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The CRADLE  
of LANGUAGE

EDITED BY

RUDOLF BOTHA  
AND CHRIS KNIGHT

STUDIES IN THE EVOLUTION OF LANGUAGE

# The Cradle of Language

# STUDIES IN THE EVOLUTION OF LANGUAGE

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# The Cradle of Language

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Edited by  
Rudolf Botha  
Chris Knight

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## Preface and acknowledgements

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Together with its companion volume—*The Prehistory of Language*—this book grew out of a conference held in Stellenbosch, South Africa, in November 2006. The organizers deliberately held the event in the part of the world where modern language is now believed to have evolved. In addition to prominent linguists, psychologists, cognitive scientists, and specialists in artificial intelligence, the conference featured some of the world's leading archeologists, historical linguists, primatologists, and social anthropologists, in many cases bringing specialist knowledge of distinctively African data and perspectives.

Shortly after the conference, we decided to publish not only the contributions from invited speakers but papers selected from the refreshingly wide range of disciplines represented at the event. Chapters dealing more generally with the origins and evolution of language appear in *The Prehistory of Language*. The present volume focuses more specifically on the origins of language in Africa. Both reflect the authors' extensive additional work on their original papers.

The Cradle of Language Conference was organized by Rudolf Botha. It was sponsored by the University of Stellenbosch and the Netherlands Institute for Advanced Study in the Humanities and Social Sciences. We gratefully acknowledge generous financial support from The University of Stellenbosch, The Ernest Oppenheimer Memorial Trust and South Africa's National Research Foundation; we also warmly thank Connie Park for her dedicated work in compiling, reformatting, and editing the manuscripts.

Chris Knight, London  
Rudolf Botha, Stellenbosch

April 2008



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# List of abbreviations

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BBC	Blombos Cave
BCT	Basic Color Term
CV	consonant–vowel
FCC	Female Cosmetic Coalitions
FLB	the language faculty broadly construed
FLN	the language faculty narrowly construed
MRCA	most recent common ancestor
MSA	Middle Stone Age
mtDNA	mitochondrial DNA
SNP (“snip”)	a single nucleotide polymorphism (i.e. a change in one letter of the genetic code)
SVO	subject–verb–object
WALS	<i>The World Atlas of Language Structures</i> edited by Martin Haspelmath, Matthew Dryer, David Gil, and Bernard Comrie (OUP 2005)

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---

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# 1 Introduction: perspectives on the evolution of language in Africa

---

CHRIS KNIGHT

## 1.1 A human revolution?

Africa was the cradle of language, mind, and culture. Until recently, the evidence for this remained little known. The prevailing “human revolution” theory saw modern language and cognition emerging suddenly and nearly simultaneously throughout the Old World some 40 to 50 thousand years ago. This “Great Leap Forward” for humanity (Diamond 1992) was depicted as a cognitive transition based on a neural mutation yielding syntax and hence true language (Klein 1995, 2000; Tattersall 1995). When modern *Homo sapiens* evolved in Africa some 200,000–150,000 years ago, according to this theory, our ancestors were modern only “anatomically”; mentally and behaviorally, they remained archaic. Only when such humans began migrating out of Africa—triggering the Middle-to-Upper Paleolithic transition in Europe—did the “leap” to cognitive and behavioral modernity occur.

Over the past decade, it has become apparent that this notion was an artifact resulting from a Eurocentric sampling of the fossil and archaeological records (Mellars et al. 2007). Recent studies by archeologists working in Africa have shown that almost all the cultural innovations dated to 50,000–30,000 years ago in Europe can be found at much earlier dates at one or another site in Africa. Blade and microlithic technology, bone tools, logistic hunting of large game animals, long-distance exchange networks—these and other signs of modern cognition and behavior do not appear suddenly in one package as predicted by the Upper Paleolithic human revolution theory. They are found at African sites widely separated in space and time, indicating not a single leap but a much more complex,

uneven but broadly cumulative process of biological, cultural, and historical change (McBrearty and Brooks 2000; McBrearty 2007).

This book addresses the fossil, genetic, and archeological evidence for the emergence of language. It also critically examines the theoretical tools available to interpret this evidence. The three opening chapters focus on personal ornamentation, whose emergence in the archeological record has been widely interpreted as evidence for symbolic behavior. Occupying pride of place are the now celebrated engraved pieces of ochre (Henshilwood et al. 2002) and marine pierced shells (Henshilwood et al. 2004; d’Errico et al. 2005) recovered from Middle Stone Age levels at Blombos Cave, South Africa, and dated to around 70,000 years ago. Shell beads in a similar cultural context have recently been found at the other end of Africa—in eastern Morocco—dating to 82,000 years ago (Bouzouggar et al. 2007). Mounting evidence for key elements of modern behavior at still earlier dates includes a South African coastal site (Pinnacle Point) yielding mollusc remains, bladelets, and red ochre pigments dating to at least 164,000 years ago (Marean et al. 2007). Use of ochre pigments extends back between 250–300 ky at some sites in the tropics; regular and habitual use dates back to the time of modern speciation (Watts 1999, this volume). These and other archeological discoveries offer compelling evidence that key elements of symbolic culture were being assembled and combined in Africa tens of millennia before being exported to the rest of the world.

Although it remains in circulation, the idea that complex language was triggered by a single mutation some 50,000 years ago (e.g. Klein 1995, 2000) is no longer widely held. This volume explains why. The book as a whole focuses on Africa, most contributors arguing on diverse archeological, genetic, and other grounds that complex language probably began evolving with the speciation of modern *Homo sapiens* around 250,000 years ago. There is increasing evidence that similar developments must have been occurring among Europe’s Neanderthals, although in their case leading to a different historical outcome (Chapters 2, 7, and 8). No contributor to this volume still defends the notion of a mutation for syntax triggering language a mere 50,000 years ago. The archeologist Paul Mellars (a prominent speaker at our conference although not a contributor here) is widely credited as principal author of the “human revolution” theory in its original form. He now readily accepts that if we

can speak of a “human revolution” at all, it must have occurred in Africa—and much earlier than previously supposed (Mellars 2007).

## 1.2 Interdisciplinary perspectives

Although no single volume can represent all current perspectives, the following 14 chapters bring together many of the most significant recent findings and theoretical developments in modern human origins research. The disciplines represented include historical linguistics, paleolithic archeology, paleogenetics, comparative biology, behavioral ecology, and paleoanthropology, to name but a few.

Adopting a broad comparative perspective, Francesco d’Errico and Marian Vanhaeren (Chapter 2) use evidence of prehistoric bead working to argue for a distinctively archeological approach to the problem of modern human origins. Too often, they write, archeologists have undermined their own discipline by seeking to explain their data on the basis of theories and assumptions developed by specialists working in other areas. Among other negative consequences, this has led to claims about Neanderthal inarticulateness or stupidity for which no evidence exists.

Until recently, the invention of bead working was considered to be contemporaneous with the colonization of Europe by anatomically modern *Homo sapiens* some 40,000 years ago. We know now that marine shells were used as beads in the Near East, North Africa, and Sub-Saharan Africa at least 30,000 years before that. Five sites—Skhul and Qafzeh in Israel, Oued Djebbana in Algeria, Grotte des Pigeons in Morocco, and Blombos Cave in South Africa—have yielded evidence for an ancient use of personal ornaments. There is then a surprisingly long find gap: no convincing ornaments reliably dated to between c. 70 and 40 thousand years ago are known from either Africa or Eurasia. Then, at around 40 thousand years ago, ornaments reappear almost simultaneously in Africa, the Near East, Europe, and Australia.

This evidence is difficult to reconcile with either the classic “Human Revolution” model or its “Out of Africa” rival. On the one hand, personal ornaments clearly predate the arrival of modern humans in Europe. On the other, no continuity is observed in bead-working traditions after their first documented occurrence in Africa. This suggests to the authors that while possession of modern capacities may *enable* the use of beads, they

certainly don't mandate it. The evidence also contradicts the view that after their invention, these decorative traditions everywhere became more complex: They did not. The production and use of a varied repertoire of personal ornaments by late Neanderthals contradicts both models since it demonstrates that this alleged hallmark of modernity was by no means confined to anatomically modern *Homo sapiens*.

D'Errico and Vanhaeren argue that the cognitive prerequisites of modern human behavior must have been in place prior to the emergence of either late Neanderthal or fully modern human populations. Instead of attributing bead-working traditions to mutations responsible for advances in innate capacity, they invoke historical contingencies triggered by climatic and demographic factors. Such factors, they argue, can explain why bead-working traditions emerged, disappeared, and re-emerged in the archeological record at different times and in different places. This forms part of a more general plea by the two authors to stop making inferences on the basis of unsupported assumptions about inter-species differences in cognitive capacity. Put the archeological evidence first!

Chris Henshilwood and Benoît Dubreuil (Chapter 3) focus upon the spectacular engraved ochre pieces and shell ornaments discovered by Henshilwood and his team at Blombos Cave and dated to at least 70,000 years ago. What do these discoveries mean? It is now widely accepted that the inhabitants of the cave probably painted their bodies with red ochre and adorned themselves with shell beads. If they wore the beads while their bodies were simultaneously decorated with pigment, some of the red pigment might have attached itself to the beads—a pattern for which some archeological evidence exists (Plate 3e).

At the Cradle of Language Conference, Henshilwood and his colleagues argued that the shell beads at Blombos possessed “symbolic meanings” so complex as to require “fully syntactical language” for their cultural dissemination and transmission. This particular way of inferring language from the archeological evidence was not universally accepted (see Chapter 5 for a sustained critique), and in the present volume a subtly different argument is proposed.

At a minimum, write Henshilwood and Dubreuil, we can infer that the inhabitants of Blombos Cave must have been attentive to how others saw and understood them. Taking this argument a stage further, the use of cosmetics and ornaments surely “suggests that one person can understand *how* she looks from the point of view of another person.” The ability to see

oneself from the standpoint of others—“to represent how an object appears to another person”—is not a development continuous with primate self-centered cognition. Citing Michael Tomasello among others (Tomasello et al. 2003; Warneken and Tomasello 2006), the authors view it as a qualitatively new development, unique to humans and lying at the root of all linguistic comprehension and production. For one person to wear beads *with a view to others’ appreciation of them* is not necessarily to take the further step of actually talking about them. But in cognitive terms, the principle is already there. The wearer is forming not just a *representation* of her beads but a *meta-representation*. To construct representations of representations in this way—switching between alternative perspectives instead of remaining imprisoned in one’s own—is to discover the creative potential of recursion as a cognitive principle. Syntactical recursion, write Henshilwood and Dubreuil, is essential to the linguistic articulation of meta-representations of this kind. If this argument is accepted, the authors conclude, we are justified in inferring complex linguistic capacity from the evidence for personal ornamentation found at Blombos Cave.

Chapter 4 takes us from the beads at Blombos to the ochre—a topic discussed also in the final two chapters of the book. Ian Watts is the ochre specialist at Blombos Cave; he also deserves recognition as the first archeologist to insist in print that “the human symbolic revolution” occurred in Africa during the Middle Stone Age, not Europe during the Upper Paleolithic. That early publication (Knight, Power, and Watts 1995) proposed what has since become known as the Female Cosmetic Coalitions (FCC) model of the origins of symbolic culture. Watts and his colleagues at the time took a number of risks—predicting in advance, for example, that the earliest evidence for symbolism anywhere in the world should take the form of a “cosmetics industry” focused on “blood reds” (see Power, Chapter 15).

Since Camilla Power first advanced this theoretical argument (Knight, Power, and Watts 1995; Power and Aiello 1997), it has become accepted that this particular prediction of the model has been borne out. The world’s earliest known mining industries were aimed at producing cosmetics; the colors consistently favored were evidently the most brilliant “blood” reds (Watts 1999, 2002; Henshilwood et al. 2001a). The possibility remains, however, that this had nothing to do with the concept of “blood” as a symbol of “fertility” in hunter-gatherer initiation rituals, as stipulated



by FCC. An alternative theory exists. Modern humans might have selected red simply because our species has an innate bias in favor of this color.

Watts (Chapter 4) forces these rival models into conflict with one another, testing between their divergent predictions. The most sophisticated version of the innatist paradigm is the theory of Basic Color Terms (BCT) in its various incarnations since first publication in the late 1960s (Berlin and Kay 1969). Watts shows how—in the face of recalcitrant empirical data—this body of theory has undergone so many revisions and qualifications as to have little in common with its original formulation. It is difficult to test a theory whose predictions are repeatedly manipulated to fit the facts. By contrast, FCC has not had to be altered since its original formulation. Its seemingly risky predictions have been borne out by the archeological data, the ethnographic and rock art data, and—if Watts’ arguments in this chapter are accepted—by what is currently known about the evolution of basic color terms.

Chapter 5 takes a critical look at such theories and claims. In a contribution cited by several of our authors, Rudolf Botha discusses the bridge theories needed if archeologists are to infer details of language evolution from findings such as those made at Blombos Cave. Beads are not linguistic phenomena. On the basis of what body of theory, then, might archeologists (e.g. Henshilwood et al. 2004) connect them with language? Why does the wearing of pierced shells suggest one level of syntactic complexity as opposed to another? The question is important because if no such theory exists, the whole chain of inferences from beads to language is indefensible. At the Cradle of Language Conference, Henshilwood and his colleagues argued that the Blombos shells had symbolic meanings requiring “fully syntactical” language for their articulation and transmission. But how might we test between this theory and its possible alternatives? Might not the inhabitants of Blombos Cave have worn ornaments simply for decoration, without having to talk about their symbolic meanings? Even if the “meanings” of the shells did require verbal transmission—an unsupported assumption—why did the requisite language have to be “fully syntactical”? Why couldn’t it have taken some simpler form?

Botha’s critique is not directed narrowly at the work of Henshilwood and his team. The problem is a much wider one. Scholarly failure to resolve problems too often reflects the theoretical disarray still characterizing much of our field. Botha notes, for example, that in interpreting his

Blombos findings, Henshilwood relies on Thomas Wynn's (1991) characterization of language as "complex behavior." Wynn cites Chomsky (1980) as his authority in this respect. But this is a puzzling citation. "One of Chomsky's most fundamental claims," Botha reminds us, "is that language is not a form of behavior." For many professional linguists, the entity known as "language" is a mental phenomenon, not a feature of bodily behavior. The misunderstanding is important because the notion of "modern behavior" plays so prominent a role in modern human origins research. If paleoanthropologists and linguists debate on the basis of incommensurable assumptions—one camp defining language as "behavior" while the other defines it as "mind"—we can hope for little progress.

In Chapter 6, Tecumseh Fitch turns to a general discussion of the connection between speech abilities and the hominin fossil record. Speech, he writes, presupposes among other things the ability to make rapid changes in formant frequencies. Why don't other mammals display comparable abilities? If the impediments were anatomical, we might hope to use comparative methods to determine the vocal capacities of diverse fossil specimens including hominins. But however surprising it may seem, anatomy turns out to be scarcely relevant.

Many mammals are quite capable of opening or closing the jaw during the course of a call. Changes in lip configuration are not uncommon. From an anatomical perspective, then, many animals should possess the ability to rapidly manipulate formant frequencies. In big cats, the entire tongue/hyoid apparatus descends along with the larynx, giving them a vocal anatomy corresponding quite closely to that of humans. So why don't these animals vocalize in more speech-like ways?

Demolishing numerous paleoanthropological myths, Fitch concludes that peripheral anatomy is largely irrelevant. If lions don't talk, it's not because they suffer from physical impediments. It's because they lack the necessary neural controls. The crucial changes required to enable rapid manipulation of formant frequencies must have been neural, not anatomical. With the possible exception of work connecting fine breath control to enlargement of the thoracic canal (MacLarnon and Hewitt 1999, 2004), neural changes are unlikely to leave any fossil signature. Fossils, therefore, can tell us little about the timing of the evolution of speech as a transmission mechanism for language.

Chapter 7 turns to the genetic capacity for language and the light shed by genes on language evolution. We now know (Krause et al. 2007) that

the Neanderthals shared with modern humans the mutations in the FOXP2 gene claimed by some to have triggered the emergence of language in *Homo sapiens* some 50,000 years ago. The genetic evidence purporting to confirm this date comes from an article by Enard et al. (2002), “Molecular evolution of FOXP2, a gene involved in speech and language.” In a devastating critique of this paper among others, Karl Diller and Rebecca Cann show the extent to which the genetic facts have been manipulated to fit the theoretical claim. The actual date calculated by Enard et al. as most likely for the human mutations in FOXP2 is not 50,000 years ago but 0 (zero) years ago with a 95% confidence interval stretching back to 120,000 years ago. “If the date of zero years ago doesn’t raise some eyebrows,” observe Diller and Cann, “then the suggestion that the date of zero supports any date we choose between now and 200,000 years ago should. It is clear that we need to look at the fine print.”

The Andaman Islanders in the Indian Ocean have been genetically isolated for at least 65,000 years—and no one doubts that these humans have full capacity for language. Any mutations important for modern language, conclude Diller and Cann, are likely to have spread through the population in Africa before *Homo sapiens* began colonizing the rest of the world. In fact, there are no good grounds for believing the widely publicized claims regarding the specifically linguistic relevance of FOXP2 in much of the recent literature on language evolution. Associated with orofacial control rather than syntax or grammar, the gene’s specifically human mutations most probably occurred some 1.8 million years ago—around the time when *Homo habilis* and *Homo ergaster* were making their appearance in the fossil record. Surveying the genetic data in the context of mounting evidence from paleoanthropology and archeology, the authors conclude that the capacity for language is likely to have been fully developed in the first anatomically modern humans by around 200,000 years ago.

Darwinians do not see genetic mutations as events capable of causing long-term evolutionary change. Within a given population, behavior adapts to changing circumstances initially on the basis of *existing* genetic capacity, novel behavioral strategies then shaping the future trajectory of genetic evolution on the basis of natural selection. This approach—known nowadays as “behavioral ecology”—is especially well illustrated in Chapter 8. Wil Roebroeks and Alexander Verpoorte ask why modern humans rapidly succeeded in colonizing the globe while their Neanderthal

counterparts became extinct. Did this difference stem from deficiencies in Neanderthal cognition or communication?

Challenging the methodological assumptions underlying this idea, Roebroeks and Verpoorte argue that the most influential current approaches to the Neanderthal question need to be more than modified—to put it bluntly, they need to be reversed. Too often, archeologists set out with an abstract concept labeled “language” which they then use as a tool to explain changes in the archeological record. Such interpretations are typically framed in “cognitive” terms, as when the Neanderthals are said to have been more “cognitively challenged” (i.e. stupid) than modern humans. One persistent narrative holds that the Neanderthals lacked “fully modern” linguistic skills and consequently became extinct. In fact, there is no evidence for any of this.

To explain the striking differences between the Middle Paleolithic (Neanderthal) and Upper Paleolithic (modern human) archeological records, Roebroeks and colleagues point out that the two species had very different energetic requirements. Unlike the smaller and more gracile immigrants from Africa, the Neanderthals had big bodies requiring for their upkeep large amounts of energy. One consequence was that their travel costs had to be kept down, constraining foraging ranges and forcing them to move camp as adjacent resources were eaten out. Why invest energy in a structured hearth or dwelling if it is likely to be abandoned in a few days? The decision of a Neanderthal group to move on rather than invest continuously in one camp had nothing to do with innate cognitive deficits. On the contrary, the strategy was optimal under the circumstances. One advantage of this kind of reasoning is that it allows us to explain why fully modern hunter-gatherers in many regions—Tasmania, for example—produced archeological signatures not unlike those left in Eurasia by the Neanderthals. It is not that these people lacked “modern” language or cognition. If they didn’t invest heavily in hearths, dwellings, or representational art, the most likely explanation is that the costs of such behavior would have outweighed any possible benefits.

According to James Hurford and Dan Dediu (Chapter 9), students of human evolution have too often been victims of their own scientific abstractions. The authors cite, for example, a recent monograph claiming (on the basis of mitochondrial DNA evidence) that “there was a first human” and that “this human was a woman.” It’s one thing to deploy metaphors for purposes of communication—“Mother Tongue,”

“Mitochondrial Eve,” “Language Faculty,” and so forth. But it’s quite another to reify metaphors to the point where they begin to take over. Entities such as “languages,” “the human genome,” and “the human language capacity” are not unitary phenomena open to scientific study. They are abstractions of our own making. If science is to proceed, we must unpack them so that the complexities they hide are exposed. In real life, there can have been no “First Human,” no single “Cradle of Language,” no “Mother Tongue,” and no moment at which “the Language Faculty” emerged.

In this critical spirit, Hurford and Dediu draw on recent paleogenetic findings to cast doubt on the “Out of Africa/Rapid Displacement” model of the origins of modern humans. They concede that Africa was one important “Cradle”—but there were others, too. Echoing many other chapters in this volume, they are persuasive in insisting that there cannot have been a single genetic mutation—whether in Africa or elsewhere—that gave rise to “language, modernity and everything else.” The authors cite recent research (Dediu and Ladd 2007) pointing to population-level variability in the capacity to process linguistic tone: in this case, at least, the relevant genetic mutations seem to them to have occurred quite recently outside Africa. This illustrates the fact that the language faculty is a complex mosaic of features of different ages and origins, by no means necessarily African. Language capabilities are not and never have been uniform across the species. Not all humans today have an equal aptitude for learning a second language—some of us are much better at this task than others. But then, variation of this kind is to be expected—without it, natural selection couldn’t work.

The next two chapters adopt perspectives from historical linguistics. Both caution against simplistic attempts to reconstruct distant historical events from the current distribution of features among African languages. In an innovative study, Cysouw and Comrie (Chapter 10) ask a question not previously asked. To what extent can we pick up signals of prehistoric events by studying the current distribution of typological diversity across African languages? They conclude that such signals can be discerned, although at present they tell only of relatively recent events such as the Bantu expansion. The authors’ statistical methods remain experimental, they concede, and cannot be used to reconstruct distant events such as at the level of large-scale language families.

In similarly cautious spirit, Sands and Güldemann (Chapter 11) focus on the click languages of Africa. These languages are often portrayed as ancient,

clicks being represented as probable relics of an ancestral African mother tongue. This view, observe the authors, rests on two unproven assumptions: first, that clicks originated just once in an ancestral population, and, second, that the click languages of Africa are for some reason peculiarly conservative. The authors counter with convincing synchronic and diachronic linguistic evidence that clicks and click languages are not frozen relics but have been evolving in Africa in the relatively recent past. The authors do not exclude the possibility that clicks were a feature of the earliest African mother tongue, but do insist that the current distribution of clicks cannot be invoked as evidence.

The next two chapters take us from the study of clicks to the study of African hunter-gatherer cultures more generally. Language evolved at a time when all humans lived by hunting and gathering. It therefore seems important that our debates should be informed by an understanding of the adaptive pressures inseparable from this lifestyle.

No one doubts that extant hunter-gatherers are as modern in their cognition and behavior as anyone else. But to a greater extent than farming or modern industry, according to Alan Barnard (Chapter 12), the productive activities of extant hunters and gatherers “allow us a model through which to speculate about the distant past.” In relation to any given individual, everyone in a hunter-gatherer society is classified as some kind of “kin.” The corresponding logic of kinship operates on principles not wholly unlike that of language. Like a language, a kinship system is a complex structure that contributes to social cohesion.

