# Not the Only Word

**Kenneally, Christine**. 2007. *The First Word: The Search for the Origins of Language*. New York: Penguin Books.

By Lyle Jenkins

Christine Kenneally's *The First Word* is a review of work in the area of language evolution intended as an introductory overview for the general reader and, as such, is a valuable resource for pointers to work in progress on a wide range of evolutionary topics; these include primate calls, birdsong, categorical perception, gene research, computer simulation studies, to name a few. This is the subject of the second and third parts of the book. These parts contain the most valuable information on language evolution research. However, the first part of the book is devoted to interviews with Noam Chomsky, Sue Savage–Rumbaugh, Stephen Pinker and Paul Bloom, and Philip Lieberman. Here the stage is set for a kind of 'linguistics wars' on language evolution, with Chomsky on one side of a 'debate' and just about everybody else on the other side. This is the least convincing part of the book, as we will see.

According to Kenneally, the study of the evolution of language can be divided into several phases — one starting in 1866, when "the Société de Linguistique of Paris declared a moratorium on the topic" (p.7), and another phase when "the official ban developed fairly seamlessly into a virtual ban" (p.79) which was maintained, it is claimed, until the publication of a paper by Pinker and Bloom around 1990 (but see below). The "virtual ban" on the study of evolution of language seems to be ascribed by Kenneally almost solely to Chomsky. Unfortunately, the historical record, which Kenneally examines somewhat superficially, does not bear out her claim of any "ban", virtual or otherwise.

Kenneally reports that the "academic censorship" lasted for more than a century and language evolution remained a "disreputable pursuit", until several conferences on the origins of language were organized in the 1970s. She neglects to mention that at the earliest conferences on the biological foundations of language during this period, Chomsky was a key participant and speaker, apparently in violation of his own ban. These include the interdisciplinary meeting on language and biology at Dedham, Massachusetts, in 1974, sponsored by the Royaumont Center for a Science of Man, as well as a conference on *Ontogenetic and Phylogenetic Models of Cognitive Development* in Paris in 1975, both organized by Massimo Piattelli–Palmarini (see also Boeckx & Grohmann 2007, in their editorial to the first issue of *Biolinguistics*).


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Moreover, Kenneally doesn't mention that at one of the conferences she lists, *Origins and Evolution of Language and Speech*, organized by the New York Academy of Sciences in 1976, one of the participants and speakers was Noam Chomsky. Far from being regarded as someone out to squelch discussion of language evolution, one of the conference organizers, Stevan Harnad, noted in his introductory remarks that "the revolution of linguistics due to Noam Chomsky has provided a very different idea of what the nature of the 'target' for the evolutionary process might actually be". That is to say, to attain a deep understanding of language evolution, one must first understand what the 'target' of evolution is (the quotes indicate that 'target' is not to be understood in a teleological sense). This was one of the reasons that Eric Lenneberg (1967) chose to include an appendix on generative grammar by Chomsky in his classic *Biological Foundations of Language* as early as 1967.

Or, as Chomsky framed it at the Lenneberg symposium at around the same time — and on later occasions — the study of biolinguistics was concerned with standard questions about mechanisms, development, and evolution that any biologist would ask about a biological system:

(1) What is knowledge of language?
(2) How does language develop in the child?
(3) How does language evolve in the species?

Although these three questions can be studied in any order, or even in parallel, the better answers we have to question (1), i.e. have an understanding of the mechanisms and structure of the language faculty, the better we can answer question (2), how these mechanisms and structure unfold in the child — and, as recent work on evolutionary developmental biology (evo-devo) has shown, the better we understand the answers to questions (1) and (2), the better equipped we are to study what Harnad above called the "target" of language evolution. For example, recursion is one the important syntactic mechanisms available to the language faculty. This insight from theoretical linguistics in turn opened the cross-species investigation of recursion in other species (as, for example, in tamarins and starlings).

As another example, at a conference on *Maturational Factors in Cognitive Development and the Biology of Language* held in 1978, there is a discussion between Chomsky and the neurologist Norman Geschwind about a number of questions concerning evolution of language; among others, about the cerebral asymmetries in the great apes, auditory tasks in the left temporal lobe of the monkey, asymmetry for emotional behavior in the brain, the recognition of species-specific cries in the left hemisphere of Japanese monkeys, male-female differences in left-right asymmetry for areas involved in bird song, and so on. (For the full exchange, see Jenkins 2000.) This interchange provides a flavor of the discussions taking place among linguists and neurologists at the time. In any case, this does not seem to be the behavior of someone out to pose a "virtual ban" on the topic of evolution.

Kenneally tends to sprinkle her factual reporting with unsupported

generalizations: "For example, scientists assumed for a long time that the parts of the brain that have to do with language must be wholly new, recently evolved additions that we do not share with nonhumans". In asserting this, she totally overlooks a whole generation of work on brain and language; one looks in vain for any mention of Geschwind and Galaburda's work on cerebral dominance and asymmetry (e.g. Geschwind & Galaburda 1986) or LeMay and Geschwind's work on the morphological asymmetries of the brains and skulls of nonhuman primates (e.g. LeMay & Geschwind 1975), to mention only a few examples. Nor was work on evolution of lateralization limited to the language areas; consider, for example, the work by Denenberg and colleagues on functional asymmetries in the rat (e.g. Denenberg 1981) as well as the numerous asymmetries in lower organisms documented by Corballis and Morgan (e.g. Corballis & Morgan 1978; Morgan & Corballis 1978).

