findings:

The Hopi language has no word that would be the equivalent of the SAE concept *time*. If there were to be a way to describe how things proceed or come into being, my reading of Whorf’s description suggests an activity something like the following: Imagine a field of newly planted corn. Go to the heart of each corn seed/budding stalk. Fix your imagining around a vertical axis from the center of the earth to the skies. Put your own heart and mind and everyone else’s heart and mind and



hope and striving into the coming-into-being of the corn. Run the whole through a very fast time-elapsd camera that implies a kind of horizontal axis. Now, take away the horizontal axis. Then take away the vertical axis. You are in the realm of the expective ...

Can any meaning at all be extracted from stuff like this? It reads like the prose of an imaginative but daffy undergraduate, rather than serious scholarship.

For anyone who would like to check how much and what we know these days about the origin of language, despite scattered errors Hurford's survey can be thoroughly recommended. It certainly told me interesting and relevant things I did not know before, such as the finding (p.124) that speakers of a non-European language may make some of the same distinctions as Europeans among facial expressions correlated with different emotions, despite having no words in their language for those emotions.

Nevertheless, neither Hurford nor Andresen persuades me that the anthropologist Bernard Campbell is mistaken to say "We simply do not know, and never will, how or when language began" (Campbell et al. 2005, quoted in Nordquist 2014). The linguists of Paris probably got it right.

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