Barnard envisages an evolutionary sequence in which the “protokinship” of early *Homo* gives way to “rudimentary kinship” in *Homo heidelbergensis* followed by “true kinship” in modern *Homo sapiens*. These stages reflect “three biologically induced human social revolutions,” each with its own consequences for the evolution of language. First came the “signifying” or “sharing revolution,” corresponding to the production of the first stone tools. Then came the “syntactic revolution,” a shift corresponding to the earliest systems of generalized reciprocity between neighboring groups of kin. Finally, Barnard turns to the “symbolic revolution” responsible for culture, kinship, and language as we know it. When true kinship emerged—that is, when relationships became governed via categories, rules, and a corresponding kinship “grammar”—the scene was set for an explosion of grammar in language as well.

Jerome Lewis (Chapter 13) takes hunter-gatherer ethnography in a different direction. Instead of offering a speculative scenario, he sets out

to ground our debates about language evolution in the day-to-day realities of hunter-gatherer life. Drawing on his own fieldwork, he urges that scientists attempting to clarify what it means to be human might learn important lessons from experts such as the Mbendjele “forest people” of Congo-Brazzaville.

While hunting, Mbendjele men listen attentively to the “sound signatures” of the forest, systematically “faking” natural sounds in order to lure their prey within range of their weapons. Did selection for such deceptive abilities play a role in the evolution of speech? Lewis notes that the non-human victims of Mbendjele vocal deception cannot fight back. Instead of developing strategies of resistance—as humans would be expected to do—they fall victim to the same trick again and again. If Lewis is right, “talking to animals” offers a novel possible explanation for our own species’ unusual ability to rapidly manipulate formant frequencies (cf. Fitch, Chapter 6).

When humans are the target audience, trickery of this kind cannot become evolutionarily stable. The difficulty is that humans quickly learn to recognize such sounds as fakes, subsequently resisting or ignoring them. As a result, successful deception is frequency-dependent, the dishonest strategy remaining parasitic on its default counterpart in honest communication. Only when deployed against other species—animals whose vocal signals simply cannot be fakes—can trickery of this kind prove stable as an evolutionary strategy.

Lewis notes that in the Mbendjele case, evolved capacities for faking animal cries have become central also to communication between humans. But in this case, needless to say, trusting listeners are not deceived. Not for a moment does anyone imagine that a Mbendjele hunter faking a crocodile’s mating call is really a crocodile. Instead, transparent fakes are valued for distinctively human reasons rooted in uniquely human levels of trust, cooperation and good-humoured play (cf. Knight 2000 and Chapter 15). Forming a core component of narrative skill, the ability to “fake” sound signatures becomes skilfully redeployed as the Mbendjele act out stories about themselves and their neighbors, rapidly switching between formant frequencies, speech styles, dialects, and tongues.

Hunter-gatherer language, concludes Lewis, is not to be confused with the literate language more familiar to western academics. It is not a bounded system but “an open, expansive communicative tool that imitates any other languages or meaningful sounds and actions that enable

Mbendjele to interact with agents with whom they wish to maintain social relations.” These agents include other Mbendjele, villager neighbors, crocodiles, duikers, monkeys, and potentially all other inhabitants of the forest. Laughter, mobbing, dance, and melodic word-play are not separate communicative systems. Instead, they comprise so many facets of one and the same “culture of communication,” testifying to the distinctively human ability to “make play” with meaningful sounds. Lewis suggests that the innate capacities enabling such cultural life must have evolved under specific selection pressures associated with play, laughter, gender solidarity, menstrual and hunting ritual, animal mimicry, and so forth. In short, language co-evolved with the establishment of a symbolically structured sexual division of labor. On that basis, he concurs with those scholars who argue for language’s emergence in Africa from around 200,000 years ago, in a Darwinian process driven by selection pressures for “hunting, mimicry, faking, and play.”

Camilla Power (Chapter 14) reminds us that every adaptation has costs as well as benefits. Her analysis sets out from the standard Darwinian premise that each sex pursues differential strategies of investment in offspring, giving rise to conflict both within and between the sexes. In the case of evolving human females, the heavy costs of producing increasingly encephalized and dependent offspring would be expected to outweigh any benefits—unless male energies could be tapped into and exploited in novel ways. Symbolic ritual, she argues, emerged out of the consequent strategies of sexual selection—“reverse sexual selection” in that males would invest preferentially in females advertising quality through ritual display.

Power sets up this model in opposition to the standard one assumed by Darwin, in which males compete while females choose those best at “showing off” their quality through sexual display. The two models make quite different predictions. Males advertising quality through costly display is a familiar pattern in the animal world. Although this model predicts elaborate and costly sexual signals, it does not and cannot predict symbolism, which entails reliance on patent fakes. The alternative is a model in which females form “cosmetic coalitions” in order to exploit male muscle-power. This predicts not only symbolism in general, but initiation ritual generating cosmetic representations in quite specific forms. Only a model that makes fine-grained predictions can be tested in the light of empirical data. Power’s Female Cosmetic Coalitions (FCC) model meets this criterion, generating predictions that can be tested



against data from the archeological, fossil, and ethnographic records. The main problem for the model is to differentiate between Neanderthal and modern human strategies; the chapter concludes with a brief discussion of one possible solution.

Our final contribution (Knight, Chapter 15) is one of many in the present volume to focus less on language per se than on the subsistence, reproductive, and alliance-forming strategies in the context of which it may have evolved. To insist on addressing the “big picture” of modern human origins research, however, is not to abandon the specific problem of the emergence of language. It is to claim instead that there are no easy solutions, no short-cuts. In the final analysis, nothing short of “a theory of everything” will do.

Knight’s specific target is an idea made popular by evolutionary psychologist Steven Pinker, according to whom language is in key respects a digital computational system. From the digital nature of language, Pinker (1999) concludes that humans—unlike other primates—must have “digital minds.” An alternative possibility, in Knight’s view, is that we inhabit a digital world.

The domain of institutional facts—facts dependent on collective belief—is just such a world. Take Barnard’s discussion of the logic of modern hunter-gatherer kinship (Chapter 12). In prohibiting certain categories of behavior while permitting others, a system of this kind must exclude intermediate states. With respect to a given man, for example, no woman can relate to him as “more or less” a sister or “something between” a sister and a wife. On logical grounds, exclusion of intermediate states must apply not only to kinship terms—but to all signs whose agreed meanings are institutional facts. The notion of analog processing of facts of this kind is inconceivable. This is not because the human brain (or some component of it) is a digital computer but simply because the notion is a contradiction in terms. Digital computation as a core feature of language cannot evolve in nature. It can evolve only as an internal feature of human symbolic culture—that is, of cognitive and communicative life in an institutionally structured world.

### 1.3 African origins

As always in an interdisciplinary collaboration of this kind, the chapters surveyed here diverge widely in their methods, approaches, and

interpretations. Yet in their different ways, they reflect the emergence of a growing consensus. Not everyone believes in “the human revolution.” Among those who do, however, there is a growing consensus that it cannot have been triggered by a single mutation. According to Mellars (2007), the revolution which made us human is best conceptualized as “a process of accelerated change” on the model of, say, the Neolithic or industrial revolutions of more recent human history. In any event, long before anatomically modern *Homo sapiens* left Africa, our ancestors would appear to have been cognitively modern in every important sense.

## 2 Earliest personal ornaments and their significance for the origin of language debate

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FRANCESCO D'ERRICO AND MARIAN VANHAEREN

### 2.1 Introduction: language origins and archeology

When did humans acquire the characteristics we normally associate with “humanness”: language, use of symbols, art, religious thought? These behaviors leave little or no trace on human remains and it is the archeologist’s job to identify and date the signs of their emergence in our ancestors’ material culture.

Traditionally, the emergence of these innovations has been considered to be the result of a sudden change, taking place in Europe 40 ky ago and coinciding with the arrival in this region of Anatomically Modern Humans (Mellars and Stringer 1989; Stringer and Gamble 1993; Mellars 1996; Mithen 1996; Bar-Yosef 1998, 2002; Conard and Bolus 2003; see Klein 1999, 2000 for a slightly different scenario). This model, known as the Human Revolution scenario, has been gradually replaced in the last decade by a new paradigm, called the Out of Africa scenario (McBrearty and Brooks 2000).

This new scenario tends to equate the biological origin of our species with the origin of modern cognition. It can be summarized as follows.

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Present-day variation in mitochondrial DNA and Y chromosome suggests our species comes from Africa (Cavalli-Sforza et al. 1994; Barbujani 2003; Templeton 1993; Ingman et al. 2000; Forster 2004). The process that produced our species in Africa must have granted it a number of advantages—syntactical language, advanced cognition, symbolic thinking—that favored its spread throughout the world, determined its eventual evolutionary success, and led to the extinction of pre-modern human populations with little or no biological contribution and, if any, little and unbalanced cultural interaction.

Underlying the Out of Africa model for the origin of modern behavior is the view, well exemplified by the famous McBrearty and Brooks graph (2000: 530), that the emergence of each of these new features marked a definite and settled threshold in the history of mankind and that the accumulation of these innovations contributed, as with genetic mutations, to create human societies increasingly different from those of their non-modern contemporary counterparts. Archeologists who adopt this position try to identify and document in the African Middle Stone Age the emergence of cultural innovations that can be interpreted as the behavioral outcome of this speciation.

In doing so, however, they face two problems. First, postulating that these advantages were determined by a biological change logically leads to the somewhat paradoxical conclusion that archeology does not inform us as to the origin of modern behavior and language. Populations will be considered smart, eloquent and symbolic according to their taxonomic status and not on the basis of the material culture they have left behind. A recent paper (Anikovich et al. 2007) is paradigmatic of this attitude. Excavations conducted at Kostenki 14, on the west bank of the Don River, have revealed an archeological assemblage dated to 41 ky BP that includes two undiagnostic human teeth, bone and ivory artifacts, and a shell bead. In spite of the absence of diagnostic human remains and the fact that archeological layers attributed to the Aurignacian, a cultural proxy for the presence of modern humans, only occur at the site much later (c. 33,000 BP), the authors conclude that the 41 ky BP assemblage reflects a colonization of the East European Plain by modern humans several thousand years before their arrival in western Europe. The logic behind this interpretation is that if the material culture is modern, its makers must also have been biologically modern even if there is no evidence supporting that. In order to promote this view, the authors bar from consideration

the fact that levels of cultural complexity similar to those found at Kostenki 14 are recorded at contemporary Neanderthal sites (d'Errico et al. 1998, 2004; d'Errico and Vanhaeren 2007; Zilhão 2001) and that, as a consequence, a Neanderthal authorship of their assemblage represents a viable alternative hypothesis.

It has been argued (d'Errico 2003; d'Errico and Vanhaeren 2007; Villa et al. 2005) that to avoid this pitfall, archeologists should adopt a large-scale comparative approach. Documenting and dating the occurrence of these innovations in various regions of the world including Eurasia, the alleged realm of pre-modern populations, may reveal their presence at times and places incompatible with the Out of Africa model. It may also show a discontinuous pattern with innovations appearing and disappearing or being associated in a way that does not match the expected trend. The aim of the archeology of language and modern cognition should be that of documenting the complex historical processes at work in and out of Africa and using the resulting chronicle to identify long-term trends that can be contrasted to those offered by other disciplines. This is particularly so considering that scenarios proposed by other disciplines such as paleoanthropology, genetics, and linguistics are not straightforward either and models accepted today as established facts may be challenged in a short while by new discoveries.

The very basis of the Out of Africa scenario—the possibility of reconstructing ancient migrations from present-day genetic diversity—has recently been challenged (Templeton 2002, 2005; Garrigan et al. 2005b; Thomas et al. 2005; Eswaran et al. 2005; Rogers et al. 2007). The mtDNA sequences obtained thus far from a dozen Neanderthal specimens seem to lie outside the range of variation of modern Europeans and the few Upper Paleolithic sequences but this does not exclude the possibility of gene flow from modern humans into Neanderthals or a genetic Neanderthal input into the gene pool of early modern colonisers, later eliminated by bottleneck and replacement events. Human remains such as those from Lagar Velho, Mladeč, Oase, and Les Rois have been interpreted as bearing Neanderthal inherited features (Wolpoff 1999; Trinkaus and Zilhão 2002; Trinkaus et al. 2003; Trinkaus 2005) but these interpretations have been challenged by authors who consider that features interpreted as evidence of admixture are plesiomorphic (Tattersal and Schwartz 1999).

Linguists such as Chomsky (1965, 1975) have long considered that language was a biologically innate ability and have been reluctant to

address the question of the origin of language in evolutionary terms. They now call for interdisciplinary cooperation to address this issue (Hauser et al. 2002). Reading their contribution, however, makes it clear that paleoanthropology and archeology are virtually excluded from this invitation. Does this mean that this sudden interest in the origin of their object of study is not associated with a concern for when, where, and among which human population or populations language emerged? One must keep in mind that the empirical facts that endorse the “Cradle of human language” owe little to linguistics but rather come from genetics, archeology and paleoanthropology.

Hauser et al. (2002) propose that what distinguishes human language from other forms of animal communication is recursion, meaning the capacity to generate an infinite range of expressions from a finite set of elements, i.e. the ability to make complex sentences. But others strongly disagree, observing that this minimalist approach underestimates the complex multifaceted nature of human language (Pinker and Jackendoff 2005). The safest attitude a discipline such as archeology can take in such a context is to elaborate scenarios that can be empirically tested and thereby improve its ability to constructively interact with other disciplines.

This takes us to the second problem. In spite of valuable efforts, archeologists have failed to develop theories on the cognitive and linguistic implications concerning the remains that they uncover and interpret as enduring evidence for the origin of major human behavioral shifts. We need informed theoretical frameworks with which to make explicit the possible links between the archeological record and the language abilities of past human populations (see Botha this volume).

In this chapter, we summarize the earliest archeological evidence for personal ornamentation in Africa and Eurasia, and discuss its significance with respect to the origin of language debate.

## 2.2 Archeological context and dating

Research conducted in the last three or four years has dramatically changed our view of the origin of bead manufacture and use. Until recently, the invention of personal ornaments was considered to be contemporaneous with the colonization of Europe by anatomically modern populations bearing the Aurignacian technology, some 36,000 years ago

(White 2001; Taborin 1993; Klein 2000). We know now that marine shells were used as beads in the Near East, North Africa, and Sub-Saharan Africa at least 30 ky earlier. Five sites—Skhul and Qafzeh in Israel, Oued Djebbana in Algeria, Grotte des Pigeons in Morocco, and Blombos Cave in South Africa—have yielded evidence for an ancient use of personal ornaments.

### 2.2.1 *Skhul*

The shelter of Es Skhul is located at Mount Carmel, 3 km south of Haifa, in the canyon of Nahal Me'arot (Wadi el-Mughara), some 3.5 km away from the Mediterranean shore (Garrod and Bate 1937). Excavations by McCown in 1931 and 1932 identified three main layers (McCown and Keith 1939): Layer A (20 to 50 cm thick) contained a mixture of Natufian, Aurignacian, and Mousterian stone tools; Layer B (about 200 cm thick and bearing all the human remains) contained Mousterian stone tools; and Layer C (shallow sandy deposits at the base of the sedimentary sequence) yielded only a sparse lithic industry and no faunal remains. Layer B was subdivided into two subunits mainly distinguished by their hardness. The upper hard earth unit B1 resembled plaster of Paris, whereas the lower breccia B2 was similar to concrete. The lithics of Skhul Layer B were attributed to the Levantine Mousterian and have been compared with those of Tabun C and Qafzeh (Garrod and Bate 1937; Bar-Yosef and Meignen 1992), while the macro-faunal remains in Layer B appeared to correspond with those of Tabun C to D (Garrod and Bate 1937). Nine intentionally buried individuals (Skhul I–IX) attributed to modern humans were recovered from Layer B. Skhul V revealed a large boar mandible in its arms which was interpreted as a grave good. Dating studies yielded closed system ESR ages on faunal teeth in the range of about 55 to 100 ky (Stringer et al. 1989) and 46 to 88 ky (McDermott et al. 1993), U-series ages on faunal teeth in the range of 43 to 80 ky (McDermott et al. 1993), and TL ages on burnt flint in the range of about 99 to 134 ky (Mercier et al. 1993). New ESR and U-series analyses indicate the best estimates lie between 100 and 135 ky BP (Grün et al. 2005). Garrod and Bate reported the presence of four marine shell species (*Acanthocardia deshayesii*, *Laevicardium crassum*, *Nassarius gibbosulus*, *Pecten jacobaeus*), identified by Connely and Tomlin, without indicating the number of specimens recovered or their stratigraphic provenance (Garrod and Bate 1937: 224). The marine shells from Skhul were recently located at the Department of

Palaeontology, Natural History Museum (NHM), London, and analyzed by a multidisciplinary team (Vanhaeren et al. 2006). The Skhul material includes two perforated *Nassarius gibbosulus*, a valve of *Acanthocardia deshayesii*, a fragment of *Laevicardium crassum*, a fragment of an undetermined shell, and a fragment of a Cypraid. The *Pecten jacobaeus* mentioned by Garrod and Bate is missing. Only the *Nassarius gibbosulus* shells (Figure 2.1) bear perforations that could have been used for suspension in a beadwork. In order to identify the layer from which these *Nassarius* originated, sediment matrix adhering to one of them and sediment samples from layers A, B1, and B2 were analyzed for mineralogy and chemical composition. Major and trace elements, as well as the

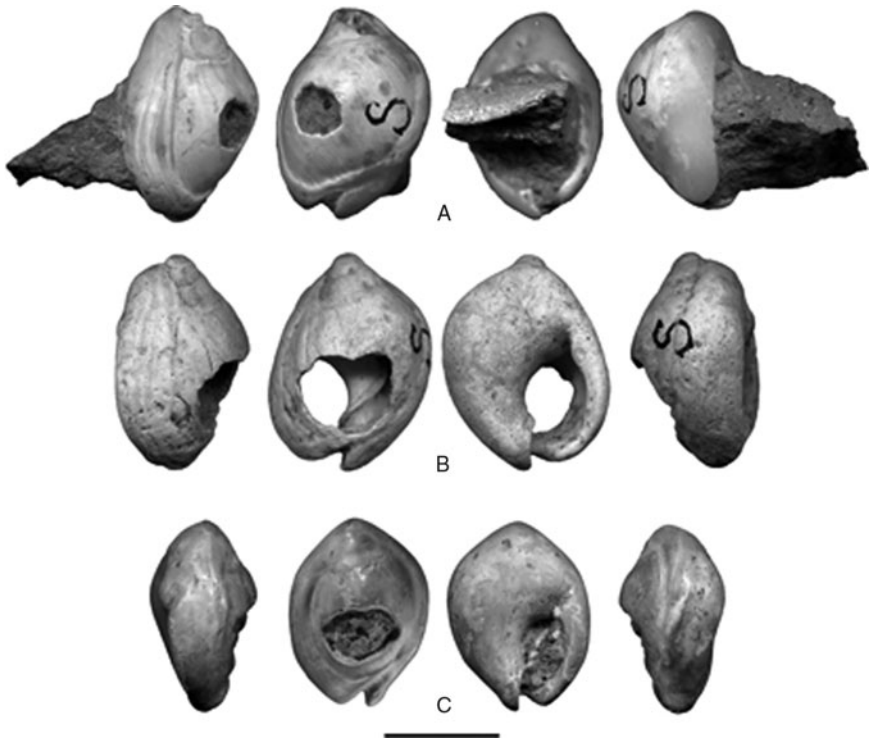


FIG. 2.1 *Nassarius gibbosulus* shell beads from the Mousterian levels of Es-Skhul (A and B) and the Aterian levels of Oued Djebbana (C). Scale = 1 cm (modified after Vanhaeren et al. 2006, photo by the authors).



hardness of the sediment adherent to the pierced shell, indicate that it comes from Layer B1-2.

### 2.2.2 Qafzeh

Located on the southeast side of the Qafzeh Mountain, near Nazareth, 30 km from the sea, this large cave was excavated in 1933–5 by Neuville and Stekelis, and between 1965 and 1979 by Vandermeersch (1981). This last excavation identified in front of the cave a stratigraphy composed of 24 (I–XXIV) layers and inside the cave one of 14 (1–14) layers.

The lower archeological layers in the former (XXIV–XVII), attributed to the Mousterian, yielded skeletal remains of fourteen individuals, six adults and eight children, bearing modern anatomical features (Tillier 1999; Vandermeersch 1981, 2006).

At least three of these individuals were intentionally buried (Hovers and Belfer-Cohen 1992; Arensburg et al. 1990; Vandermeersch 1981). Thermoluminescence (TL) and electron spin resonance (ESR) age estimates for these layers suggest occupations at 100,000–90,000 years ago, with an average TL date of  $92,000 \pm 5,000$  years ago (Valladas et al. 1988). Seventy-one ochre pieces, some of which present clear anthropogenic modifications, come from layers XVII–XXIV (Hovers et al. 2003). Taborin (2003) and Hovers et al. (2003) report four complete *Glycymeris* sp. shells with a perforation on the umbo (Figure 2.2) and a fragment of bivalve belonging to the same species from layers XXIV (n=1), XXII (n=2), and XXI (n=3).

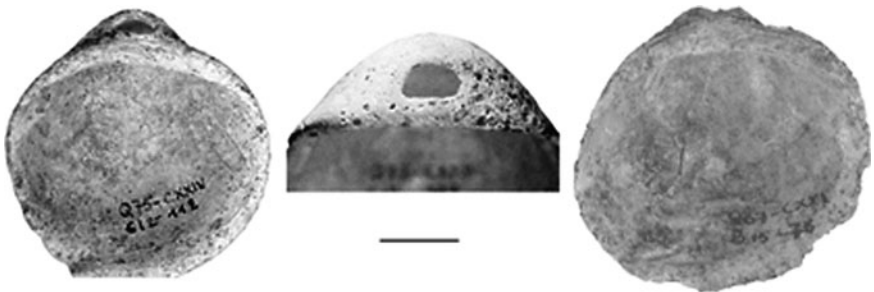


FIG. 2.2 *Glycymeris* sp. shells with a perforation on the umbo from Mousterian levels XXI and XXIV of Qafzeh, scale = 1 cm (modified after Taborin 2003).

### 2.2.3 *Grotte des Pigeons*

Grotte des Pigeons is a large cave situated in eastern Morocco near the village of Taforalt, which lies approximately 40 km from the Mediterranean coast. The cave, discovered in 1908, was the subject of excavations in 1944–7, 1950–5, and 1969–76 by Roche (1976), and in 1977 by Raynal (1980). They identified a 10 m thick archeological sequence containing Iberomaurusian (Upper Paleolithic) and Aterian (Middle Paleolithic) artifacts. The burials of 180 individuals were found in two areas within the Iberomaurusian layers. Excavations conducted since 2003 by Barton and Bouzouggar identified a 2.5 m thick stratigraphic sequence with five principal units (A, B+C, D, E, and F), each of which is bracketed by a significant shift in sediment type (Bouzouggar et al. 2007). Middle Paleolithic occupation horizons have been recorded in units C–F. Unit E is characterized by Middle Paleolithic tools such as side scrapers and small radial Levallois cores, and a few thin, bifacially worked foliate points. Thirteen *Nassarius gibbosulus* shells (Plate 1) have been recovered from this unit, the majority (n=11) coming from contiguous squares covering an area of 6 m<sup>2</sup>. Seven shells come from a lightly cemented 12 cm thick ashy lens with abundant evidence of human presence including archeological finds and hearth debris. The presence of two beads in the overlying unit is attributed, in the light of the site formation process, to reworking due to human activity. The four remaining shells were found in the fill of burrows which intersects the ashy lenses, the layers from which they probably derive.