In 1998, when Gannon and colleagues reported their findings of an asymmetry in the left planum temporale area of chimpanzee brains (Gannon *et al*. 1998), this was heralded in the press as "challenging cherished notions of how language evolved in humans and why apes cannot talk" (*New York Times*). However, left-right asymmetries in non-human primates, including the planum temporale area, had been long known (see e.g. Cunningham 1892, Fischer 1921, Yeni–Komshian & Benson 1976). The German magazine *Der Spiegel* claimed that until the study of Gannon *et al*., it had been thought that the left and right sides of the brains of non-human primates were absolutely equal, although this had been shown twenty years earlier not to be the case by the study of Beheim–Schwarzbach (1975), who had compared the temporal regions in humans, chimpanzee, and the orangutan.

Moreover, in 1979, the Linguistics Society of America held its first Summer Institute abroad at the Joint Linguistic Society of America and University of Salzburg Summer Linguistics Institute at the University of Salzburg, Austria with the theme of *Linguistics and Biology*, which included courses, seminars, and other presentations as well as discussions on linguistics and biology of language, including neurology and the evolution of language. Although many scholars from Europe and around the world attended this Linguistics Institute, no mention was made of the "virtual ban" that had supposedly been imposed by Chomsky. Also around this time there were many fruitful contacts between the ethologist and evolutionary biologist Konrad Lorenz and his colleagues in Austria and at the Max Planck Institute in Germany, and generative linguists at the universities of Vienna and Salzburg. In 1976, Lorenz and his colleagues participated in a symposium on language and biology at the Salzburg Summer School of Linguistics (apparently in violation of the "ban").

Much is made by Kenneally of a paper by Pinker and Bloom, in which the central thesis is: "In one sense our goal is terribly boring […]. All we argue is that […] the only way to explain the origins of such abilities [such as language, vision, etc. — LJ] is through the theory of natural selection" (Pinker & Bloom 1990). However, the idea that natural selection and adaptation play a role in language evolution was hardly controversial long before the Pinker and Bloom paper. In fact, the application of these standard biological ideas may be seen in the popular writings of the biologists and Nobel Laureates Monod, Jacob, and Luria in the

early 1970s in their discussion of the ideas of Chomskyan generative grammar. In fact, Kenneally notes: "He [Chomsky — LJ] reiterated that there were factors in evolution other than natural selection, which were as likely to be significant. And in this regard, Chomsky, Pinker and Bloom were essentially in agreement, their debate arising more from differing emphases than actual discord". So the reader is left scratching his or her head as to what the fuss is all about, and why people make such fantastical claims, such as that Chomsky believes that language is not a product of evolution (Plotkin 1998) or that he is trying to enforce a ban on language evolution studies.

Kenneally (p. 200) states that after the discovery of the language effects of FOXP2, "the possibilities (were hailed) for a new science" and that it was called "neurogenetics" by Vargha–Khadem and her colleagues (who worked on the phenotype of the FOXP2 system; cf. Vargha–Khadem 1995). However, the field of neurogenetics has been around a lot longer than since 2001, and its scope is considerably broader than language, referring to the genetics of the nervous system in general. For example, nearly 30 years ago, Xandra Breakefield edited a volume on the subject. This collection also included an article by Kenneth K. Kidd & Mary Ann Records entitled 'Genetic methodologies for the study of speech' (Kidd & Records 1979). Although the term was even in use much earlier than this, this shows that the genetics of language was understood to be a subfield of the much broader field of neurogenetics. In addition, the *Journal of Neurogenetics* launched its first issue in 1983.

Kenneally notes that "it's been pointed out that the rules of phonology contradict Chomsky's notion of the poverty of stimulus — the idea that there is not enough information in the language a child hears for it to learn language" (p. 155). However, the work cited only argues that certain data from phonology don't require appeal to poverty of stimulus. Moreover, poverty of stimulus doesn't require that all language data be part of man's genetic endowment. Clearly, then you would not be able to explain how an infant born in a Japanese-speaking environment,but moved at birth to an English-speaking environment comes to learn English. It has always been understood that language learning requires a complex interplay between internal and environmental factors. So, for example, to argue that poverty of stimulus is superfluous, you would have to show that the phenomena explained by it; e.g., structure-dependence of syntactic rules such as question inversion in English can be derived from the data available to the child. There have been serious efforts to do this, e.g. Pullum & Scholz (2002). However, Legate & Yang (2002) examined the child language corpus used and showed that this particular attempt failed. Even if the attempt had succeeded, one would still have to go on to provide an alternative explanation for all of the numerous phenomena that have been discovered over the years and explained by poverty of stimulus (island conditions and others) to rule it out. In fact, however, researchers in biolinguistics now accept that there is a genetic endowment underlying the language faculty. Most research has long since shifted to other questions such as what is genetically specified and what environmental input is required, and whether this genetic endowment is in part or in whole domain-specific or not, or even species-specific or not.

In fact, an alternative picture to Kenneally's 'linguistics wars' view of the

study of language evolution is that biolinguistics has moved gradually through several phases in recent decades, corresponding to the questions posed earlier about (1) knowledge of language, (2) acquisition of language, and (3) evolution of language. The first phase was primarily concerned with the construction of generative grammars that represented the knowledge of language (language faculty). The second phase extended the findings across many languages and attempted to account for both the universal properties of language as well as their variation (e.g., in the Principles–and–Parameters model). The third phase, building on the results of the first two phases, is increasingly concerned with questions of function, design, and evolution (e.g., the Minimalist Program).

Kenneally cites the famous maxim of Dobzhansky (1964) that "nothing in biology makes sense except in the light of evolution". Modern work on evolution ('evo-devo') has turned that maxim on its ear, as Pennisi (2002) has noted, so that one might say that higher stages of evolution make no sense except in light of (developmental) biology. Accordingly, if we want to understand the evolution of language, we must also understand the (developmental) mechanisms underlying one of evolution's magnificent achievements, the human language faculty.

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