Radiocarbon accelerator mass spectrometry determinations on charcoal provided dates ranging between 11 ky and 23 ky BP for the upper part of the archeological sequence (A–C). The lower layers, including the shell-bearing unit E, were dated by OSL using the multiple and single grain methods, TL determinations were obtained on burnt flint artifacts, and Uranium-series isotopic measurements were performed on flowstone samples. A Bayesian age model based on the obtained age estimates constrains the horizon containing the pierced *Nassarius* shells to between 73,400 and 91,500 years ago with a most likely date of 82,500 years BP.

### 2.2.4 *Oued Djebbana*

Oued Djebbana is an open-air site, located at Bir-el-Ater, 97 km south of Tebassa, Algeria, 200 km from the Mediterranean Sea (Morel 1974a). The

site contained a 1 m thick archeological layer situated under 3.9 m of sterile alluvial deposits. This layer yielded a lithic assemblage that associates typical Aterian pedunculated tools with other Middle Paleolithic tool types produced in some cases with the Levallois technique, and few Upper Paleolithic tool types. The central area of the site, rich in ash, contained one perforated *Nassarius gibbosulus*, which is curated at the Musée de l'Homme, Paris (Figure 2.1). A single infinite conventional radiocarbon date of 35,000 BP (MC 657) is available for this site (Morel 1974b).

### 2.2.5 *Blombos Cave*

Blombos Cave is situated 20 km west of Still Bay in the southern Cape and some 100 m from the coast. The site is currently excavated by Henshilwood, who has been investigating this cave deposit since 1992. Excavations have identified a stratigraphic sequence with, from the top to the bottom, 80 cm of LSA deposit, an undisturbed 10–50 cm sterile aeolian dune sand, and three main MSA units (M1, M2, and M3) (Henshilwood et al. 2001a, b; Jacobs et al. 2003; Jacobs 2004). Principal markers of the M1 phase are bifacial foliate points, typical of the Still Bay technocomplex. Two slabs of ochre engraved with geometric patterns and more than 15 bone tools come from the M1 phase (Henshilwood et al. 2001b, 2002). The M2 phase markers include fewer Still Bay points and an increased frequency of bone-tool use. In the M3 phase, bifacial flaking is absent and there are fewer retouched tools than in M1. Ochre is particularly abundant in the M3 levels. The LSA layers have been radiocarbon dated to 2 ky BP. Multiple and single grain OSL and TL methods have provided occupation dates for each phase: c. 70 ky for the sand layer lying on top of the MSA layers, c. 75–77 ky for the M1, c. 82 ky for the M2 phase, and c. 125 ky or earlier for the M3 phase (Jacobs et al. 2003a, b, 2006; Jacobs 2004; Tribolo et al. 2006). Thirty-nine beads manufactured from *Nassarius kraussianus* gastropod shells (Figure 2.3) come from the upper MSA phase, M1, and two derive from the M2 phase (Henshilwood et al. 2004; d'Errico et al. 2005). The two shell beads found in M2 may be intrusive due to slumping of the deposits in the recovery area and probably originated from the overlying M1 phase. Thirty-three beads were found in six groups of two to twelve beads, each group being recovered in a single square or in two adjacent subsquares.

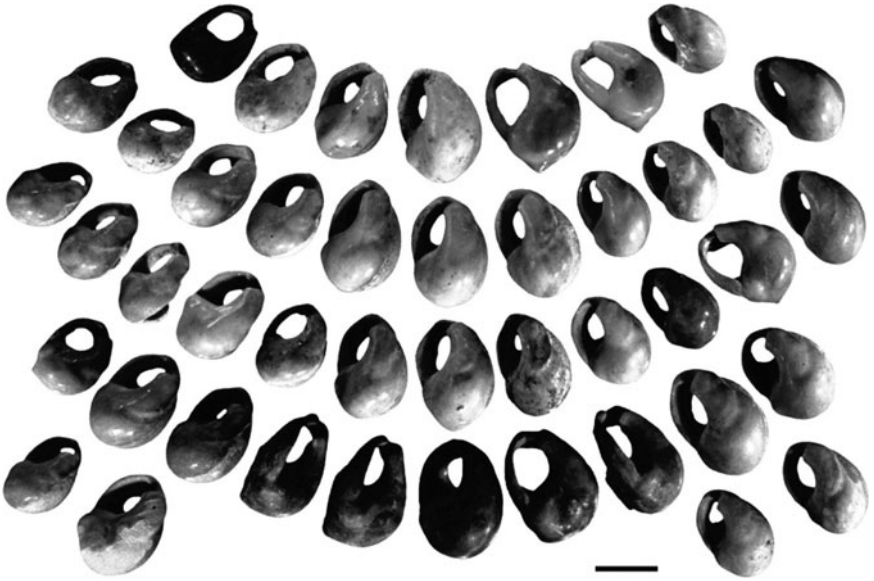


FIG. 2.3 *Nassarius kraussianus* shell beads from the M1 phase of Blombos Cave, Western Cape Province, South Africa. Scale bar = 1 cm (modified after Henshilwood et al. 2004, photo by the authors).

### 2.3 The earliest bead evidence

Non-human taphonomic processes are known to produce pseudo personal ornaments that can mimic humanly modified and used beads. To determine whether purported ancient beads were used as such requires evidence for human involvement in their selection, transport, manufacture, and use. In addition, it is crucial to eliminate the possibility of contamination from younger layers dated to periods during which shell beads are known to occur.

As far as the *Nassarius gibbosulus* specimens are concerned, their presence at Skhul, Grotte des Pigeons, and Oued Djebbana cannot be explained by natural causes and must be attributed to human behavior (Vanhaeren et al. 2006; Bouzouggar et al. 2007). This species is absent in the geological formations in or close to where these three sites are situated. During the accumulation of layers B1-2 (100–135 ky), distance

from the sea of the Skhul site varied between 3 and 20 km (Siddall et al. 2003); that of Grotte des Pigeons between 73,400 and 91,500 years ago was c. 40 km. Oued Djebbana was never, during the whole Upper Pleistocene, closer than 190 km to the sea shore. The altitude above sea of Skhul (45–150 m between 100 and 135 ky), the good state of preservation of the archeological shells, their low frequency, and the reduced species spectrum excludes storms as a transporting agent (Claassen 1998). This species has many predators but none of them is known to transport these shells into caves or such a distance inland. They cannot be interpreted as the remains of human subsistence practices, since 100 specimens only provide 4.84 g of dry soft tissue and require 30 min. to extract. In the case of the Grotte des Pigeons, the shells show features characteristic of dead shells accumulated on a shore. These include encrustations produced by bryozoa, tiny shells and sea-worn gravel embedded into the body whorl, and perforations produced by a predator on the ventral side of the shell (Figure 2.4).

The *N. gibbosulus* from these three sites do not represent a random sample from a natural living or dead population. With the exception of two unperforated specimens from Grotte des Pigeons, all shells show a unique perforation located in the center of the dorsal side, a combination of features only observed in 3.5% of modern collections of dead shells; the probability of randomly collecting a sample of shells like those from these three sites is extremely low ( $P < 0.0001$ ). This suggests that the shells with a perforation on the dorsal side were either deliberately collected or perforated by humans. Although the latter seems more probable, the agent responsible for the perforations cannot be firmly identified.

Perforation edges on the dorsal aspect are rounded and smoothed on four shells. The remainders have irregular outlines with chipping of the inner layer indicating that the agent responsible for the perforation punched the shells from the exterior surface of the dorsal side. Holes with irregular edges may be produced by punching the dorsal side with a lithic point. Smoothed perforation edges have been replicated by wearing strung modern shells. Both types of hole edges occur on shells used as beads in Upper Paleolithic sites. However, they are equally common on naturally perforated shells from a shoreline context. The exclusive collection of naturally perforated shells, however, is contradicted at Taforalt by the presence of two unperforated shells in the excavated assemblage. The aperture of these specimens is obstructed by gravel, which might explain why they were never modified. It also suggests that some, if not all,

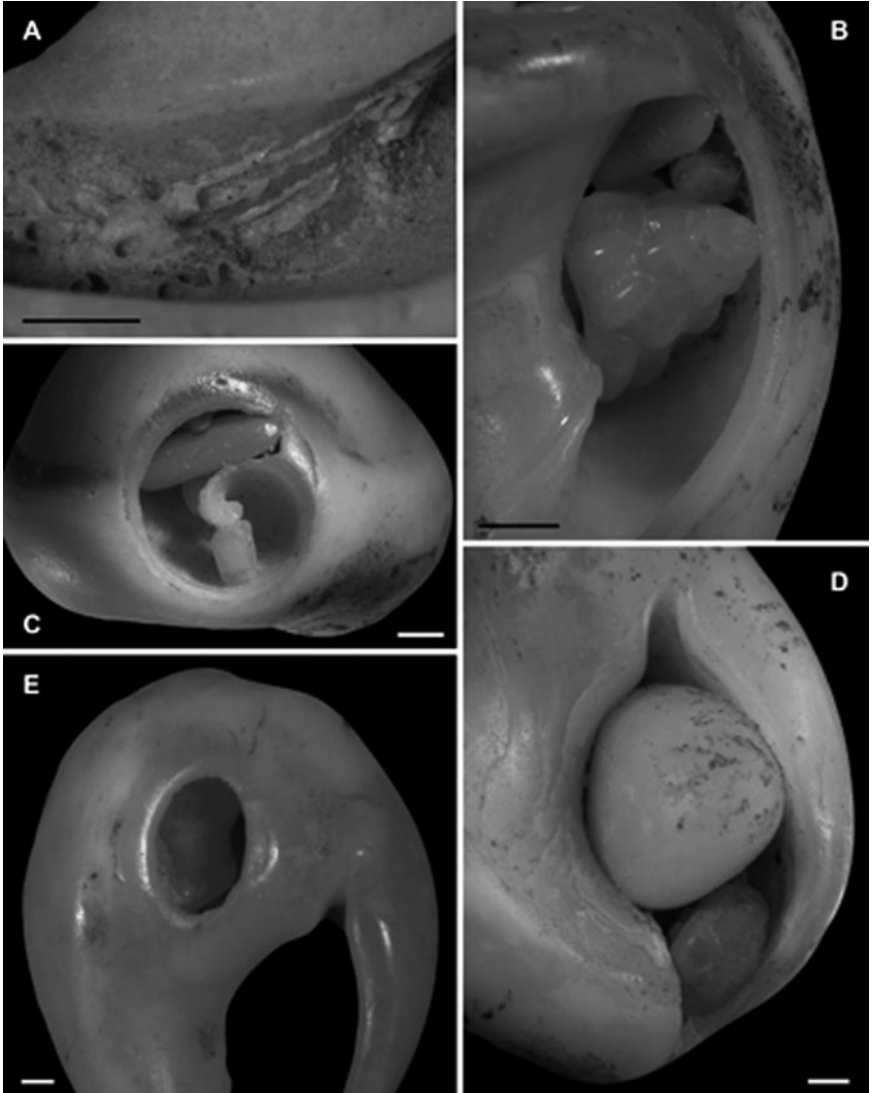


FIG. 2.4 Postmortem modifications on the *Nassarius gibbosulus* shells from the Grotte des Pigeons indicating that they were collected on the shore: (A) encrustation produced by bryozoa; (B) sea gravel, fragment of a bivalve, and a tiny Rissoidae embedded in the shell body whorl; (C) sea gravel stuck in the hole produced by abrasion of the protoconch; (D) sea gravel obstructing the shell aperture; (E) perforation produced by a predator subsequently enlarged by abrasion on the beach (scale bars = 500  $\mu\text{m}$ ). (Modified after Bouzouggar et al. 2007, photo by the authors).

of the shells from Taforalt had no perforations when they were collected and that they were subsequently perforated by humans. In contrast, the presence of sea gravel stuck in the broken apex of three shells indicates that the breakage of the apex, also recorded on three other specimens, was already present when the shells were collected and is not the result of human agency. Possible evidence for the stringing of the perforated shells as beads comes from the identification on ten specimens from Grotte des Pigeons of a wear pattern different from that observed on modern reference collections and unperforated specimens from this site. The wear in the latter case homogeneously affects the whole surface of the shells and consists of a dull smoothing associated with micropits and rare short randomly oriented striations. The wear on the presumed strung examples is found on the perforation edge and on spots of the ventral and lateral side, and it is characterised by an intense shine associated with numerous random or consistently oriented striations. The state of preservation of the Skhul and Oued Djebbana shells is such that no definite conclusion can be reached as to the human origin of the wear.

Microscopic residues of red pigment were detected on one unperforated and nine perforated shells from the Grotte des Pigeons (Plate 2). Elemental and mineralogical analysis of the residue has identified the red pigment as iron oxide with a very high proportion (over 70%) of iron. The most likely explanation for the presence of pigment on the shells is rubbing against ochred material during use.

We can rule out accidental causes because for two specimens colorant is stuck in micro-cracks that cross the worn area, indicating that wear and coloring were intertwined processes. No other objects (e.g. artifacts or bones) from these deposits carry similar pigments, nor are there obvious particles of natural ochre in the site sediments.

The use of shell beads at Qafzeh is less compelling than at the other four sites presented above. The shells were certainly brought to the site, which is approximately 40 km from the sea. Analysis conducted by Walter (2003) has detected the presence of ochre inside one specimen and manganese oxide, probably post-depositional in origin, both inside and outside two other specimens. However, no traces were detected on the perforations indicating that the shells were deliberately perforated, and no study of modern or fossil thanatocoenoses was conducted in order to quantify the occurrence of perforations on the umbo in natural assemblages. Such analyses are needed as we know that natural perforations, produced by a



variety of biotic and abiotic agents, are common on the umbos of bivalves (Claassen 1998). Recent analysis of accumulations of dead *Glycymeris* sp. shells located along the Israeli coast, conducted to study the paleoecology of this species, indicates that 19.7% of the shells are, as those from Qafzeh, unbroken and bear an abraded hole in the umbo (Sivan et al. 2006). This implies (d'Errico and Vanhaeren 2007) that the probability of selecting by chance four such shells in a natural accumulation is low ( $P=0.008$ ) and suggests, as in the case of the *Nassarius* shells from Skhul, that Qafzeh inhabitants either selected naturally perforated *Glycymeris* or deliberately perforated them leaving no obvious manufacturing traces or leaving traces that were subsequently erased by taphonomic processes.

As for the 41 perforated *Nassarius kraussianus* shells from the Still Bay levels at Blombos, morphometric, taphonomic, and microscopic analysis of these shells and of biocenoses and thanatocoenosis of modern shells of the same species indicate that their presence at the site cannot be due to natural processes and that they were selected by humans for their size (Henshilwood et al. 2004; d'Errico et al. 2005). Experimental reproduction indicates that the perforations were made by humans, probably with bone tools. Use-wear recorded on the perforation edge, the outer lip, and the parietal wall of the aperture indicates that the shells were strung and worn. The strings and the beads were deliberately stained with ochre, judging from the remains of red pigments observed microscopically inside a number of shells.

## 2.4 More recent bead evidence

No convincing personal ornaments reliably dated to between c. 70 ky and 40 ky ago are known in Africa and Eurasia. At around 40 ky, this type of material culture reappears almost simultaneously in Africa and the Near East and appears for the first time in Europe and Australia.

### 2.4.1 Africa

Ostrich eggshell beads (OESB) and stone rings are reported (McBrearty and Brooks 2000; Vanhaeren 2005; d'Errico et al. 2005) at nine MSA and Early LSA sites from South (Border Cave, Cave of Earths, Boomplaas,



Bushman Rock Shelter, Zombepata) and east Africa (Enkapune Ya Muto, Mumba, Kisesse II, Loyangalani). Most of these sites, however, are still not securely dated. Many lack radiometric determinations or have been dated long ago with conventional  $^{14}\text{C}$  methods that provide infinite ages or ages close to the limit of the method. Furthermore, when dates obtained with other methods are available they often differ from the  $^{14}\text{C}$  dates and from each other. At present, the most parsimonious interpretation of this evidence is that ostrich eggshell beads were produced in Sub-Saharan Africa, probably by modern humans, since at least 40 ky. Considerable uncertainty remains on the chronological attribution of a burial of a young individual with a perforated *Conus* shell from Border Cave, South Africa, which may be recent in age (Sillen and Morris 1996) or as old as c. 76 ky (Millard 2006).

#### 2.4.2 *Near East*

In the Near East, 43 perforated marine shells, most of which are *Nassarius gibbosulus*, were found at Uçagizli, south of Turkey (Kuhn et al. 2001) in layers dated to  $41,400 \pm 1,100$  BP (AA37625). They are associated with a lithic assemblage attributed to the Ahmarian, an Upper Paleolithic technocomplex present in the eastern Mediterranean that stratigraphically underlies the Aurignacian. In Lebanon, at Ksar' Akil (Mellars and Tixier 1989), 243 shell beads (146 *Nassarius gibbosulus*, 22 *Columbella rustica*, and 26 other marine gastropods, as well as 48 *Glycymeris* sp. and one other marine bivalve) are reported from layers that have yielded lithic assemblages similar to those found at Uçagizli (Kuhn et al. 2001) and are stratigraphically situated between layers dated to  $43,750 \pm 1,500$  BP and 32 ky BP. As in Europe, the authorship of this transitional industry is uncertain. The cast of a lost infant skull from the Ahmarian layers of the Ksar' Akil site bears modern features. The dating and archaeological context of these remains, however, is uncertain (Bergman and Stringer 1989). Five Levantine Aurignacian sites, contemporaneous with those from Europe, have also yielded personal ornaments. These consist of perforated animal teeth—mostly fox, wolf, and red deer canines—and perforated shells. The latter belong to the same species as those used as beads by the Ahmarian inhabitants of the region (Vanhaeren and d'Errico 2006).

### 2.4.3 *Europe*

In Europe, the question of the origin of beadworking traditions is intimately intertwined with that of the tempo and mode of the Middle to Upper Paleolithic transition. This is because beads are associated not only with the Aurignacian—generally considered the product of modern humans—but also with other Early Upper Paleolithic (EUP) cultural traditions of more ambiguous authorship. Widely accepted for the Châtelperronian—the only tradition associated with Neanderthal remains (Bailey and Hublin 2006)—the Neanderthal authorship of the other EUP techno-complexes, even if plausible in view of technological and geographic continuity with preceding local Mousterian industries, is still undemonstrated. It has been proposed that some of them—such as the Bachokirian or the Bohunician—may have been produced by moderns (Otte and Kozłowski 2003; Svendsen and Pavlov 2003; Svoboda et al. 2003). The presence of personal ornaments at Châtelperronian and Uluzzian sites has been interpreted as the consequence of acculturation of local Neanderthals by incoming Aurignacians (Hublin et al. 1996; Mellars 1999, 2004), as reflecting independent cultural evolution of Neanderthals before the spread of the Aurignacian (d’Errico et al. 1998; Zilhão and d’Errico 1999a, b, 2003a, b), or as emerging from cross-cultural fertilization of Châtelperronian/Uluzzian Neanderthals and Aurignacian Moderns (d’Errico et al. 1998; White 2001).

The oldest bead evidence in Europe comes from the 43 ky levels of the Bacho Kiro site, Bulgaria, where a perforated wolf canine and bear incisor were found associated with a lithic industry called Bachokirian and interpreted by the excavators as a possible precursor of the Aurignacian (Kozłowski 1982, 2000). At Kostienki 14 (Markina gora), a Mediterranean shell with two holes has been recovered from a level yielding a Streletskian lithic assemblage and radiocarbon ages ranging between 32.6 and 36.5 ky BP (Sinitsyn 2003; Anikovich et al. 2007). *Dentalium* sp. shells come from the Uluzzian layers of Klisoura cave, Greece (Koumouzelis et al. 2001). The same shell species, as well as *Cyclonassa neritea*, *Columbella rustica*, *Natica* sp., *Trochus* sp., and *Glycymeris* sp. shells, were recovered from the contemporaneous sites of Grotta del Cavallo and Cala, located in southern Italy (Palma di Cesnola 1993; Ronchitelli et al. in press). At the latter site, a fragment of red coral was recovered and the anterior and posterior margins of two perforated *Glycymeris* were purposely modified to create square-shaped beads. The Uluzzian layers of Castelcivita, in southern

Italy, yielded a *Pecten* sp. shell (Palma di Cesnola 1993). In France, a varied collection of perforated or gouged beads is reported from the Châtelperronian layers of Grotte du Renne, in the Yonne region. It is comprised of at least eight fox canines, four bovid incisors, three reindeer incisors, two bear incisors, two marmot incisors, one red deer canine, five bone pendants, three ivory beads, and two fossil belemnites (Leroi-Gourhan and Leroi-Gourhan 1965; d'Errico et al. 1998; White 2001). Perforated wolf, fox, and red deer canines were also found in the Châtelperronian layers of Quinçay Cave (Granger and Lévêque 1997), and a perforated fox canine was recovered from the eponymous site of Châtelperron (White 2001). Bovid incisors and an ivory ring come from the contemporary layers at Roche au Loup (White 2001), a bear incisor and a *Pecten* sp. shell from Trilobite Cave (Taborin 1993), and a *Turritella* sp. shell from Cauna de Belvis Cave (Taborin 1993). *Dentalium* sp. shells were apparently found at Saint-Césaire (Lévêque in d'Errico et al. 1998), and a carnivore canine, identified as a lynx canine, was recovered from Roc de Combe (Sonneville-Bordes 2002).

The variety of personal ornaments, already considerable in the other EUP technocomplexes, becomes even more important during the Aurignacian. A recent study (Vanhaeren and d'Errico 2006) identifies 157 different bead types at 98 sites. Statistical analysis of bead type associations reveals a definite cline sweeping counter-clockwise from the Northern Plains to the Eastern Alps via western and southern Europe through 14 geographically cohesive sets of sites. The sets most distant from each other include Aurignacian sites from the Rhône valley, Italy, Greece, and Austria on the one hand, and sites from northern Europe, on the other. These two groups of sites do not share any bead types. Both are characterized by particular bead types and share personal ornaments with the intermediate groups, composed of sites from western France, Spain, and southern France. This pattern, which is not explained by chronological differences between sites or by differences in raw material availability, has been interpreted as reflecting the ethnolinguistic diversity of the earliest Upper Paleolithic populations of Europe.

#### 2.4.4 *Asia*

In Asia, eleven EUP sites from Siberia and in particular from the Altai and West Baikal have yielded personal ornaments (Anikovich 2005;

Derevianko 2005; Derevianko and Rybin 2005; d’Errico and Vanhaeren 2007). Some of them were recovered from sites (Denisova, Podzvonkaya, Khotyk, Kara-Bom, Maloyalomanskaia) that are at least as old as the Aurignacian in Europe and could well be contemporaneous with earlier European EUP technocomplexes. The repertoire of personal ornaments from these sites is varied (32 types recorded) and comparable to that observed at Aurignacian and Châtelperronian sites. Additionally, a number of types closely resemble, with the noteworthy exception of the OESB, those found at Aurignacian sites from northern and central Europe.

Evidence for early beadworking elsewhere in Asia is scant and in many areas ornaments are not reported from sites older than 20 ky BP and may be relatively rare or absent until 10 ky BP.

#### 2.4.5 *Australia*

The earliest evidence for bead manufacture and use comes from the site of Mandu Mandu, Cape Range of Western Australia, where 22 *Conus* sp. shell beads were recovered in a layer dated to c. 32 ky BP (Morse 1993). Six have their apex perforated and the columella removed to form a hollowed-out shell with a round hole at the top. A second type of bead is a shell ring obtained by cutting the shell perpendicularly to its main axis. Ten Dentaliidae shell beads are reported from the 30 ky old layers of Riwi in the Kimberly of Western Australia, a site located 300 km inland (Balme and Morse 2006).

## 2.5 Discussion

### 2.5.1 *Evolution or revolution?*

Current evidence on the earliest use of beads supports neither the Human Revolution nor the Out of Africa model for the emergence of modern behavior. On the one hand, personal ornaments clearly predate the arrival of AMH in Europe and the 50,000-year-old rapid neural mutation that (according to some authors) would have qualitatively changed human cognition. On the other hand, no continuity is observed in beadworking traditions after their first occurrence in the archeological record. Beads are not found in southern Africa at sites attributed to the Howieson Poort, the

archeological culture that follows chronologically and stratigraphically (Wadley 2006; Rigaud et al. 2006) the cultural entity (Still Bay) to which shell beads are associated at Blombos Cave. They reappear in the same region 35 ky later in the form of OESB. A similar picture is seen in North Africa and the Near East. At Taforalt, shell beads are restricted to a single layer in spite of this site featuring a long Middle Paleolithic sequence, and personal ornaments are absent from younger North African sites until the Upper Paleolithic. The same applies to the Near East, where perforated shells occur sporadically at old sites but are absent afterwards, and only reappear with the Ahmarian, 50 ky later. This pattern can hardly be attributed to a lack of investigation. A large number of sites younger than those that have yielded personal ornaments have been excavated in North and southern Africa and the Near East. In this context, a reliable expectation is that while more ancient sites with shell beads will be identified, they won't be able to fill the gap.

The archeological evidence also contradicts the view that after their invention, beadworking traditions became increasingly complex. If it is true that the first personal ornaments consist of shell beads belonging to a single species, no intermediate stage is observed between this use and the "explosion" that characterizes the earliest Upper Paleolithic beadworking traditions since their very beginning. In addition, such complexity is not found during the same period in Africa, where beads seem to reappear and remain for a long period of time in the form of OESB.

Not only does the empirical evidence contradict a linear evolution of this behavior, but it also reveals the behavior's independence from taxonomic affiliation.

The production and use of a varied repertoire of personal ornaments by Neanderthals at the end of their evolutionary trajectory contradicts both models since it demonstrates that this alleged hallmark of modernity was perfectly accessible to other fossil species. A growing body of evidence suggests that ornaments may have been independently invented in Europe before the arrival of Aurignacian moderns (Zilhão and d'Errico 2003b; Zilhão et al. 2006). Even if it could be demonstrated that the use of personal ornaments by Neanderthals resulted from cultural contact, this would in fact reinforce rather than dismiss the modern character of their cognition since it would show their ability to incorporate external stimuli and reshape those influences in order to make them an integral part of their own culture.

We argue that the failure of the Revolution and the Out of Africa models to account for the origin of personal ornaments is due to the fact that they both directly link this phenomenon to the emergence of modern cognition. The alternative view (d’Errico 2003; Zilhão 2006; d’Errico and Vanhaeren 2007) is that the cognitive prerequisites of modern human behavior were in place prior to the emergence of both “archaic” and “modern” populations. This would lead us to invoke historical contingencies triggered by climatic and demographic factors (as opposed to neural mutations) to explain the emergence, disappearance, and re-emergence in the archeological records of hallmarks of modernity such as beadworking traditions. According to Hovers and Belfer-Cohen (2006), Middle Paleolithic populations underwent recurrent demographic crashes that reduced their capacity to store knowledge and obliged them to “reinvent” the same or similar innovations. Were Middle Paleolithic societies more vulnerable to environmental changes due to their social systems or ways of transmitting knowledge? Answering this question remains a primary challenge for the archeology of behavioral modernity.

The more recent discoveries of early personal ornaments from North Africa and the Near East suggest that when innovations of this kind arose, they were able to traverse cultural boundaries and diffuse over large territories. Documented lithic raw material procurement patterning in the African MSA and the Levantine Mousterian only rarely exceeds 100 km, and is generally much lower. The transport of shells over distances up to 200 km (Oued Djebbana) and of more than 40 km in the case of the shell beads from Taforalt suggests the existence, already at this early stage, of previously unrecorded interlinking exchange systems or of long-distance social networks. These networks apparently crossed cultural boundaries defined by lithic technology, since at least three of the four sites where similar bead types were found can be attributed to different technocomplexes, the Aterian for Taforalt and Oued Djebbana, the Still Bay for Blombos, and the Levantine Mousterian for Skhul.

### 2.5.2 *Beads and language*

Most archeologists believe that the cross-cultural analysis of historically known human societies can identify regularities that may help shed light on the way people thought, communicated, and acted in the past (Binford

1983; Gardin 1990; Renfrew and Zubrow 1994; Renfrew 1996; Tschauner 1996; Roux 2007). To reject any kind of uniformitarianism as an epistemological foundation or any kind of analogy as an heuristic tool would be to deny to archeology (not to mention other disciplines concerned with the past) the possibility of saying anything sensible about ancient times. Contrary to what has been recently suggested (Wynn and Coolidge 2007), the use of analogy in this field cannot be described as tautology. And, as archeologists, we do not need to first identify the cognitive structures involved in order to establish a link between, for example, communication skills and material culture. To be even more provocative, we do not need to propose a model for the cognitive changes that led to a given innovation before we can detect this innovation in the archeological record and make inferences about the role it may have played in the past. Scenarios shaped by other disciplines concerning the origin of cognitive innovations are of course vital. But they are useful to archeologists only if they are falsifiable, i.e. if they can be tested against the empirical evidence.

Personal ornaments play many different roles in human societies, but all are eminently symbolic (Vanhaeren 2005 and references therein). Personal ornaments represent a technology specific to humans that signals their ability to project a meaning onto the members of the same or neighboring groups by means of a shared symbolic language (Vanhaeren and d'Errico 2006; Kuhn and Stiner 2007). Human language is the only known natural system of communication that has a built-in meta-language that enables the generation of other hierarchically structured symbolic codes. Once created, these codes are shared by the members of a society and transmitted, as with language, from one generation to another (Peirce 1931–5; Deacon 2003; Bickerton 2003). Only a communication system like human language or equivalent to it can unambiguously transmit the symbolic meaning of signs as well as the structured links between them.

Symbolic meanings can be attributed to elements of the natural world (such as humans, animals, or features of the landscape) but doing this leaves no detectable archeological signature. When symbolic codes are embodied in material culture, the link between meaning and referent becomes not only arbitrary but also, as with sounds in language, artificially created. This freedom from natural constraints allows the members

of a social group to locate symbols in strategic locations and spatially—if not syntactically—to organise the links between them. Apes are able to learn referential symbols and represent other minds in socially competitive contexts (Byrne 1995; Rumbaugh and Washburn 2003; Tomasello et al. 2005). Chimpanzees clearly have the capacity to develop and transmit cultural traditions (Whiten 2005), but in the wild they have never been observed creating systems of symbols, displaying them on the body, or embodying them in their material culture.

We argue that symbolic items with no utilitarian purpose, created for visual display on the body, and the meaning of which is permanently shared by the members of a community, represent a quintessential archaeological proxy for the use of language or, at least, of an equally complex communication system. Symbols applied to the physical body ascribe arbitrary social status to the wearers that can be understood by other members of the group only if the latter share the complex codes that establish a link between the worn items, the place and way they are displayed on the body, the social categorisation they signal, and the symbolic meaning carried by the objects. No “institutionalised” symbolic meaning can be transmitted without language abilities (Searle 1996; Knight, this volume). The variety in their morphology, color, raw material, perforation, and shaping techniques, as well as their geographic variability and associations, indicate that the personal ornaments found at Aurignacian sites perfectly fit the interpretation that they reflect complex codes and were conceived to project a meaning on the members of the same or neighboring groups by means of a shared symbolic language. Such a complexity, which is comparable to if not higher than that observed in historically known traditional societies, implies language abilities equivalent to ours.

If we accept this we must also, by the same token, grant similar language abilities to the Châtelperronian Neanderthals. At least 15 different personal ornament types, produced with different raw materials (teeth, fossils, ivory, and bone) and manufacturing techniques are attested at Châtelperronian sites. Nine types are found at the Grotte du Renne alone. The number of bead types found at Aurignacian sites varies between one and 40 (Vanhaeren and d’Errico 2006). The latter figure, however, is only found at the multistratified site of Mochi. Most of the other sites have yielded fewer bead types than Grotte du Renne. This suggests that the



symbolic codes embodied in personal ornaments by late Neanderthals were of a complexity comparable to those used by Aurignacians when using the same media.

The inescapable corollary is that Neanderthals must have had a communication system at least equivalent to the one we can infer for Aurignacian moderns. This is consistent with recent genetic evidence (Krause et al. 2007a) indicating that a critical gene known to underlie speech—namely FOXP2—was present in the Neanderthal genome and that its appearance predates the common ancestor (dated to around 300–400 ky) of modern humans and the Neanderthals (see Diller and Cann this volume).

It might be countered that the personal ornaments found at older African and Near Eastern sites do not suggest the complexity of ornament use that we observe in Europe at the beginning of the Upper Paleolithic and that for this reason, these first instances of body decoration should be seen as mirroring an intermediate stage of language capacities, that of protolanguage rather than syntactical language. However, cases of historically known human societies whose members wear beadwork made of a single bead type or perishable beads associated with a single archeologically visible type are well documented (Ambrose 1998; Vanhaeren 2005). Since the evidence falls within the known variability of modern human cultural behavior, it would be more parsimonious to attribute the single-bead tradition to cognitive capacities equal to those underlying complex beadworking traditions rather than invoking an intermediate stage in the evolution of modern capacities. The case is especially compelling in view of the fact that the analysis of the personal ornaments found so far at Blombos Cave, Grotte des Pigeons, Oued Djebbana, and Skhul reveals consistencies that fit well with the language hypothesis. The archeological layers at Blombos that have yielded shell beads cover a time span of centuries and probably even millennia. A long-lasting tradition of shell bead use in North Africa and the Near East is also suggested by the available chronometric evidence. This indicates that the use of these beads was a permanent form of behavior in these societies. Dozens of shell species were available on the shores of both regions. Had the tradition in question been an idiosyncratic behavior performed by individuals using shell beads only rarely and with no communicative

purpose, we would expect to find various species used over this time span, not just one. For an immediate or short-term use, performed with no specific goal in mind by a single individual or a few individuals, it would be reasonable to see the shell species used being those available in close proximity to the sites. At Blombos, by contrast, the shells were collected in estuarine environments located at least 20 km from the site. In North Africa and the Near East, shells are found at sites that were, at the time of their occupation, 3–20 km (Skhul), 20–30 km (Qafzeh), 40–50 km (Taforalt), and 200 km (Oued Djebbana) from the sea.

The techniques used in the production of the Blombos beadwork attest to the shared nature of the knowledge involved. Experimental reproduction of the perforations observed on the Blombos beads indicates that they were probably made by punching the shell through the aperture with a thin bone point. Pointed bone tools suitable for this task, made of bird bone, have been found at the site. The perforations on well-preserved shells from Blombos are tiny and must have required particularly thin albeit robust strings. These strings and the beads were deliberately stained with ochre, a material that, like the shells, is not immediately available at Blombos and was certainly ground and mixed with a binder before being applied to the string and the shells. Short use of shell beads leaves no or little use-wear (d'Errico et al. 1993). In contrast, the intense wear pattern recorded on the beads from Blombos and Grotte des Pigeons, along with the results of ongoing experimental reproduction of use-wear on Blombos shell, indicate that the shells were used for a long period of time and probably permanently worn by some or all of the Blombos inhabitants. The consistent location of the use-wear on Blombos shells suggests that the way the beads were arranged and strung did not change over time. This contradicts the hypothesis of a random behavior and better fits a scenario in which these shells were a part of beadwork routinely used by the Blombos inhabitants and worn or displayed on the body in a consistent manner.

In conclusion, present evidence indicates that the choice, transport, modification, coloring, and long-term wearing of these items were all part of a deliberate, shared, and transmitted form of symbolic behavior. To be conveyed from one generation to another and, in the case of North African and Near Eastern sites, over such a wide geographic area, this

behavior must have implied powerful cultural conventions that could not have survived if they were not intended to record meaning and if this meaning was not shared and transmitted. Only beings in possession of language or language-like systems of representation could have created and maintained such conventions.

# 3 Reading the artifacts: gleaned language skills from the Middle Stone Age in southern Africa

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CHRISTOPHER STUART HENSHILWOOD AND  
BENOÎT DUBREUIL

## 3.1 Introduction

Finding archeological evidence that hints at when people first used language, in the “modern” sense, has long fascinated archeologists. Most archeologists are not linguistic specialists, but speculating on the origins of language, or at least commenting on the topic, seems an innate component of many archeological publications, especially those relating to the origins of the behavior of our own species, *Homo sapiens* (e.g. Noble and Davidson 1996; Deacon and Wurz 1996; Ambrose 2001; Deacon 2001; Barham 2001; Wadley 2001; Henshilwood et al. 2002; Mellars 2006). The terminology used by archeologists to describe “modern” language varies and includes “syntactic” language (Barham 2002a), “symbolic” language (Wurz 2000), “modern, complex” language (Mellars 2006), and “phonemic” language (Klein and Edgar 2002). Broadly speaking, these terms are understood to mean the ideas or emotions that were communicated by means of symbolic elements, for example vocally, by gesture, or by marks, and that these elements can be recombined according to systematic, conventionalized criteria to create meaning. Members of society interact with one another in terms of their total culture through language,

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a non-instinctive, human institution. It is according to the above definitions that the terms “modern” language and “syntactic” language are used in this chapter.

Linguistic theory that may have helped formulate archeological models for language has been lacking, partly because the origins of language was not in the forefront of linguistic research. Chomsky (1965), who was evasive on both the evolution of language and the theory of natural selection, actively discouraged such research (Jackendoff 2003). He argued that the ability to learn the rules of grammar is innate and that the linguistic system in the mind/brain of an individual is the appropriate object of study. Recent efforts by archeologists, linguists, and behavioral scientists to jointly explore the origins of language are encouraging. The goal is to integrate linguistics with the other cognitive sciences, not to eliminate the insights achieved by any of them. To understand language and the brain, we need all the tools we can get. But everyone will have to give a little in order for the pieces to fit together properly (Jackendoff 2003: 651).

One result of the lack of a solid multifaceted foundation for the origin of language is that most arguments put forward by archeologists center on first attempting to identify “symbolic” material culture and then assuming there is a link with “syntactic” language (e.g. Henshilwood and Marean 2003; d’Errico et al. 2003; Wadley 2001; Mellars 2006). The “symbolic explosion” associated with the Upper Paleolithic in Europe after c. 35 ky provides a clear example of this common association. Cave art, personal ornaments, and bone or ivory with carved or engraved designs from European sites are typically portrayed as central to identifying the origins of our symbolic abilities (Mellars 1989; Mithen 1996; Gamble 1999). A common following assumption is that these “modern” people must have had the facility for and used “syntactic” language. This seemed to make sense as without syntax in language it would arguably not have been possible to convey, within and across individuals or groups, the meaning of these material symbols, for example the rock art or personal ornaments. In the late 1980s it seemed that, with few exceptions, the most positive evidence for symbolic material culture lay in the Upper Paleolithic of Europe and this evidence also suggested a *terminus post quem* of c. 35–40 ky for modern or syntactic language. The core of a number of papers presented at the seminal “Human Revolution” conference held in Cambridge in 1987 (Mellars and Stringer 1989) revolved around this doctrine.

Since the late 1980s there have been several developments which indicate that the origins of “modern” language may lie in Africa. First, a number of authors have readdressed the theoretical definition of fully symbolic *sapiens* behavior and importantly how this behavior may be recognized in the archeological record. For example, assessing whether an artifact or behavior perhaps functioned in a symbolic way, and addressing how these “symbolic” functions may have differed in Africa and Europe, has opened up the definitions of what we mean by behavioral modernity. This method enlarges the restricted parameters of an approach that confines “symbolic culture” mainly to an Upper Paleolithic-derived checklist (see Henshilwood and Marean 2003). Second, new clues to the origin of “modern” language are emerging from genetics and from African Middle Stone Age archeological sites. An entry point into the genetic and neural basis for language may be provided by slight variation in the protein encoded by the FOXP2 language gene in humans at c. 100 ky (Enard et al. 2002; Lieberman 2005). Coincident with the evolution of *H. sapiens* before 200 ky (Ingman et al. 2000; Cavalli-Sforza 2000) is a rapid change in human material culture (McBrearty and Brooks 2000). The apparent sameness that characterized the Acheulian lifestyle and material culture of hominins for more than a million years stands in stark contrast to the relative cultural complexity of the Middle Stone Age, a period that encompasses the first frequent emigrations of *H. sapiens* out of Africa into Asia, Europe, and Australia after c. 80–60 ky (Mellars 2006; Grine et al. 2007; Manica et al. 2007). But clearly language did not evolve in the Middle Stone Age and its origins, in whatever simple form, must lie with the earliest hominins (for a full discussion see e.g. Bickerton 1995, 2003; Dunbar 1996; Mithen 1996; Pinker and Jackendoff 2005; Johansson 2005; Corballis 2002b).

In this chapter we consider first the contribution that archeology can make in tracing the origins of specifically “syntactic” language. We refer to the Still Bay and Howiesons Poort periods in southern Africa and in particular draw on evidence from Blombos Cave. With the aid of cognitive psychology we then construct one scenario that explains how material culture can reflect the presence of “syntactic” language and how we can “read” this evidence from the archeological record. The proposed framework relates the emergence of symbolic artifacts with a domain-general cognitive change that enabled modern humans to represent conflicting perspectives of objects.

### 3.2 Language, *Homo sapiens*, and the Middle Stone Age

The Middle Stone Age (MSA) in Africa starts at c. 300 ky or slightly earlier (McBrearty and Brooks 2000) and is associated with anatomically modern *H. sapiens*. Early physical evidence comes from Omo (McDougall et al. 2005) and Herto (White et al. 2003), both in Ethiopia and dated respectively at c. 195 ky and c. 160 ky. A review of the African evidence by McBrearty and Brooks (2000) indicates that a variable montage of cognitive advances associated with anatomically modern humans can be detected in the MSA. However, the development of “modern” behavior is likely to have been a vast and complex mosaic of events and a number of authors make the point that the likely scale and repertoire of “modern” behavior in the Middle to Late Pleistocene is enormous (cf. Chase and Dibble 1990; Foley and Lahr 1997, 2003; Gibson 1996; Renfrew 1996; Deacon 2001; McBrearty and Brooks 2000; Henshilwood and Marean 2003). Was this also the case for the evolution of syntactic language? Although not specifically answering this question a number of archeologists do argue that the origins of “syntactic” language lie in the MSA (Milo 1998; Wurz 2000; McBrearty and Brooks 2000; Ambrose 2001; Barham 2002a; Henshilwood et al. 2002; Klein and Edgar 2002; Henshilwood and Marean 2003; d’Errico et al. 2005; Brumm and Moore 2005; Mellars 2006). Reference to “modern” humans is also taken by many authors to implicitly assume the inclusion of “modern” or syntactic language. Whether the evolution of syntactic language could have had independent origins at various locales in Africa is not easily answered. While direct evidence for language does not survive, material culture does, or at least some of it, and interpreting this data has become a focal point for language studies by archeologists. A brief review of some of the pertinent evidence follows.

During the Acheulian to MSA transition the Middle Awash valley of Ethiopia and the Olorgesailie basins of Kenya constituted a major center for behavioral innovation (Brooks 2006b). It is likely that the large terrestrial mammal biomass of these regions supported substantial human populations with subsistence and manufacturing patterns similar to those of ethnographically known foragers. The use of syntactical language at these sites associated with what appear to be cognitively modern humans cannot be excluded (Brooks 2006b). Blades and backed pieces from the Twin Rivers and Kalambo sites in Zambia dated at c. 300 ky indicate a suite of new behaviors (Barham 2002a) and Barham (2001: 70)

believes that syntactic language was one behavioral aspect that allowed these MSA people to settle in the tropical forests of the Congo. A high level of technical competence is also indicated for the c. 280 ky blades recovered from the Kapthurin Formation, Kenya (Deino and McBrearty 2002) and the same argument for language could be applied here. Wurz et al. (2003) contend that distinct technological changes in lithic style between the MSA I period (c. 110–115 ky) and the MSA II (c. 94–85 ky) at Klasies in the Western Cape is associated with cognitively modern behavior and, by extension, language. Ochre is reported from some early MSA sites, for example at Kapthurin and Twin Rivers, and is common after c. 100 ky (Watts 2002). Barham (2002b) argues that even if some of this ochre was used in a symbolic, color-related role then this abstraction could not have worked without language. Ochre, he suggests, could be one proxy for trying to find the emergence of language. Formal bone tools are frequently associated with “modern” behavior by archeologists (e.g. Klein 2000; Henshilwood et al. 2001b). Sophisticated bone harpoons manufactured at Katanda, west Africa at c. 90 ky (Yellen et al. 1995; Brooks et al. 1995) and those from Blombos Cave dated at c. 77 ky (Henshilwood et al. 2001b) may then also serve as examples of material culture associated with “modern” language.

Evidence for “modern” subsistence behavior in the MSA that may also link with the origins of syntactic language comes from a number of sites. Based on his analysis of the MSA bovid assemblage at Klasies, Milo (1998) reports MSA people were “formidable” hunters and that their social behavior patterns approached those of “modern humans.” Deacon (2001: 6) maintains that the management of plant food resources through deliberate burning of the veld to encourage the growth of plants with corms or tubers in the southern Cape during the Howiesons Poort (c. 70–55 ky) is indicative of modern behavior. A family basis to foraging groups, color symbolism, the reciprocal exchange of artifacts, and the formal organisation of living space are, he suggests, further evidence for modernity in the MSA.

While archeologists might presuppose that “modern” behavior can be read from the evidence above it is seldom made clear how or by what method “syntactic” language is recognized (see Henshilwood and Marean 2003; d’Errico et al. 2005). The assumption that there is a link between “symbolic” material culture and “syntactic language, or for that matter modern behavior (e.g. Henshilwood et al. 2002, 2004; d’Errico et al. 2005),



has correctly been questioned (e.g. Donald 1998; Wynn and Coolidge 2007) and it is this challenge we address here.

By c. 80–60 ky MSA humans spread out of Africa to Asia, Australia, and Europe (Mellars 2006), perhaps only in small numbers initially (Manica et al. 2007), but by c. 30 ky they had replaced Neanderthals and *H. erectus*. Based on the measurement of a large number of human skulls a recent study supports a central/southern African origin for *H. sapiens* as this region shows the highest intra-population diversity in phenotypic measurements. Genetic data supports this conclusion (Manica et al. 2007: 346). What made these African hominids so successful? A critical factor was their behavior. Although the advent of anatomical physical modernity cannot confidently be linked with paleoneurological change (Holloway 1996) it does seem probable that hominid brains evolved through the same selection processes as other body parts (Gabora 2001). Genes that promoted a capacity for symbolism may have been selected, suggesting that the foundations for symbolic culture may well be grounded in biology but behavior that was mediated by symbolism may have only come later, even though this physical capacity was already in place much earlier. Symbolically mediated behavior may variously have been adopted, perhaps rejected and readopted. Only when these new behaviors conferred a sustainable advantage during this proofing process would their adoption have become permanent. Many authors have speculated that at the core of this “symbolic explosion,” and in tandem, was the development of syntactic language that evolved through a highly specialized social learning system (Richerson and Boyd 1998) providing the means for “semantically unbounded discourse” (Rappaport 1999). Syntax would have played a key role in this process and its full adoption could have been a crucial element of the symbolic behavioral package (Bickerton 2003). In the discussion we argue that syntactic language is essential to convey symbolic meaning, although the emergence of symbolic artifacts in the archeological record may not be the result of a change in syntax.

### 3.3 The transition to behavioral modernity

If any claims are to be made that *H. sapiens* were behaviorally “modern” before they left Africa and that the European “symbolic” explosion was in fact only one result of earlier behavioral advances in Africa, material

evidence for this needs to stem from Africa. Theoretical models may not have the capacity to argue for an African or non-African origin for syntactic language but if the “hard” evidence for this does lie in Africa then the place to look for it is in the archeological record. The latter assumption presents its own problems as there is no common agreement on how symbolic behavior or “syntactical” language is recognized in ancient African material culture. The term “modern” requires definition, particularly because of its assumed links with “syntactical” language. A number of suggestions have been published, including “symbolically organised behavior” (Chase 2003: 637), “fully cultural” (Holliday 2003: 640) and, as stated above, “fully symbolic *sapiens* behavior” (Henshilwood and Marean 2003: 644). The key criterion for modern human behavior is not the capacity for symbolic thought but the use of symbolism to mediate behavior. In this paper we have adopted the definition for “modern behavior” proposed by Henshilwood and Marean (2003: 635):

Modern human behavior is mediated by socially constructed patterns of symbolic thinking, actions, and communication that allow for material and information exchange and cultural continuity between and across generations and contemporaneous communities.

One way to translate this into finding evidence for “modern” and “syntax” in the archeological record is provided by Donald’s (1991) three-stage model. A key point he refers to in the third stage is the ability to store and apply symbols externally—this allows material culture to intervene directly on social behavior. Thus, according to Donald (1991) the transition to symbolically literate societies is a defining factor for behavioral “modernity” and by extension “syntactical” language. Material culture from African or Eurasian sites has the potential to inform an assessment of the origins of “modernity” but a note of caution is that symbolic artifacts, even of the more elaborate kind, rarely encode the conventions governing their use and may not contain enough information to allow us to rediscover the detailed thought-habits of an ancient culture *a posteriori* (Donald 1998: 184), nor their use of syntactic language. In the discussion, we propose a cognitive framework that directly addresses this problem and explain how the capacity to store symbols externally resulted from a domain-general cognitive change in *H. sapiens*.

### 3.4 The Still Bay and Howiesons Poort

Two remarkable periods that fall within the MSA in southern Africa, the Still Bay (c. 77–73 ky) and the Howiesons Poort (c. 70–55 ky), have the potential to provide insights into the origins of “modernity” and “syntactical” language. These phases may well represent two examples of localized evolution. Continuity between the Still Bay and the Howiesons Poort is not a certainty but a limited presence of backed artifacts at some Still Bay sites suggests that there was an overlap. Lithics that are novel and in some cases precocious for the Middle Stone Age represent one aspect of these two phases (e.g. Foley and Lahr 1997, 2003; Mellars 2005), as does the recent recovery of engraved bone (d’Errico et al. 2001) and ochre (Henshilwood et al. 2002). Formal bone tools (Henshilwood et al. 2001b) and personal ornaments (Henshilwood et al. 2004; d’Errico et al. 2005) from the Still Bay and engraved ostrich egg shell from the Howiesons Poort (Rigaud et al. 2006) further support arguments for the early evolution of symbolically mediated behavior in Africa. It now seems highly probable that developments within these phases contributed directly or indirectly to the expansion of *H. sapiens* within and out of Africa about 80–60 ky ago (Mellars 2006). For this paper we have chosen Blombos Cave as a case study as we believe aspects of the material culture recovered from the c. 77–73 ky levels at this site provide the means for identifying one early example of “syntactic” language linked to fully modern sapiens behavior. The link between syntactic language and modern behaviors, however, does not imply that behavioral modernity is caused by a change in syntax. We will rather argue that the capacity to represent conflicting perspectives of objects was responsible for the emergence of symbolic artifacts at both Blombos and other MSA sites.

### 3.5 Blombos Cave—early evidence for symbolism

Blombos Cave is located near Still Bay in the southern Cape, South Africa (Henshilwood et al. 2001a). Three phases of MSA occupation have been named M1, M2, and M3 (Figure 3.1). Dating by the optically stimulated luminescence (OSL) (Henshilwood et al. 2002; Jacobs et al. 2003a, b, 2006) and thermoluminescence (TL) methods (Tribolo 2003; Tribolo et al. 2006)

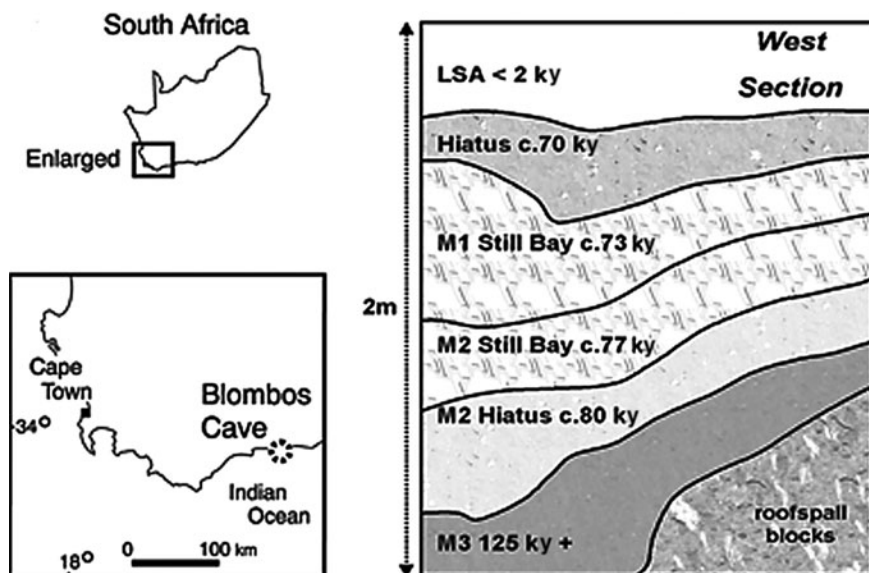


FIG. 3.1 Location of Blombos Cave showing west section of the excavation. M1, M2, and M3 are occupation phases within the Middle Stone Age levels.

has provided occupation dates for each phase: these are c. 73 ky for the M1 Still Bay phase, c. 77 for the M2 Still Bay phase, c. 80 ky for the M2 low density phase, and >125 ky for the M3 phase (Figure 3.1).

### 3.5.1 Material culture

The M1 and M2 phases are of particular interest in this paper and both fall within the Still Bay complex. Artifacts from the M1 phase include bifacial points, bone tools, marine shell beads, and engraved ochre (Plate 3). In the Upper M2 phase bifacial points and bone tools were recovered (Henshilwood and Sealy 1997; Henshilwood et al. 2001a, b, 2002, 2004; d'Errico et al. 2001, 2005). Unworked and striated ochre pieces are ubiquitous in both phases. Well-preserved evidence of terrestrial animals hunted and gathered and extensive exploitation of marine resources comes from both phases, indicating modern subsistence practices (Henshilwood et al. 2001a). We highlight briefly below the marine shell beads and the engraved ochre.

### 3.5.2 *Marine shell beads (Plate 3d, e)*

In the archeological literature beads are indisputably regarded as symbolic artifacts and indicative of “modern” human behavior (Mellars 1989, 2005; Klein 2000; McBrearty and Brooks 2000; Wadley 2001; Klein and Edgar 2002; d’Errico et al. 2005). More than 65 marine shell beads have been recovered from the MSA levels at Blombos (Henshilwood et al. 2004; d’Errico et al. 2005). *Nassarius kraussianus* (tick shell) shells were brought to the site from rivers located 20 km west and east of the cave and then pierced by inserting a small bone tool through the aperture to create a keyhole perforation (Plate 3d) (d’Errico et al. 2005).

Distinct use-wear is visible, consisting of facets which flatten the outer lip or create a concave surface on the lip close to the anterior canal. Wear patterns on the shells from the thread and also from repeated contact with human skin tell us that some of these “necklaces” or “bracelets” were worn for considerable periods of time, very possibly more than a year. Use-wear patterns are the principal factor that defines the MSA shells as beads. Microscopic residues of ochre detected inside the MSA shells (Plate 3e) may also result from such friction or deliberate coloring of the beads (Henshilwood et al. 2004; d’Errico et al. 2005). Beads were found in groups of 2 to 17 and within each group beads display a similar size, shade, use-wear pattern, and type of perforation. Each bead cluster may represent beads coming from the same beadwork item, lost or disposed during a single event. This information clearly indicates that the wearing of beads at Blombos was not an idiosyncratic act by one person but rather a shared behavior with symbolic meaning within the group. Wearing personal ornaments such as beads implies self-awareness or self-recognition (Bower 2005: 121) and we explain below how this capacity to put oneself in perspective involves the same cognitive abilities that are required to transform an object into a symbol.

### 3.5.3 *Engraved ochres (Plate 3c)*

Two unequivocally engraved pieces were recovered in situ from the M1 phase (Henshilwood et al. 2002) and nine potentially engraved pieces are under study. The former were recovered from undisturbed contexts and are stratigraphically secure. Both flat surfaces and one edge are modified by scraping and grinding on AA 8937 and the larger ground edge carries a

cross-hatched engraved design. On AA 8938 (Plate 3c) the engraving consists of a row of cross-hatching, bounded top and bottom by parallel lines. A third parallel line through the middle forms a series of triangles. For both pieces the choice of raw material, situation and preparation of the engraved surface, engraving technique, and final design are similar, indicating a deliberate sequence of choices and intent. Arguably the engraved Blombos ochres are the most complex and best formed of claimed early abstract representations (d'Errico et al. 2003; Lewis-Williams and Pearce 2004; Mellars 2005; Mithen 2005). They are not isolated occurrences or the result of idiosyncratic behavior, as suggested for many early “paleo-art” objects and would certainly not be out of place in an Upper Paleolithic context (Mellars 2005).

### 3.6 Discussion

Although the material culture found at Blombos is taken by some as indisputable evidence of modern language and cognition, there is a persistent difficulty in mapping the archeological data onto a non trivial cognitive framework. Wynn and Coolidge (2007: 88) rightly mention that archeologists remain muddled when attempting to explain what cognitive structures enabled the modern behaviors they describe: “We do not mean to suggest that beads, or ochre, or engraved bones, might not be acceptable bits of evidence for modern behavior [ . . . ]. However, they cannot stand as evidence for modern cognition unless one can specify the cognitive abilities they require.” The problem is particularly acute when archeologists introduce the concept of symbolism to explain the use of beads and other artifacts: “As we have seen, symbolic reference itself is not difficult. So what cognitive ability is required for the invention and maintenance of symbolic culture? As much as we would like to conclude that enhanced working memory is the answer, we cannot” (Wynn and Coolidge 2007: 88).

In this next section, we address this challenge and propose a framework that specifies what cognitive ability is required for the invention and maintenance of symbolic culture, but also that explains why symbolic culture appears in the MSA archeological record along with other behaviors whose symbolic component is far less obvious.

### 3.6.1 *Perspective on symbols*

In their discussion of the possible use of beads at Blombos, Wynn and Coolidge (2007: 88) suggest one possible connection with their own cognitive account based on enhanced working memory:

The use of beads at Blombos suggests attention to personal identity. At a minimum it suggests that individuals attended to how others saw and understood them. This is true theory of mind [...] and, like recursion in language, depends on attentional capacity and working memory. Note that this does not require that the beads stand for anything at all, but it does require a level of intentionality typical of modern human social interaction.

We would like to take this argument further. The wearing of beads suggests that one person can understand *how* she looks from the point of view of another person. This task, although it may look simple, is complex. In cognitive psychology, it is well established that children under the age of four fail to fully represent the point of view of others.

Psychologists usually draw a distinction between perspective-taking at level-1 and level-2 (Flavell 1992). Level-1 perspective-taking develops in the second year of life and enables children to “understand that the content of *what* they see may differ from the content of what another sees in the same situation” (Moll and Tomasello 2006: 603). This ability is also present in apes (Hare et al. 2000, 2001). Level-2 perspective-taking, on the other hand, involves the capacity to reconstruct *how* an object looks from another person’s perspective.

Interestingly, level-2 perspective-taking develops in children along with other closely related abilities. One of them is the capacity to distinguish appearance from reality. In a classic experiment, Flavell et al. (1983) presented children with a sponge that looked like a rock and asked children to identify the object. All of them answered that it was a rock. The experimenter then let the children touch the object. Once they realized that it was sponge, the experimenter asked what the object looked like. Children under four said it looked like a sponge, while older children said it looked like a rock, although it was a sponge. The lesson that the older children learned is that appearance can differ from reality.

A second task that is closely related to level-2 perspective-taking is the understanding of false beliefs. Children by the age of four are able verbally to ascribe a false belief to someone about an object’s location; this task is

considered as a pinnacle of mind reading (Tomasello et al. 2005). In this classic false-belief task, the child has to explain that someone thinks an object is somewhere even if the child knows that the object is somewhere else (Wimmer and Perner 1983). The developmental synchrony between the understanding of false beliefs, the appearance–reality distinction, and level-2 perspective-taking is not that surprising. All these tasks require that the child represents potentially conflicting viewpoints about an object (Mounoud 1996; Perner et al. 2002).

The connection of level-2 perspective-taking with the use of symbols is straightforward. As Wynn and Coolidge (2007) point out, symbolic reference, the very act of referring to an object by using an arbitrary token, is not difficult. So what other factors in human behavioral evolution must have been in place before symbols could appear in material culture? The standard answer is a change in language or, more precisely, in syntax (Bickerton 2003; Corballis 2004; Coolidge and Wynn 2007). From this point of view, fluent speech and recursive syntax make it possible to talk about abstract things, and this in turn facilitates cultural transmission and the use of symbols. The weakness of this account is that it does not explain precisely how fluent speech and recursive syntax are necessary for symbol use, and why symbolic reference is insufficient. In a similar way, Donald's three-stage model defines the external storage of symbols as the hallmark of modern cognition, but does not specify what cognitive mechanism enables the transition to symbolic material culture (Donald 1991, 1998).

We propose that the cognitive change that led to cognitive modernity was similar to the one that enables children to understand level-2 perspective-taking, false beliefs, and the appearance–reality distinction. This does not mean, however, that cognition and language in archaic humans and Neanderthals was similar to that of three-year-old modern children. Other cognitive functions, including long-term memory, could have been very similar in different hominins (Coolidge and Wynn 2004; Wynn and Coolidge 2004). It simply means that a gradual change in higher-level cognition could have fostered the whole diversity of modern behavior and, in the first place, the use of symbolic artifacts. How can this be possible?

### 3.6.2 *Turning an object into a symbol*

At the most basic level, a symbol is “something that someone intends to represent something other than itself” (DeLoache 2004). To transform an



object into a symbol, however, is more cognitively demanding than using a string of phonemes to refer to an object (Donald 1998; Chase 2006). Symbolic reference develops in the second year of life and builds on gaze detection and affective mechanisms that motivate toddlers to share their attention with others (Tomasello et al. 2005). Without these mechanisms, children would not learn their first words.

The symbolic use of objects develops later and two stages can be identified. The first stage develops during the second and third years of life when children begin to associate real objects with replicas or representations of these objects (DeLoache 2000, 2004). Children understand that a toy truck should be matched with the picture of a truck and not with that of a hammer (Younger and Johnson 2004). In brief, they grasp the iconic relationship between objects and their representation. It is also during the second and third years of life that children begin to pretend that an object is something different (Leslie 1987). Pretend play becomes more complex and creative with time and can be said to be symbolic in a weak sense as it involves treating an object *as if* it were something else (Striano et al. 2001).

A second stage appears along with level-2 perspective-taking and full-fledged theory of mind between four and five years. Children of that age begin not only to use symbolic artifacts, but to think and talk explicitly about them through meta-representations (Rakoczy et al. 2005). They understand, for instance, that someone who knows nothing about rabbits, but who produces a rabbit-shape drawing, can not really be drawing a rabbit (Richert and Lillart 2002). It is also around that age that children begin to understand abstract symbols such as written numbers (Zhou and Wang 2004) and to grasp that written words can have a stable meaning (Bialystok 2000; Apperly et al. 2004).

These developmental studies are telling because they show that the use and understanding of symbols can take different forms depending on the cognitive loads of the task and children's ability to process information about perspectives. They also show that there can be a significant gap between the comprehension and the production of symbolic artifacts.

### 3.6.3 *The cognitive foundations of perspective-taking*

Around the age of four, changes observed in children show how the brain can build on previous knowledge and skills to produce a whole array of

new behaviors. This behavioral change actually depends on one core ability: the capacity to represent different (and even conflicting) perspectives of an object (Perner et al. 2002). If the analogy with the developmental data holds, the emergence of artifacts such as the Blombos beads and engraved ochres could indicate that a similar cognitive change occurred during the MSA. Beads could come to symbolize social statuses (e.g. one's position within a kinship structure), because people would have been able to recognize the stability of its meaning across contexts and perspectives. The challenge is then to identify what cognitive mechanism is responsible for this new ability.

Many of the first psychologists to work on Theory of Mind (ToM) thought that the understanding of false beliefs emerged as a specific module in humans around the age of four, and was deficient in autistic children (Baron-Cohen et al. 1985; Baron-Cohen 1995). This view changed when it became clear that both apes and very young children understand intentions, although apes tend to use this information in competitive rather than in cooperative contexts (Hare et al. 2000, 2001; Tomasello et al. 2003; Lizskowski et al. 2006; Warneken and Tomasello 2006). Even in the case of false beliefs, Onishi and Baillargeon (2005) established that children as young as 15 months were expecting people to behave according to their own false beliefs and not to true beliefs that they should not have. These experiments have allowed psychologists to interpret the change in performance associated with the classical false-belief task as a consequence of a change in domain-general cognitive skills (Stone and Gerrans 2006; McKinnon and Moscovitch 2007).

An interesting hypothesis, in line with Coolidge and Wynn's (2004; Wynn and Coolidge 2004) scenario on cognitive evolution, would be that enhanced working memory could have enabled the invention and maintenance of symbolic culture, because creating and using symbolic artifacts implies the ability to hold different perspectives of an object in the human mind. This interpretation, however, is not the only possible one. Other executive functions, apart from enhanced working memory, play a role in the development of level-2 perspective-taking and ToM. Carlson and Moses (2001) showed that the capacity to inhibit a pre-potent response while activating a conflicting novel response was the best predictor of success in the ToM tasks. Planning ability was not significantly related to it when accounting for age and receptive vocabulary (Carlson et al. 2002, 2004).

The exact causal relationship between conflict inhibition and ToM, however, remains unclear (Perner and Lang 1999). Although conflict inhibition correctly predicts success in the false-beliefs task, three-year-olds do succeed in some tasks with seemingly similar executive demands (Perner 2000). It may be that such tasks pose specific problems to younger children because they are unable to appreciate that an object can have multiple identities (Perner et al. 2002). Recent neuroimaging studies have shown that the temporo-parietal junction was specifically involved in processing conflicting perspectives and false beliefs (Saxe and Kanwisher 2003; Aichhorn et al. 2006; Perner et al. 2006).

To summarize our point, the higher-level cognitive ability that is needed to invent and maintain symbolic culture is the capacity to simulate how objects look from conflicting viewpoints. Such a task depends heavily on working memory and inhibitory control, although the causal nexus still needs investigation. For the moment, it would be hasty to suggest a more fine-grained cognitive account. Nevertheless, we believe the framework proposed above can already shed considerable light on the emergence of behavioral modernity and the evolution of language.

### 3.6.4 *The diversity of modern behaviors*

To say that an object is symbolic, by definition, means that it stands for something else. Wynn and Coolidge (2007: 88) suggest that the beads found at Blombos but also at the Grotte des Pigeons at Taforalt (Morocco) (Bouzouggar et al. 2007), and we assume those from Oued Djebbana or Skhul (Vanhaeren et al. 2007), could also have stood for nothing at all. They contend the beads may only have had an aesthetic or decorative function that could be taken as evidence of modern intentionality, but they were not necessarily symbolic. Their argument, however, raises a puzzling epistemological question for archeology in general. For example, how do we establish whether the most complex forms of Upper Paleolithic art really stood for something else, a common assumption, and were not partly (or entirely) decorative? The theoretical limits of archeology to answer this question may be exposed, but this does not necessarily block our ability to understand the evolution of cognition and language. If our arguments presented earlier are correct, then the cognitive ability that enables the invention and maintenance of symbolic culture is the same one that enables humans to fully represent the perspective of others on

objects (including oneself). In brief, using an object as a symbol is not the same as using it for an aesthetic purpose, but the same cognitive ability comes into play in both cases.

Our case for the cognitive modernity of the Blombos people is further strengthened if one takes into consideration the other artifacts recovered with the beads such as the bifacial points, engraved ochres, and finely made “formal” bone tools. One defining feature of culture in modern humans is the cumulative aspect of transmission (Richerson and Boyd 2005). This has been described as a ratchet effect, permitting cumulative modifications to create ever more complex behaviors (Tomasello 1999; Alvard 2003). For such a process to exist, the innovative contribution of individuals is essential, although it may be difficult to identify in the archeological record (Henshilwood and d’Errico 2005).

The cognitive framework presented in this paper helps explain the profoundly innovative nature of the material culture of the MSA. The development of level-2 perspective-taking and ToM would have increased the pace of cultural transmission by allowing agents to reach a stable representation of the point of view of others while manufacturing various tools. This view is also supported by phylo- and ontogenetic data. Chimpanzees, during cultural transmission, tend to omit the movements that play no causal role in the attainment of the goal (Nagell et al. 1993; Horner and Whiten 2005). Human children, on the other hand, take pleasure in imitating even causally irrelevant sequences and their ability to do so is enhanced between the age of three and five (McGuigan et al. 2007). This tendency leads to the emergence in culture of a non-functional component whose impact on behavior can even be maladaptive (Richerson and Boyd 2005; Gergely and Csibra 2006).

The emergence of regional styles is one of the major changes in lithic industries during the MSA and suggests the introduction of a non-functional component (McBrearty and Brooks 2000; Wurz 2000; Barham 2002a). Linking style to behavioral modernity, however, has been criticized on the basis that it has no obvious symbolic function (Chase 2006). Our cognitive account explains how style can be an indicator of modernity even if it does not convey symbolic meaning. In our opinion style appears not only because it can mean something, but also because level-2 perspective-taking facilitates the diffusion of non-functional cultural components.

The term “formal” is ascribed to tools that result from complex manufacturing processes, during which, for example, bone, ivory, and antler are cut, carved, or polished to produce pieces like “projectile points, awls, punches, needles and so forth” (Klein 2000: 520). The manufacture of the MSA bone tools at Blombos fits within this definition (Henshilwood et al. 2001b: 666). Bifacial points arguably also fit within the formal tools category. The ratchet effect in cultural evolution could be responsible for the selection of formal manufacturing techniques, but there is an additional reason to include these tools within behavioral modernity. The development of level-2 perspective-taking and ToM does not only have an impact on cultural transmission, but also on categorization, one of the most basic cognitive mechanisms found in all vertebrates (Harnad 2005).

Children from a very young age understand that an object can belong to different categories. In the second year of their life, they understand that “dog” and “animal” can refer to the same object, although they know that the category “animal” is more extensive and includes also cats, squirrels, and so forth. There is a task, however, closely synchronized in development with ToM and spatial perspective, in which they cannot succeed. Children who lack complete ToM and level-2 perspective-taking are incapable of holding in mind a basic and a subordinate category (to look at an object simultaneously as a “dog” and as an “animal”) (Perner et al. 2002). Once again this implies that one is able to hold in mind two conflicting perspectives of an object. Our argument is that level-2 perspective-taking would be essential to produce formal tools, as it enables the craftsman to keep in mind the subordinate category to which the tool belongs during its production. In the case of the Blombos bone industry, the tools fall both within the basic category of “bone tools” and the subordinate category of “bone projectile points” or “bone awls” (Henshilwood et al. 2001b). Another example is the harpoons found at the Katanda sites in the Semliki Valley, Democratic Republic of the Congo, and dated at c. 90 ky (Yellen et al. 1995). The harpoons fall within the subordinate category of “barbed bone points,” the basic category of “bone points,” and the superordinate category of “bone tools.” The capacity to move across categories could have facilitated the reproduction in material culture of increasingly formalized types of tools. Our cognitive framework thus accounts not only for the common emergence of symbolic and aesthetic artifacts, but also for style and formalization in a henceforth cumulative material culture.

### 3.6.5 *What does it imply for the evolution of language?*

The framework presented in this paper does not give a central role to the evolution of language, but to a domain-general change that enabled level-2 perspective-taking. Does this mean that beads, engraved ochres, bifacial points, and finely made bone tools could be taken as evidence of modern cognition, but not of modern language? As many linguists have stressed, the Rubicon of modern language probably lies in recursion. Syntax is recursive if it enables clauses to be embedded within clauses (Jackendoff 1999; Hauser et al. 2002; Bickerton 2003). The faculty of language arguably possesses other specific traits—at the level of speech perception, phonology, or word-learning (Jackendoff and Pinker 2005; Pinker and Jackendoff 2005)—but none is as meaningful for cognition as recursion (Suddendorf and Corballis 2007). Imagine a language with a linear syntax, in which the meaning of a word changes with the position of the word in the sentence. The meaning of “Bob hit Fred,” for instance, would be different from the meaning of “Fred hit Bob.” Such a language would be insufficient to verbalize the kind of meta-representations associated with level-2 perspective-taking and ToM. Meta-representations have to be articulated in a hierarchical way by embedding clauses, as in sentences like: “Fred sees that I wear the beads” or “Fred knows that I am the chief.” Without recursive syntax, it is impossible to articulate conflicting perspectives. This is precisely what d’Errico and his colleagues have in mind when they argue that “syntactical language is the only means of communication bearing a built-in meta-language that permits creation and transmission of other symbolic codes” (d’Errico et al. 2005: 19).

The main reason to think that syntactic language comes with modern cognition is that recursion is essential to articulate the kind of meta-representations created by level-2 perspective-taking and ToM. If modern cognition does not come with modern language, this means that organizing recursion in language is more difficult than organizing it in cognition in general. This is an implausible thesis for at least three reasons.

First, it goes against ontogenetic data. Recursion in language does not only develop before level-2 perspective-taking and full-fledged theory of mind, but is even a good predictor of these abilities (de Villiers and Pyers 2002). There are reasons to think that language provides scaffolding for higher meta-representational ability (Clark 1998; de Villiers 2000, 2005).

Second, if modern cognition precedes modern language, it means that syntactic recursion has its own computational basis. This goes against the economic principle saying that the brain evolves by redeployment, that it makes multiple uses of the same structures, and that it avoids recreating structures anew (Anderson 2007). Moreover, many linguists and cognitive scientists have argued that recursion does not result from a specific computational process, but is rather the outcome of semiotic constraints on symbol use (Deacon 2003; Bouchard 2005), of enhanced domain-general cognition (Bickerton 2000; Coolidge and Wynn 2007), of an exaptation of an older recursive system (Hauser et al. 2002), or simply a convenient way to map multiple signals onto the linear structure of speech (Kirby 2000; Nowak et al. 2001).

Third, if modern cognition precedes modern language, one still needs to explain how this accommodates the genetic data concerning the evolution and dispersal of modern humans across the old world after c. 80 ky. All modern humans alive are able to learn recursive syntax. If the cognitive basis for recursion evolved after c. 80–60 ky one still needs to explain why it can be found in all human lineages today and what archeological and genetic data support the hypothesis of a late emergence of linguistic recursion. In the face of these difficulties, we think by far the most parsimonious hypothesis is that the MSA inhabitants at Blombos Cave, and probably all people during the Still Bay and Howiesons Poort periods in southern Africa, had fully modern language.

### 3.7 Conclusion

In years to come the debate on the origins of language may crucially depend on our capacity to explore in more detail the link between neuroscience and paleogenetics. For the moment, however, our knowledge remains insufficient to make confident connections between behavioral change and specific neural structures or genetic mutations. In the face of these limitations the most useful line of cooperation between archeologists and cognitive scientists is still to try to identify what cognitive abilities enable what behavior. In this chapter we propose that the Still Bay and Howiesons Poort phases of the MSA were dynamic periods of change in southern Africa and that at least some of the recovered material from a number of archeological sites in this region provides evidence for

modern cognition at c. 77–55 ky. In particular, we highlight the shell beads and engraved ochres from Blombos Cave as being examples of artifacts with clear symbolic meaning. A number of authors have previously suggested that the Blombos ochre pieces and the marine shell beads equate with information being stored outside of the human brain (e.g. Henshilwood and Marean 2003; d’Errico et al. 2005) and that the transmission and sharing of the symbolic meaning of these items must have depended on “syntactical” language (Henshilwood et al. 2004; d’Errico et al. 2005; Mellars 2005).

However, the link between these items and “syntactic” language has been too tenuous to permit a confident “leap of faith” (e.g. Donald 1998; Wynn and Coolidge 2007). Our response is to propose a framework to link at least some of this material culture with specific cognitive abilities. We argue that the capacity to represent how an object appears to another person (level-2 perspective-taking) enables the invention of symbolic artifacts like beads and engraved ochres, but also of other artifacts whose symbolic component remains contentious, such as bone tools, bifacial points, and engraved ostrich egg shell. Because recursion is essential to articulate in language the kind of meta-representations provided by level-2 perspective-taking and theory of mind, we predict that the presence of syntactic language can now confidently be “read” from some of the Blombos artifacts. Certainly our arguments can be equally applied to symbolic material culture from other regions and time periods in the rest of Africa, including the Still Bay and Howiesons Poort. The Blombos Cave finds are unlikely to represent the earliest example of human cognitive abilities or a *terminus post quem* for syntactic language; rather they represent one culmination of a long trajectory of increasing cultural complexity during the MSA in Africa. Exactly when or why syntactic language appeared in Africa and whether the source was single or multiple remains elusive, and is likely to remain so in the immediate future. We believe that cross-disciplinary model building through incremental observation is one way forward. By combining “hard” archeological evidence and cognition theory we show that human behavior in southern Africa was mediated by symbolism by at least 77 ky. A fundamental and integral component of this symbolically driven package was “syntactic” language.



# 4 Red ochre, body painting, and language: interpreting the Blombos ochre

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IAN WATTS

## 4.1 Introduction

Whereas language leaves no material trace, collective ritual—with its formal characteristics of amplified, stereotypical, redundant display—might be expected to leave a loud archeological signature. Does the archeological record of ochre use provide such a signature, and can it indirectly contribute to our understanding of the evolution of language?

I begin by highlighting the formal differences between language and ritual as modes of communication. Why, despite having opposed characteristics, is ritual widely regarded (Durkheim 1961; Rappaport 1999; Knight 1999) as establishing the social conditions for language? I then turn to the principal theories and inductive hypotheses that can be brought to bear on the interpretation of early (pre-45 ky) ochre use. In addition to being the first major theorist to posit a link between language and ritual, Durkheim drew attention to the role of body-painting in grounding the collective representations central to ritual action. Subsequent theoretical perspectives can be distributed along a spectrum. At one extreme is the innatist view that biology provides sufficient constraint to account for universal features of color labeling (Berlin and Kay 1969). Although this “Basic Colour Term” (BCT) theory is biological, it is not evolutionary and generates no predictions as to when pigments should be expected to emerge. It has, however, been used to predict the order in which different pigments should appear (Hovers et al. 2003). At the other

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extreme is the “Female Cosmetic Coalitions” (FCC) model (Knight et al. 1995; Power and Aiello 1997; Power this volume). This sets out from premises in human behavioral ecology, prioritizing the role of reproductive strategies in driving early pigment use and generating archeologically testable predictions. Between these two poles is the qualified innatism of Deacon (1997: 119), who treats the evolution of BCTs as subject to constraints from both neurophysiology and “pragmatic constraints of human uses.” Deacon’s model specifies a ritual and a temporal context, but is indistinguishable from BCT theory with respect to the sequence in which terms should arise. Finally, challenging the presumption that ochre was a pigment, several utilitarian hypotheses have been proposed (Klein 1995; Wadley et al. 2004; Wadley 2005a). I evaluate these perspectives and their implications in the light of a survey of early potential pigments and my research on the Blombos Cave ochre assemblage.

## 4.2 Context

Our species evolved in Africa sometime between  $\sim 150$  ky and  $\sim 200$  ky (Ingman et al. 2000; McDougal et al. 2005). Shell beads and geometric engravings on red ochre (d’Errico et al. 2005; Bouzouggar et al. 2007; Henshilwood et al. in press) indicate that symbolic traditions were present in Africa by the time a small subgroup of *Homo sapiens* migrated beyond the continent, between  $\sim 80$  ky and  $\sim 60$  ky (Oppenheimer 2003; Mellars 2006). All of this occurs within a technological stage known in Sub-Saharan Africa as the Middle Stone Age (MSA), a prepared core technology that evolved out of the Acheulian around 300 ky, and lasted until  $\sim 25$  ky in southern Africa (Clark 1997),  $\sim 45$  ky in eastern Africa (Ambrose 1998). The beads and engravings double the conventionally accepted antiquity of symbolic traditions, previously regarded as restricted to the Upper Paleolithic (Eurasia) and Later Stone Age (Africa). These findings have been used as a proxy for language (e.g. Henshilwood and Marean 2003: 636; Henshilwood et al. 2004: 404; Mithen 2005: 250; but see Botha, this volume). In most Middle Stone Age contexts, the only recurrent artifact class other than stone tools is red ochre.

Archeologists commonly use “ochre” as a generic term for any rock, earth, or mineral producing a reddish or yellowish streak when abraded, attributable respectively to hematite (an iron oxide) or one of

the iron hydroxides (typically goethite). Ethnographically and archeologically, red ochre is the most widely reported earth pigment. Use of ochre and other potential earth pigments such as black manganese is not restricted to *Homo sapiens*. The field may, therefore, provide comparative insights on the signaling strategies of closely related species (cf. d’Errico 2003).

### 4.3 Language and collective ritual

In the animal world, signals vary according to whether they encounter strong or weak resistance. High resistance from receivers prompts costly multimedia display; by contrast, low resistance permits low-cost “conspiratorial whispering” (Krebs and Dawkins 1984). Translating this general principle into the domain of human social communication, ritual (“costly signaling”) and language (“conspiratorial whispering”) have the expected diametrically opposed formal characteristics (Knight 1998, 1999: table 12.1), summarized in Table 4.1. With resistance minimal, language evolves to be conventionally coded, low cost, generally of low amplitude, digitally processed, interpersonal, focused on underlying intentions, and allowing for potentially infinite creativity. Being cheap and intrinsically unreliable, words are unable to signal social commitment (Rappaport 1999). Language leaves no direct archeological trace (Botha this volume), and is biologically unprecedented (Chomsky 2002). Designed to overcome high levels of resistance and to cement social contracts, ritual signals are multimedia indexical displays, costly to produce, of high amplitude and redundancy, evaluated on an

TABLE 4.1 *Signals: speech versus ritual (adapted from Knight 1999: fig. 12.1).*

Speech	Ritual
Cheap signals	Costly signals
Conventionally coded	Iconic & indexical
Low amplitude	High amplitude
Digitally processed	Analog scale evaluation
Productivity/Creativity	Repetition/Redundancy
Interpersonal	Group-on-group
Focus on underlying intentions	Focus on body boundaries and surfaces

analog scale and focused on body boundaries and surfaces (Sperber 1975; Rappaport 1979: 173–246, 1999). These features suggest that collective ritual might leave a loud archeological signature. Unlike language, human ritual has clear evolutionary precedents in the ritualized displays of other animals (e.g. Laughlin and McManus 1979; Maynard Smith and Harper 2003).

Although ritual and language represent opposite extremes, a long theoretical tradition holds that they are mutually interdependent. This position holds that collective ritual created the supportive framework for contractual understandings and associated symbolic communication between group members to become established (Durkheim 1961; Turner 1967: 93–111; Rappaport 1979, 1999; Gellner 1988; Maynard Smith and Szathmáry 1995: 272–273; Searle 1996; Deacon 1997: 402–407). The central function of ritual is to create the intensity of in-group trust necessary for symbolic communication to be possible (Knight 1998). Deacon (1997: 402) adds that ritual facilitates the transition from concrete sign–object associations (indices and icons) to abstract sign–sign associations. Alcorta and Sosis (2005) explain that it is the costliness of ritual that enables it to demonstrate commitment and deter freeriders.

#### 4.4 A prediction ahead of its time

The basic premise concerning the role of ritual in sustaining symbolic culture can be traced back to Durkheim. Less well known, Durkheim also proposed that “the first form of art” consisted of geometric designs painted on sacred objects and on the bodies of ritual performers (1961: 149 fn.150, see also pp. 148, 264–265, 417), these designs bearing witness to the participants’ “moral unity” (1961: 432). Typically, according to Durkheim (1961: 159–161), such designs would be executed in red ochre, a substance of “equal importance, religiously” as blood. While drawing heavily on Aboriginal Australian ethnography, Durkheim advanced the following arguments on the basis of general theoretical considerations:

- The “emblems” of collective representations had to be abstract because the representations concerned “social facts”—things that have no real-world likenesses but exist only by virtue of collective agreement (1961: 236).

- Such emblems had to appear first on the body because collective ritual is a bodily display of participation “in the same moral life” (1961: 264–265).
- Red ochre inevitably symbolized blood, this substance being reified to a “sacred principle” (1961: 159–61).<sup>1</sup>

#### 4.5 Innatist theory

The theory of “basic color terms” (e.g. Berlin and Kay 1969) has come to exemplify the more general proposition that perceptually grounded semantic categorization is a direct projection of innate cognitive universals, structured by hard-wired neural mechanisms. This view, endorsed by some linguists (Landau and Jackendoff 1993), is associated with cognitive psychology (Fodor 1975; Pinker 1994) and evolutionary psychology (Tooby and Cosmides 1992). While a strong form of innatism is found in some “basic color term” (BCT) literature (e.g. Berlin and Kay 1969: 109; Kay and McDaniel 1978: 611; Kay and Berlin 1997: 201), elsewhere the more limited claim is that “the semantics of basic color words . . . is partially constrained by parameters of the visual system” (Kay et al. 1991: 24; see also Kay in Ross 2004).

A tacit assumption of BCT theory has been that the color domain is a natural and universal semantic field of such salience that all languages will have dedicated terms exhaustively partitioning the domain (cf. Kay 1999: 76). Among the original criteria for BCT status (Berlin and Kay 1969: 6) were that terms should be monolexemic (excluding referent-based similes for hue, e.g. “blood”) and that application should not be restricted to a narrow class of objects (as in the case of “blond”). The principal cross-cultural findings informing the theory were:

- All languages have between two and eleven BCTs.
- There are high levels of agreement within and between cultures as to the focal points of the extremes of the achromatic scale (black and white) and the four unique hues of red, yellow, green, blue (where languages have BCTs in the appropriate hue area).
- The foci of BCT terms can be predicted from their number.

<sup>1</sup> The argument as to why blood is reified to a sacred principle is minimally developed in *Elementary Forms* (1961: 161 and footnote 50), the reader being referred to an earlier paper (Durkheim 1898) in which menstruation plays the central role.

The last finding led to the hypothesis of an implicational scale of seven cultural evolutionary stages (Berlin and Kay 1969: 4). Stage I comprised Black versus White, followed by Red (Stage II), followed by either Green or Yellow (Stages IIIa and IIIb). In outlining this successive encoding of new foci, labels were used loosely, sometimes referring to category foci (e.g. pure black), sometimes to the foci plus its extension (e.g. pure black and all dark hues). In a revised formulation (Kay and McDaniel 1978), the initial BCTs comprise two composite categories with several potential foci, successively fractioned out in subsequent stages. Stage I comprised “light/warm” versus “dark/cool” terms (respectively focused on white or red or yellow versus black or green or blue). The new composite of Stage II was “warm” (focused on red or yellow). The vague biological explanation of Berlin and Kay (1969: 109) was reformulated such that six “fundamental neural response categories” (FNRs) of black, white, red, yellow, blue, and green are “encoded as the universal semantic categories” (Kay and McDaniel 1978: 625, 627). FNRs were the postulated output of the achromatic vision provided by rod cells (cf. Kandel et al. 2000: 507–513), and of trichromatic vision—where signals from three types of cone cell are pitted against one another (opponent processing) to derive a difference signal, enabling the four unique hues to be discriminated from the wavelength continuum (De Valois and De Valois 1993; Abramov 1997; but see Jameson and D’Andrade 1997). Trichromatic vision evolved with Old World monkeys (Jacobs 2002).

How far back in time are these posited stages projected? Stage I terms could potentially date to when vocabularies were of a size comparable to “the repertoire of discreet [sic.] verbal signs used by apes and monkeys” (Berlin and Kay 1969: 16). However, contrary to the impression given in some commentaries (e.g. Hovers et al. 2003: 493), a biological mechanism accounting for the order in which BCTs are labeled (as distinct from the category foci) forms no part of the original theory (Berlin and Kay 1969: 17). Despite red being invariably labeled before other hues, red and yellow are treated as equally plausible potential foci of “light/warm” and “warm” terms (Kay and McDaniel 1978: fig. 13; Kay and Berlin 1997: 201). Sahlin (1976: 3–8) outlined possible mechanisms and biases underpinning the “natural-perceptual logic” to the emergence of BCTs, while arguing persuasively that the terms are “codes of social, economic, and ritual value” (1976: 8), not labels for natural categories.

The ambiguous classificatory status of red in any binary lexicon is evident in Berlin and Kay's (1969) discussion of Stage I languages. Both their examples concerned New-Guinea Highland cultures of the Danian language group. Among the Jalé, "the appearance of blood is *sin* 'BLACK,' exactly as 'blood (red)' should be at Stage I because of its low brightness" (1969: 24, citing a seminar presentation by K. F. Koch). However, their second source (Bromley 1967) reported that related languages divided colors into "brilliant" and "dull," the "brilliant" category including most reds, yellow, and white (Berlin and Kay 1969: 24). The 1978 revision of Stage I categories largely arose from Heider's (1972) research with the Dugum Dani. Presented with a saturated array of Munsell color chips, the best exemplar of the "light/warm" term selected by her informants ( $n = 40$ ) was not white but dark red (selected by 69%), followed by light pink (most informants selecting pink already having a term for red that denoted a red clay pigment).

One of the few other published studies of an arguably Stage I language concerns the Gidjängali of Australia (Jones and Meehan 1978). The Gidjängali "light" term—*gungaltja*—"refers to light, brilliant and white colors, and also to highly saturated red" (1978: 27). The authors emphasized the element of "brilliance" or "animation" in the *gungaltja* concept. Asked to identify examples of this term among saturated Munsell chips, their principal informant responded that there were no proper *gungaltja* colors there, pointing instead to some silver foil. Subordinate to this universal binary classification, the color of a restricted range of objects or states could be described using the terms for four pigments (pipe-clay, yellow ochre, red ochre, and charcoal), constituting the four ritually recognized colors. Jones and Meehan considered that *djunu* (red ochre) was the color that excited most interest (1978: 31). The best Gidjängali exemplars of this term were two types of ochre, with Munsell hues of Purple and Red-Purple, both decidedly dark (brightness levels 4 and 3). The darker type was "a high grade haematite with a lustrous purple streak" which, when burnished on objects, gave "a metallic sheen" (Jones and Meehan 1978: 32). These ritually defined and recognized colors are at least as salient to color lexicalization as the two Gidjängali BCTs.

Both Jones and Meehan (1978: 27) and Heider (1972: 464) noted that red's inclusion in the "light" or "light/warm" term was paradoxical given its low brightness (cf. Solso 1994: fig. 1.7). Heider speculated that the original meaning of the term glossed as "light/warm" was centered on "warm" dark

colors (i.e. red). Her findings led Berlin and Berlin (1975: 84) to conclude that the foci for the two primordial categories are “fluid and unstable,” and to speculate that red might be the principal focus of “light/warm” terms, with black dominating “dark/cool” terms. Neither speculation has been pursued in subsequent BCT-inspired research, but one would not predict specifically dark red to be exemplary in a red-versus-black opposition.

Red plays a more prominent role in well-documented Stage I languages than is conveyed by the BCT glosses. Factors contributing to its exclusion as a BCT may include active nominal reference and restricted usage; but, given the critical role of metaphor in the evolution of language (Deutscher 2005), nominal reference is likely to provide clues as to the domain in which color lexicons arose. The most common analyzable root of any BCT or referent-based simile for hue is “blood” (Greenberg 1963: 134; Berlin and Kay 1969: 38; Nash 2001; Everett 2005: 627; Deutscher 2005: 237), probably followed by “red earth pigment” (see above; Koch in Berlin and Kay 1969: 23). Analyzable “black” and “white” terms (e.g. Berlin and Kay 1969: 38–39, citing Rivers 1901; Levinson 2000: 10; Everett 2005: 627) also often refer to things with partible color (e.g. cockatoo feathers, charcoal, cuttlefish ink). Important exceptions to this tendency are “black” and “white” terms such as “night,” “darkness,” or “to see,” taking us beyond the labeling of surface color. Such exceptions notwithstanding, ritual display would appear to be deeply implicated in simple forms of color lexicalization.

Paul Kay himself now grants that there is no physiological evidence for or against neural processing determining BCTs (Ross 2004). While there are constraints from visual perception, it seems that perception can itself be biased by linguistic categorization (Kay and Kempton 1984; Davidoff et al. 1999; Levinson 2000, 2003). Some cultures with simple classificatory systems have no universal partitioning of the color domain, no composite color terms, and referent-based similes for hue may circumscribe BCTs (Levinson 2000). In societies with simple coloring technology, other aspects of surface appearance such as brightness/dullness, freshness/dryness, brilliance, or pattern may be at least as salient as hue (e.g. Conklin 1955; MacLaury 1992; Lyons 1995; Casson 1997; Lucy 1997; Levinson 2000). Addressing some of these challenges, BCT theory has been further revised (Kay and Maffi 1999), but as Levinson (2000, 2003) points out, the revision is incompatible with the innatist claim that universal perceptual constraints directly determine semantic universals (Kay and McDaniel 1978: 610; Durham 1991: 281; Shepard 1992: 522; Pinker 1994: 63; Hovers et al. 2003: 493).



#### 4.5.1 *Archeological application of BCT theory*

Using Berlin and Kay's (1969) original formulation, Hovers and colleagues (2003: 493) attempt to apply BCT theory to archeological data.<sup>2</sup> One would predict on this basis that the earliest pigments should be black and white, followed by red, and then yellow. They claim: "red and black pigments are relatively ubiquitous in Paleolithic . . . sites, from the Plio/Pleistocene to Upper Paleolithic" (2003: 491). Archeologists are urged to re-examine existing collections for black and white pigments, as their presence would be "in line with linguistic studies of color terms" and the "infrastructure of trichromatic vision" (2003: 518).<sup>3</sup> If, instead, Kay and McDaniel's (1978) formulation had been used, there is no theoretically grounded predictable order in which red, black, yellow, and white (the most common earth pigments) should appear, since all are potential foci of Stage I composite terms.

#### 4.6 Qualified innatism

Deacon (1997) accepts the BCT hypothesis concerning the stages of color lexicalization; he, too, presents the unrevised stages (1997: 117).<sup>4</sup> However, challenging innatism, he argues (1997: 119) that the process by which shared—perceptually based—semantic categories emerge is determined both by hard-wiring and "the pragmatic constraints of human uses." He goes on to make a more general argument, positing the demands of *ritual action* as the earliest pragmatic constraint in "symbol discovery" (1997: 402). Specifically, Deacon argues for the primacy of rituals cementing sexual contracts, arguing that these extend back ~2 my (1997: 384–401). He concludes: "Out of the ritual processes for constructing social symbolic relationships, symptoms of the process itself (exchanged objects, body markings, etc.) can be invested with symbolic reference" (1997: 406).

Discussing a probable association of red ochre with early *Homo sapiens* burials at Qafzeh (Palestine), ~92 ky, Hovers and colleagues (2003: 508-509) invoke Deacon's argument about the role of ritual in constructing symbols (his critique of innatism goes unremarked). No

<sup>2</sup> Although Kay and McDaniel (1997) and MacLaury (1992) are cited, Hovers and colleagues do not refer to revised BCT stages.

<sup>3</sup> Trichromatic vision does not concern achromatic perception.

<sup>4</sup> Deacon's account incorrectly states that the simplest classifications comprise three terms and that green necessarily follows the labeling of red.

ritual context is proposed for any other early pigment occurrences. They conclude (2003: 509) that the record of early use of red and black pigments (purportedly extending back  $\sim 2$  my) accords with Deacon's claim for early beginnings to the gradual co-evolution of the brain and symbolism. However, they continue, "normative social constructs" can be inferred only when co-associations of the kind argued for at Qafzeh occur. The use of Deacon to theoretically justify a focus on ritual represents a welcome development in archeological discussion of early pigment use and symbolism in general. However, uncritical adherence to BCT theory precludes the possibility of ritual displays themselves influencing pigment choice.

#### 4.7 The "Female Cosmetic Coalitions" model

The FCC model (Knight et al. 1995; Power and Aiello 1997; Power 1999; Power this volume) has much in common with Deacon's model of the origins of symbolic culture. Both approaches stress conflicting male-versus-female reproductive interests in the context of encephalization, maternal energy budgets, and access to meat and mating opportunities. Both identify ritual as the basic mechanism for resolving these conflicts. Finally, both agree that as brain size increased with increasing group size, females had to bear the costs of producing and maintaining increasingly slow-maturing, energetically demanding babies. While Deacon envisages wedding ceremonies stretching back to the Plio-Pleistocene, the FCC model envisages initiation rituals of much more recent date. According to this model, pressure to reward investor males at the expense of philanderers favored concealed ovulation, extended receptivity, and enhanced capacities for ovulatory and menstrual synchrony. With signals of ovulation phased out, menstruation was left salient as a signal of imminent fertility. Males are expected to compete to bond with females perceived to be cycling, doing so at the expense of current partners who are pregnant or nursing. Females threatened by corresponding loss of male investment should respond by scrambling the signal. Building on standard explanations for ovulation concealment (Alexander and Noonan 1979; Hrdy 1981; Sillén-Tullberg and Møller 1993), a similar logic is applied to menstruation. How might females scramble the information divulged by this biological signal? Artificial pigments suggest one possibility (Plates 6–8). In this scenario, menstrual onset prompts pregnant/lactating females to paint

up as “imminently fertile” on the model of their cycling relatives. This leads to the following archeological predictions concerning pigment use:

- The initial focus should be on red rather than black, white, or yellow.
- Pigment use should not predate the marked increase in encephalization that begins in the middle of the Middle Pleistocene (between ~400 ky and ~550 ky, Ruff et al. 1997; Rosenberg et al. 2006). It should predate the achievement of modern encephalization quotients, between ~200 ky and ~100 ky (De Miguel and Henneberg 2001).
- Within this time-window (c. 500 to 150 ky), there should be a shift from irregular to regular use of red cosmetics (accompanied by rapid spread of such usage) as an initially context-dependent “sham menstruation” strategy was raised to the level of a regular monthly ceremony, performed whether or not a menstruant was present.
- Coalitions living in areas lacking blood-red earth pigments would be expected to incur heavy costs to procure them from elsewhere.

#### 4.8 Utilitarian hypotheses

Challenging the presumption of use as pigment, some archeologists have suggested alternative general explanations for early ochre use—foremost being the hypothesis that ochre was used as a tanning agent and/or as a functional ingredient in cements for hafting stone tools (Klein 1995; Wadley et al. 2004; Wadley 2005a).

The tanning hypothesis arises from a misunderstanding of basic chemistry, where the properties of certain soluble iron salts (e.g. iron sulphate) have been assumed to be shared by relatively insoluble iron oxides (e.g. Keeley 1980: 172; Knight et al. 1995: 88; Wadley et al. 2004: 662; all citing Mandl’s [1961] experiments with metal salt solutions). Iron salts have been used as tanning agents (Tonigold et al. 1990), but no ethnographic or leather industry sources confirm similar use of iron oxides.<sup>5</sup> This hypothesis can therefore be dismissed.

<sup>5</sup> The two claimed ethnographic precedents for use of ochre as a tanning agent (cf. Wadley et al. 2004: 662; Wadley 2005a: 589; Audouin and Plisson 1982: 57) do not bear scrutiny. Steinmann’s (1906: 78) inference is contradicted by more detailed observations on Tehuelche hide working (Cooper 1946: 148, with refs.). Sollas (1924: 275) made no functional claim; his uncredited primary source (Mathews 1907: 35) simply stated that the mixture of ochre and grease made garments water resistant. The claimed experimental support (Wadley et al. 2004: 662; Wadley 2005a: 589, citing Audouin and Plisson 1982) can be more parsimoniously accounted for by the desiccating action of red ochre (Phillibert 1994: 450).

The hafting-cement hypothesis is consistent with archeological reports from relatively late (post-80 ky) MSA assemblages, where ochre residues on stone tools were predominantly restricted to parts which would have been in a haft (Lombard 2007 with references). Replication experiments (Allain and Rigaud 1986; Wadley 2005a) confirmed that the inclusion of either yellow ochre or hematite in resin-based cements made them more manageable during use, helped drying and hardening, and made them less brittle.

However, no property of ochre has been identified that might make it preferable to the wide range of ethnographically documented filler/loading agents, most of which would be easier to procure and process. Australian accounts mention the use of plant fiber, dung, calcined powdered shell, powdered charcoal, dirt, sand, and ochreous dust (Dickson 1981: 67–69, 164; Helms 1892–6: 274, 280). The primary requirement appears to have been for substances that were desiccant but otherwise inert.<sup>6</sup>

Presenting this as a plausible general account even for early large MSA “pigment” assemblages such as Twin Rivers (where 60 kg of pigment is estimated to have been recovered in the original excavations—Barham 2002b), Wadley (2005a: 599) has suggested that such assemblages might resemble the material used in her hafting experiments. This comprised 3 kg of ironstone nodules, only the weathered cortices of which were pigmentaceous. Consequently, seven hours’ “vigorous” grinding (Wadley 2005b: 5) exhausted the nodules, but produced just 70 ml of powder (enough to haft 28 tools). However, available evidence is that MSA assemblages overwhelmingly comprise homogenously pigmentaceous material (Watts 1998; Barham 2002b; and see below).<sup>7</sup> Additionally, if 2.5 ml of powder (representing 15 minutes’ work) was required to haft one tool, one would not predict pieces of ochre with solitary, small grinding facets. That both yellow ochre and hematite were experimentally successful implies that the hypothesis is null with respect to the hue and chroma of raw materials. At present, ochre in hafting cements is more parsimo-

<sup>6</sup> Burnt shell may additionally have served as a polymerising agent (Dickson 1981: 70).

<sup>7</sup> Twin Rivers is currently the best described MSA pigment assemblage; there is nothing in Barham’s (2000, 2002) accounts indicating non pigmentaceous associated material. In Watts’ examination of over 4,000 pigments from 11 southern African MSA assemblages, only three pieces are reported as predominantly non pigmentaceous material (Watts 1998: plates 5.1, 6.81 and tbl. 6.46).

niously interpreted in terms of symbolic considerations determining functional choices.

#### 4.9 Early pigment occurrences: differences between African and Eurasian hominins

There are two claims (Leakey 1958: 1100; Beaumont and Vogel 2006: 222) and one suggestion (Clark and Kurashina 1979) for ochre use in the Lower Pleistocene (790 ky to 1.8 mya) and early Middle Pleistocene (c. 500 ky to 790 ky), but these are not compelling.<sup>8</sup> Middle Pleistocene (130 ky to 790 ky) occurrences are listed in Table 4.2. Current evidence suggests initial use in the middle of the Middle Pleistocene, between ~300 ky and ~500 ky (Howell 1966; de Lumley 1969; Barham 2002b; Tryon and McBrearty 2002; Brooks 2006a; Beaumont and Vogel 2006). However, only one of these early occurrences (Barham 2002b) has been adequately published, and doubts have been raised whether the material at two of the European sites was pigment (Butzer 1980 re. Ambrona; Wreschner 1983, 1985 re. Terra Amata). A stronger case for initial European use can be made at ~250 ky (Thévenin 1976: 984; Marshack 1981).

Initial occurrences of red ochre may be broadly coeval, but European and African records for the later Middle Pleistocene and earlier Late Pleistocene differ dramatically. For Middle Pleistocene Europe, there are at most five occurrences, three of which are questionable. All are thought to predate 220 ky, and are followed by a find gap of at least 100,000 years (Wreschner 1985: 389). Even after this gap, I know of only two cases from the earlier Late Pleistocene, between 128 ky and 75 ky (Demars 1992 re. Combe Grenal layers 57/8; Marshack 1976 re. Tata). The great majority of the 40 or so European Mousterian sites with pigment date to the Last Glacial (beginning 74 ky), most post-date 60 ky, and manganese predominates over red ochre (Demars 1992; d'Errico and Soressi 2006: 86). Forty is a small proportion of

<sup>8</sup> Citing Mary Leakey (Leakey 1971), Dickson (1990: 42–43) states that the two pieces of red ochre (subsequently identified as rubified tuff) from Olduvai Bed II at site BK “show signs of having been struck . . . by hammerstone blows”. The basalt manuports at the Lower Pleistocene site of Gadeb, Ethiopia (Clark and Kurashina 1979) showed no signs of use and pigmentaceous material was weathered cortex. Beaumont and Vogel (2006) claim that hematite use at Wonderwerk extends to the initial Middle Pleistocene; however, the hematite is thought to derive from the cave host rock and no use wear is reported, so the claim remains to be substantiated.

TABLE 4.2. *Middle Pleistocene potential pigment occurrences.*

Site	Country	Unit	Approximate age	Technological association	Pigment	References (dating references in parentheses)
<b>Europe</b>						
Terra Amata	France		380 ± 80 ky (ESR) or 214 & 244 ky (TL)	Acheulian	Questionable	de Lumley 1966, 1969; Wreschner 1983, 1985 (Falguères et al. 1991; Wintle & Aitken 1977)
Ambrona	Spain		>350 ky	Early Acheulian	Questionable	Howell 1966; Butzer 1980 (Pérez Gonzáles et al. 2001)
Maastricht Belvedeer	Holland	Site C, Unit 4	c. 250 ky	Middle Paleolithic	Probable	Roebroeks 1988
Achenheim	France	Middle Loess (lvl 19)	c. 250 ky	Middle Paleolithic	Good	Thévenin 1976; Wernert 1952  (Buraczysky & Butrym 1984)
Bečov 1A	Czech Rep.		c. 222 ky?	Middle Paleolithic	Good	Fridrich 1976, 1982; Marshack 1981
<b>India</b>						
Hunsgi	southern India		c. 200-300 ky	Acheulian	Good	Bednarik 1990

(Continued)

TABLE 4.2. (Continued).

Site	Country	Unit	Approximate age	Technological association	Pigment	References (dating references in parentheses)
<b>Africa</b>						
Sai Island	Sudan	BLG/TLG gravel	c. 200 ky?	‘Lower’ Sangoan	Good	van Peer et al. 2004
		RS sand	152 ± 10 ky, 182 ± 20	‘Middle’ Sangoan	Good	
Olorgesailie† Kapthurin (GnJh 15)	Kenya	B OK 1	>340 ky, <493 ky	Post Acheulian	Probable	Brooks 2006a
	Kenya	K3 Sedi ments	>285 ky	Fauresmith/MSA	Good	McBrearty 2001 (Tryon & McBrearty 2002)
Mumba^	Tanzania	Stratum VIB	132 ky	Sanzako (MSA)	Probable	Mehlman 1979:91 (Mehlman 1991)
Twin Rivers	Zambia	A Block	266 ky to >400 ky (?)	Early Lupemban	Good	Barham 2002
		F Block	140 ky to 200 ky	Early Lupemban	Good	
Mumbwa	Zambia	Unit X	>172 ± 22 ky	MSA	Good	Barham 2000
Kabwe	Zambia		c. 300 400 ky ?	Charama?	Probable	Clark 1950 (Barham et al. 2002)
Kalambo Falls*	Zambia			Lupemban (Sis zya)	Probable	Clark 1974
Pomongwe	Zimbabwe	Area 1, lysrs 22 27		Charama (MSA)	Good	Cook 1963, 1966; Watts 1998
Bambata	Zimbabwe	Lower Cave Earth		Charama (MSA)	Good	Jones 1940:17 (cf. Cook 1966 re. Charama)

Wonderwerk	S.A. (Northern Cape)	Major Unit 7	c. 790 ky	Acheulian	Questionable	Beaumont & Vogel 2006
		Major Units 3 4	$276 \pm 29$ , >350 ky	Fauresmith	Probable	
Kanteen Koppie	S.A. (Northern Cape)	Stratum 2a		Fauresmith	Probable	Beaumont 2004
Nooitgedacht 2	S.A. (Northern Cape)			Fauresmith	Good	Beaumont 1992a
Pniel 6	S.A. (Northern Cape)	Stratum 3		MSA/Fauresmith?	Good	Beaumont 1992b, Watts 1998
Kathu Pan 1	S.A. (Northern Cape)	Stratum 4a		Fauresmith	Probable	Beaumont 1992c
		Stratum 4b		Acheulian	Questionable	Beaumont & Vogel 2006¶
Kathu Townlands 1	S.A. (Northern Cape)			Acheulian	Questionable	Beaumont & Vogel 2006¶
Duinefontein 2	S.A. (Western Cape)		>270 ky, <290 ky	Late Acheulian	Good	Cruz Uribe et al. 2003

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(Continued)



TABLE 4.2. (*Continued*).

Site	Country	Unit	Approximate age	Technological association	Pigment	References (dating references in parentheses)
Border Cave	S.A. (Kwa	6BS	>227 ky	MSA	Good	Watts 1998 (Grün & Beaumont 2001)
	Zulu	5WA	$227 \pm 11$ , $174 \pm 9$	MSA	Good	
	Natal)	5BS	$166 \pm 6$ , $147 \pm 6$	MSA	Good	
Pinnacle Point 13B	S.A. (Western Cape)	LC MSA Lower	164 ky	MSA	Good	Marean et al. 2007
Blombos Cave	S.A. (Western Cape)	Layers CL CP	>143 ky	MSA	Good	Watts this paper (Jacobs et al. 2006)

† Pending geochemical analysis, the artifactual status of the ochre at Olorgesallie remains indeterminate (Brooks pers. comm.)

^ The traits of the Sanzako industry suggest that the U series date on bone (from approximately the same level as the oldest ochre), may be a minimum age.

\* The Siszya Lupemban is only assigned a Middle Pleistocene age on the basis of comparison with Twin Rivers

¶ The authors cite Beaumont & Morris 1992 for this claim, but the relevant paper (Beaumont 1990c) does not mention pigment in stratum 4b at Kathu Pan or in the Townlands site

excavated Mousterian sites. It is not until the arrival of modern humans (~40 ky) that pigment use in Europe becomes ubiquitous, when it overwhelmingly takes the form of red ochre. The last (Châtelperronian) Neanderthals, living alongside the newcomers, also start using much larger quantities of red ochre (Harrold 1989: 696; Couraud 1991).

In Africa, it is estimated that the number of excavated MSA sites is only a tenth of the European Mousterian ones (McBrearty and Brooks 2000: 531). Despite the less intensive history of research, excavation units from at least 18 sites dated or believed to date to the Middle Pleistocene (three to six times the number of European sites) have provided probable earth pigments. Most of the earliest occurrences span the transition from the Acheulian to the MSA. Contrary to some authors (Wadley 2005b: 2; d'Errico et al. 2003: 4), Barham's Twin Rivers excavation did not provide evidence for use of a wide range of colors. Use-wear was restricted to nine pieces of specularite (laminar crystalline hematite) and a piece of pedogenic, earthy "hematite" (Barham 2002b: table 1). The specularite is thought to have come from further afield than the hematite; it produced "a darker, purple shade of red (Munsell 10R 4–3/3–3) that sparkles" (Barham 2002b: 185). Of the 302 potential pigments (1,617 g), 93.1% were red (92.4% by weight), these being overwhelmingly specularite. None of the yellow limonite was utilized, although a limonite "crayon" is reported from the original F Block excavations (Clark and Brown 2001: fig. 20, no. 23). Barham treats the tiny amount of manganese as introduced to the site, but this could readily have come from autochthonous concretions (Barham 2002b: fig. 3). The only well-supported case for Middle Pleistocene black pigment is a small fragment of graphite associated with "Middle" Sangoan material at Sai Island. This site is also unique among both Middle and earlier Late Pleistocene assemblages in that yellow predominates over red. All other Middle Pleistocene reports exclusively concern red ochre in one form or another.

Pigment use is not ubiquitous in the early MSA, nor is it necessarily a regular behavior in the early assemblages where it is documented. For example, at Kalambo Falls in Zambia it is absent in the early Lupemban but present in the later Lupemban (Clark 1974: table 10). In the long cave sequences of Mumba, Pomongwe, and Bambata, it is rare in the basal assemblages, becoming more frequent in overlying layers. In South Africa, it is absent in the large, early (undated) MSA assemblages at Peers Cave and Cave of Hearths, but is a recurrent feature of overlying Late

Pleistocene MSA layers (Volman 1981: 325; Mason 1957: 135; pers. obs. regarding Peers Cave Late Pleistocene).

Currently the most informative South African site for this period is Border Cave (Watts 1998). Figure 4.1 shows the relative frequency of pigment for the first five stratigraphic aggregates. The basal unit (6BS) is >227 ky (Grün and Beaumont 2001); a sample of almost 10,000 lithics provided just one piece of ochre, on the threshold of archeological visibility. The overlying unit, with a similarly sized lithic sample, provided just three pigments. The youngest of the Middle Pleistocene units (5BS) provided inverted dates of 166 ky and 147 ky, placing it in the middle of the penultimate glacial. This witnesses a fivefold increase in relative frequency, with overlying Late Pleistocene samples providing comparable percentages. At this site, use of red ochre only became regular between ~170 ky and ~150 ky.

Pinnacle Point (approximately 85 km east of Blombos) confirms regular use of red ochre from ~164 ky (Marean et al. 2007). There is suggestive evidence, therefore, that red ochre use in southern Africa only became habitual and ubiquitous to cave/rockshelter occupations with the spread of *Homo sapiens*. It remains so thereafter (Watts 1999). In the Late Pleistocene MSA of southern Africa, non-red pigments are rare, with black, white, and yellow largely restricted to a few Still Bay and Howiesons Poort contexts (~75 ky to ~60 ky) (Watts 2002: 10–11).

To conclude, archeology provides no support for revised or unrevised versions of BCT Stage I, for Deacon's proposed Plio-Pleistocene weddings, or for use of red and black pigment (“ubiquitous” or otherwise) extending back to the Plio-Pleistocene. The African Middle Pleistocene record is probably of greater antiquity than non-African counterparts, is certainly much more extensive, and—unlike the European record—is continuous. The habitual use of red ochre can be considered a species-defining trait. Occasional use may have occurred among all post *Homo erectus/ergaster* lineages, but it is no longer acceptable to suggest that the pigment record of Neanderthals and their immediate ancestors is comparable to that of early *Homo sapiens* and their immediate ancestors (e.g. Klein 1995: 189). Having discounted the two principal utilitarian hypotheses as alternative general explanations, it is the habitual nature of the behavior from the end of the Middle Pleistocene in southern Africa (probably earlier in the African tropics) that permits the inference of habitual collective ritual,

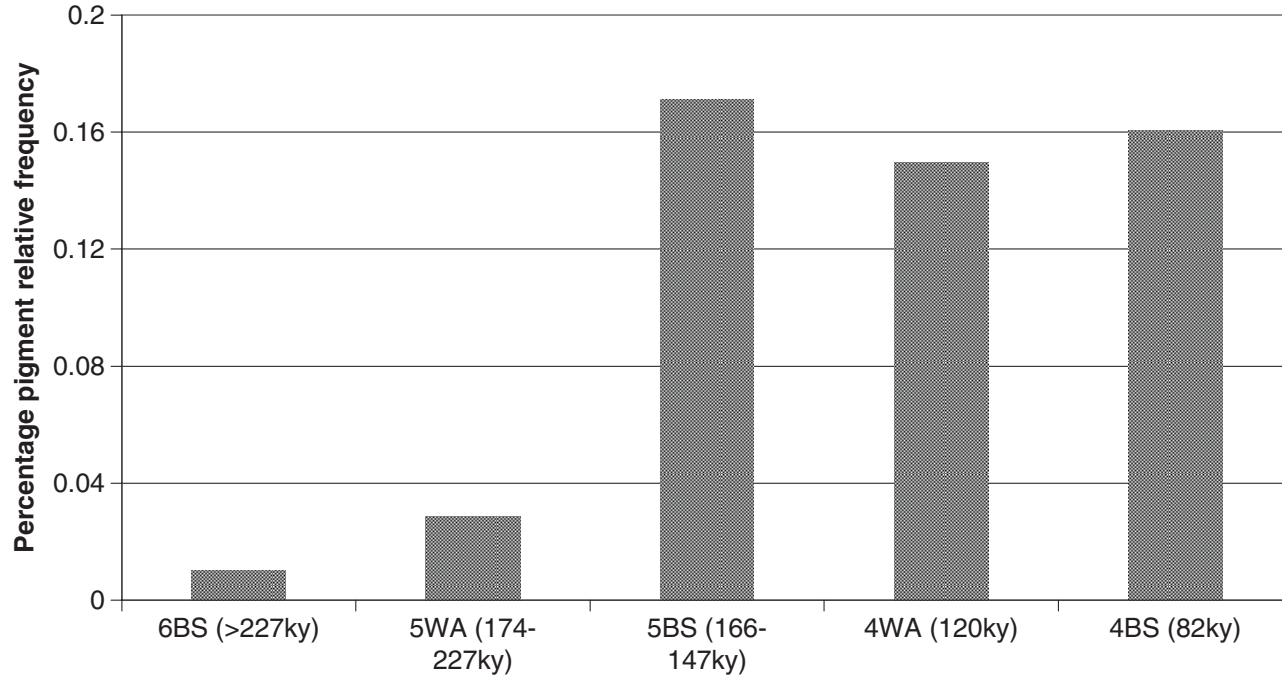


FIG. 4.1 Changes in the relative frequency of pigment in the earlier MSA (Pietersburg) units at Border Cave (KwaZulu-Natal). Excavation units from Beaumont's 1987 excavation. Relative frequency = pigment counts as a percentage of the combined pigment and lithic assemblages. Source: Watts 1998: fig.7.23, dates from Grun & Beaumont 2001.

with applications of red pigments to the body playing an integral part in ritual displays.

Given the posited relationship between collective ritual and language (Knight 1998), the higher-level inference is that, at least by the terminal Middle Pleistocene, speech communities were distributed across Africa, with roots probably going back at least 250 ky within the tropics (see also Barham 2002b). The temporal and color focus predictions of the FCC model are particularly consistent with the summarized African data, although a detailed account of the claimed early hematite at Wonderwerk (footnote 8) is awaited, and the predominance of yellow ochre at Sai Island is surprising. This raises some intriguing questions in relation to Europe. Why should a lineage ancestral to Neanderthals have briefly and sporadically engaged in a behavior consistent with the FCC hypothesis, only to abandon it? Why should a more varied form of pigment use reappear with late classic Neanderthals, only to converge with modern human practice during the brief period of co-existence (see Power this volume)?

#### 4.10 Red ochre use at Blombos Cave

The coastal site of Blombos Cave has provided some of the earliest compelling evidence for symbolic traditions: shell beads (some bearing ochre residues), dated to  $\sim 75$  ky (d'Errico et al. 2005), and geometric engravings on ochre spanning the period from  $\sim 100$  ky to  $\sim 75$  ky (Henshilwood et al. 2002, in press). I know of no hunter-gatherer society without some form of body marking—predominantly body painting, but including also tattooing and scarification. As predicted on theoretical grounds by Durkheim, the designs are invariably non-figurative, comprising geometrically arranged lines or shapes (e.g. Spencer and Gillen 1899; Teit 1927–8; Drury 1935: 102; Marshall 1976: 276; Lewis 2002, Plate 9.4, 9.5; Fiore 2002). It is almost inconceivable that the MSA occupants of Blombos were engraving such designs onto pieces of ochre while not doing similar things with ground ochre powder on their bodies (grinding being the predominant form of use-wear).

The MSA sequence spans the period from  $>143$  ky to  $\sim 70$  ky (Jacobs et al. 2006). Ochre is present throughout. At least in the younger occupa-

tions, its use seems to have permeated many aspects of life. As well as appearing on some beads, it may have been used as a polishing agent to lend “added value” to some of the bone tools from the ~77 ky layers (Henshilwood et al. 2001b). My own cursory examination of selected lithics found variably distributed ochre residues on a variety of tools—predominantly from the younger layers.

Over 1,500 pigment pieces  $\geq 1$  cm in length were analyzed, weighing 5.6 kilos.<sup>9</sup> Shale, siltstone, and coarse siltstone predominate (Figure 4.2). Fewer than a dozen pieces (c. 150 g in total) were associated with significant non-pigmentaceous material. While pigment use was habitual, the quantities vary enormously through time (Layer CI, for example, accounts for half the assemblage mass). This is attributed to changes in sea level and sand cover, exposing and then masking a local exposure of Bokkeveld shale and siltstone. The inference is based on two observations. First, in layers where pigment is most abundant (CJ-CH)—culminating around 100 ky—much of it shows borings by pholadid molluscs and carbonate tests of marine organisms (e.g. Plate 4.1), testifying to procurement from the wave-eroded coastal peneplane. Second, although there is currently no exposed Bokkeveld within c. 15 km of the site, there is an extensive, largely masked contact between the Bokkeveld and (non-ochreous) quartzitic sandstone (Table Mountain Group), running parallel to the coast (Rogers 1988: 411); the closest coastal intercept to Blombos is estimated 3–5 km WNW (masked by beach sand). Where ochre is less abundant (underlying CL-CP, and overlying CF-BZ), traces of marine organisms are rare (absent above CF), and hematite and fine sandstone are better represented (Figure 4.2). Color profiles<sup>10</sup> track the raw material changes (Figure 4.3), with the combined representation of “saturated reddish-brown,” “very red,” and “very dark” values tracking hematite

<sup>9</sup> The data presented here supersedes the preliminary site report (Henshilwood et al. 2001a). They will be presented more exhaustively in a forthcoming report.

<sup>10</sup> The Natural Color System Index (2nd edn., 1999) was used to code streaks. This uses a percentage metric for blackness, chroma, and hue. Values were grouped along two axes. Nuance (combined blackness and chroma) was divided into pastel, intermediate, and saturated groups. “Saturated” cases have the highest chroma for given levels of blackness. Pastels have the lowest chroma (in the range of 10–25%) for the same levels of blackness. Intermediate nuances fall between these poles. Blackness and chroma values above the 5th percentile in the 10% intervals are rounded up. Hue groupings were “yellow brown” (<50% redness), “reddish brown” (50–74% redness), and “very red” ( $\geq 75\%$  redness). Values with  $\geq 56\%$  blackness were grouped into a “very dark” category, regardless of chroma or redness.

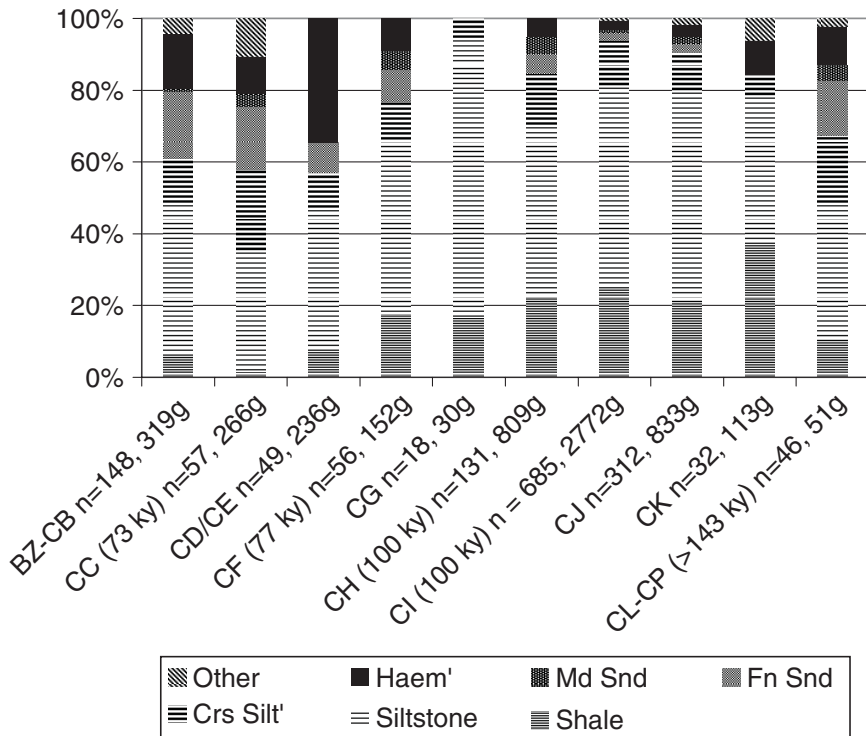


FIG. 4.2 Pigment raw material profiles by excavation aggregate at Blombos Cave (1998/1999 excavations). Percentages based on frequency, column headings also providing total mass (grams).

and fine sandstone, and “intermediate” reddish-brown and yellowish-brown tracking fine-grained sedimentary materials.<sup>11</sup>

When ochre was scarce, Blombos occupants could have abandoned its use; or traveled 15–20 km east to obtain similar material from the lower Goukou Valley; or travelled 35–40 km north (inland) to obtain higher-quality materials (an area commercially quarried for red and yellow ochre). The inland exposures are also Bokkeveld, but, being beyond the

<sup>11</sup> Twenty two of the 23 “very dark” values had  $\geq 70\%$  redness and just four had  $\geq 70\%$  blackness; most can, therefore, be considered extensions of intermediate and saturated reddish brown and very red groupings. Among intermediate yellowish browns ( $n = 156$ ), 78.2% fall within 10% of the yellow/red cut point, and can be treated as an extension of reddish browns.

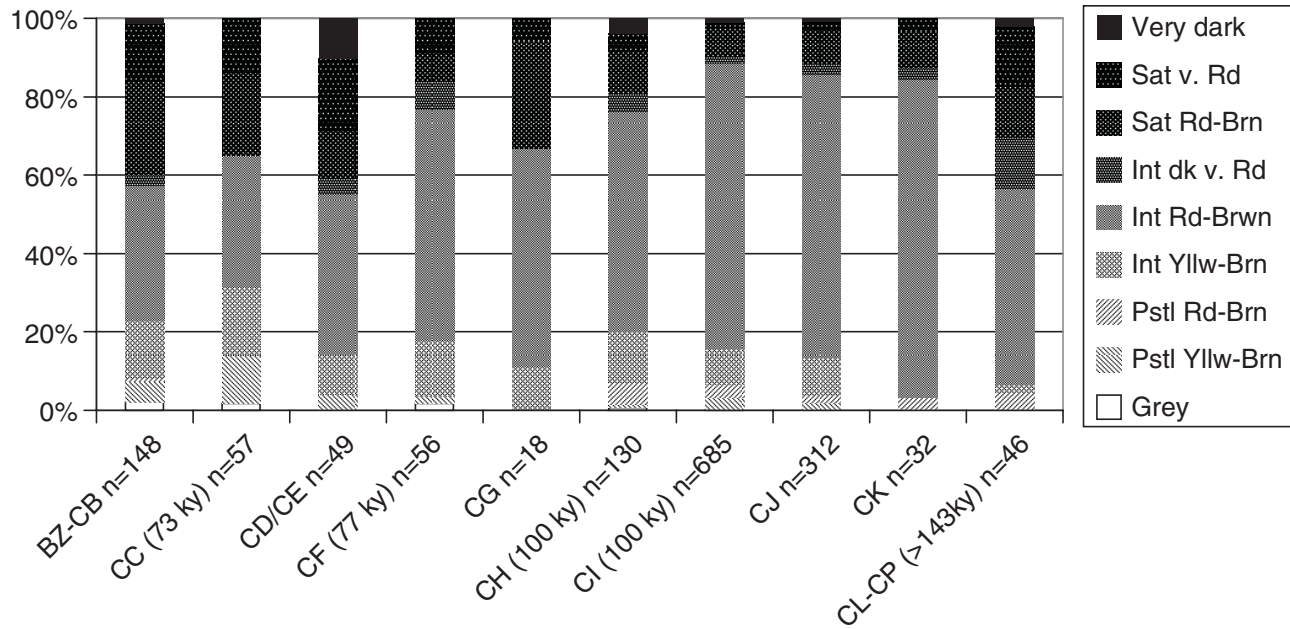


FIG. 4.3 Grouped colour (streak) profiles by excavation aggregate at Blombos Cave (1998/1999 excavations). Percentages based on frequency of grouped Natural Color System (NCS) values.



reach of Cainozoic marine transgressions, they are more deeply weathered, with more pronounced secondary alteration (e.g. hematization). Additionally, sandier expressions of Bokkeveld are more common than to the south (Theron 1972: 18, 58).<sup>12</sup> Judging by raw material and color profiles, the last option was frequently chosen. This complements results from Qafzeh, where local ochre was ignored in favor of more distant, more hematite-enriched material (Hovers et al. 2003).

The incidence of use-wear is strikingly correlated with redness (Figure 4.4). Only about 10% of yellowish-brown pieces were utilized; as soon as red predominates, utilized percentages incrementally increase, peaking at ~50% of pieces with  $\geq 80\%$  redness. Not only were the redder pieces more likely to be used; saturated reds were more likely to be used than intermediate counterparts (Figure 4.5). The pattern persists even in layers such as CI, where local procurement prevailed (peak rates of utilization shift to “intermediate very red” and “saturated reddish-brown,” owing to small sample size bias among “saturated very red” [ $n = 10$ ]). The great majority (82.7%) of “saturated very red” values were moderately dark ( $\geq 35\%$ ,  $< 56\%$  blackness). The preferential use of what might be dubbed “blood-reds” is borne out by estimates of the intensity of grinding (Figure 4.6). The reddest and the most saturated pieces were more likely to be intensively ground than less red, less saturated counterparts. Intensively ground pieces were multifaceted, typically with facets converging to a point (e.g. Plate 4.2, 4.3, 4.4a–b), such pieces generally being described as “crayons” (but see Wadley 2005b). Harder pieces could only have acquired this morphology through prolonged processing, probably involving multiple episodes of use. This in turn would imply safekeeping between episodes of use, further supporting the inference of high esteem. The color selection is consistent with that reported from Twin Rivers, from the terminal Middle Pleistocene at nearby Pinnacle Point (Marean et al. 2007), and with Watts’ (2002: 10) more subjective observations on a multi-site MSA sample. It also accords with the two previously discussed ethnographic accounts of the best (indigenous) exemplars of what have been glossed as “light” or “light/warm” color terms.

While “saturated very red” pigments (typically fairly dark) were the most valued, the margins of the utilized distribution deserve comment.

<sup>12</sup> Because coastal and inland ochreous exposures are the same substrate, and because Bokkeveld shale formations are mineralogically uniform (Danchin 1970), it is unlikely that geochemical and mineralogical analyses could test this interpretation.

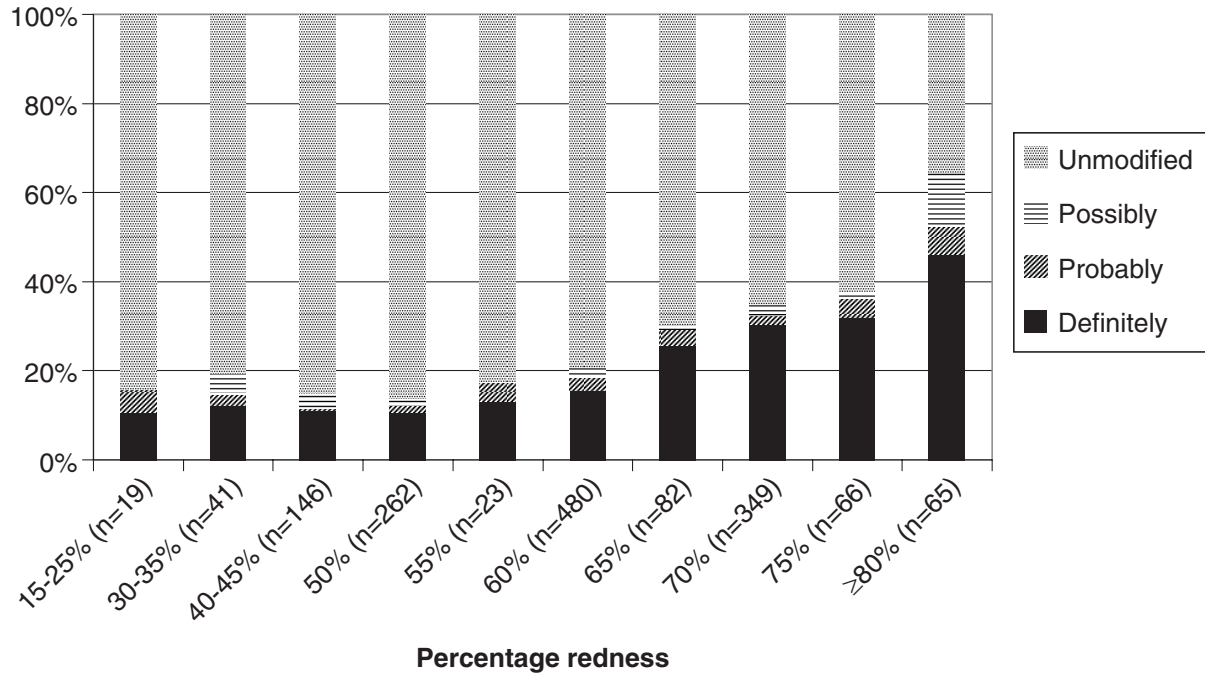


FIG. 4.4 Percentages of utilization confidence assessments by percentage redness (redness as a ratio to yellowness).

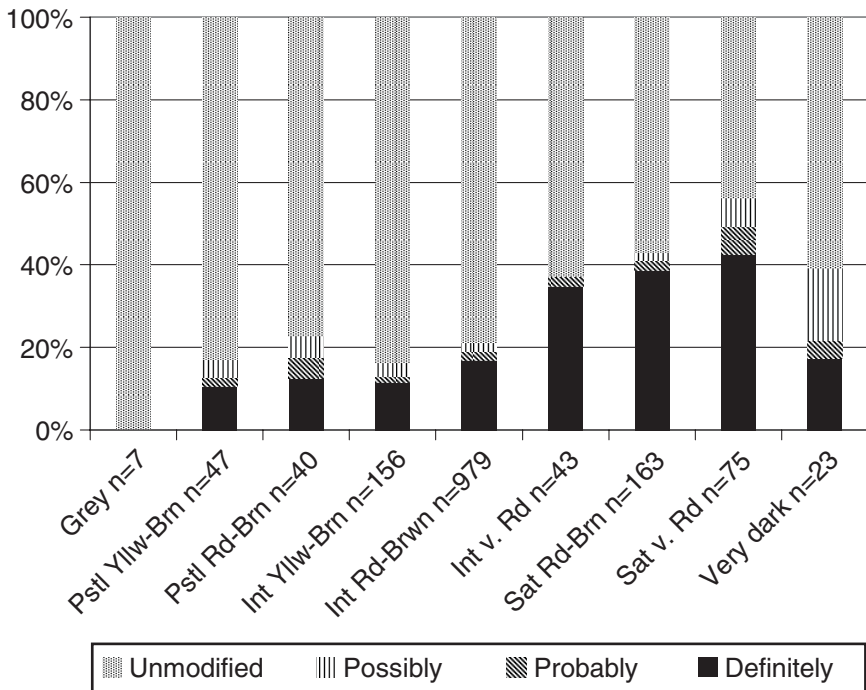


FIG. 4.5 Percentages of utilization confidence assessments by grouped NCS values.

Utilized “pastel yellowish-browns” ( $n = 5$ ) have  $\leq 25\%$  blackness; lightness rather than hue seems to be the most perceptually salient attribute of this group (which includes two geometrically engraved pieces from CI). Of 18 utilized “intermediate yellowish-browns,” three had  $>60\%$  yellowness, the rest were close to reddish-browns. One of the four utilized “very dark” values had 70% blackness and was described as brownish-black. These three peripheral subgroups are significant in showing that the focus on reds was not exclusive; light, very dark, and yellowish materials were occasionally used. Assuming a color lexicon, such pieces may have been distinguished from the vast bulk of the assemblage; but, as at Twin Rivers, their rarity underlines just how preoccupied MSA people were with red. As with the overall survey, these few pieces do not support the binary oppositions predicted by either the original or the revised versions of BCT theory. Short of invoking untestable propositions about use of white ash and charcoal, the only recurrent opposition that might be archeologically

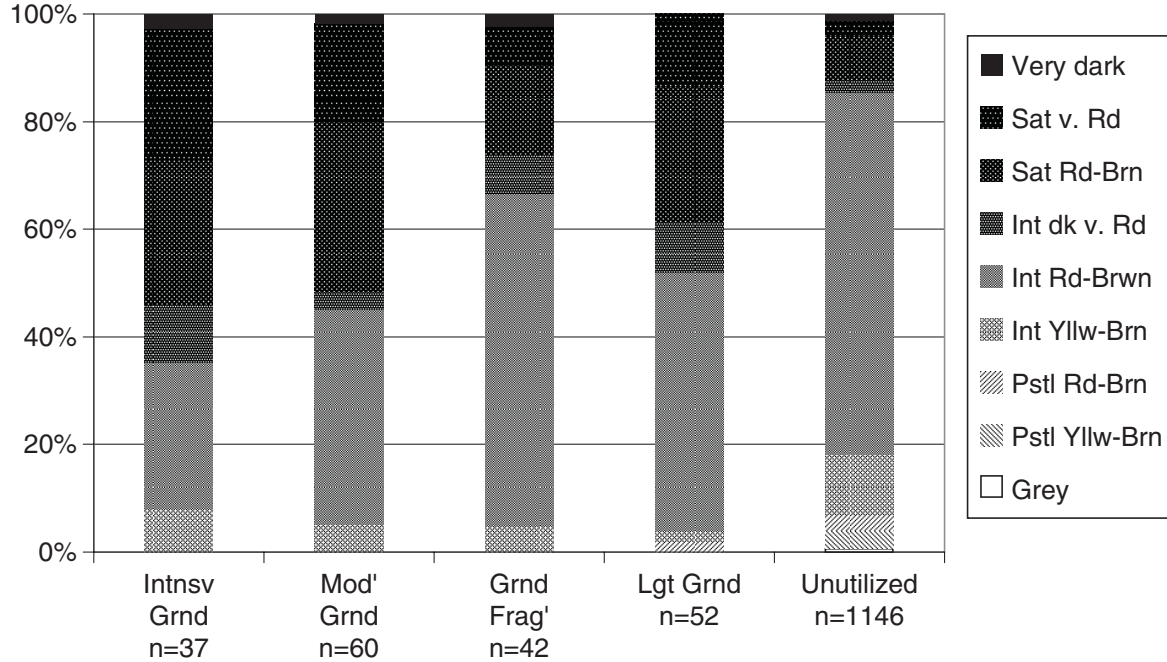


FIG. 4.6 Percentages of grouped NCS values by intensity of grinding compared to unutilized pieces (excludes non-ground forms of utilization and non-definite assessments).

inferred would be “signal on” (ritual display with red pigment) versus “signal off” (no pigment use).

Two final features worth noting are the small size of many utilized pieces, and the high proportion of lightly utilized ones. Of 307 definitely used pieces, 80 were judged to be  $\geq 90\%$  complete. A quarter of these were between 1.5 cm and 2.5 cm long (mean 19 mm, s.d. 2.9 mm,  $n = 22$ ), just large enough to be held between forefinger and thumb. Some are intensively utilized, others less so (e.g. Plates 4.5, 4.6). That individual episodes of use often only produced tiny amounts of powder is evident among lightly utilized pieces (e.g. Plates 4.7, 4.8). Eleven lightly ground pieces ( $n = 52$ ) were judged  $\geq 90\%$  complete with single facets; facet widths were recorded for seven of these, providing an average of 2.9 mm (s.d. 0.8 mm). Ten of the eleven cases had  $\geq 70\%$  redness. The high proportion of saturated and very red values among lightly ground pieces (Figure 4.6) suggests that, rather than representing mere trials in the search for the reddest, most saturated pigments, these were used similarly to more intensively ground counterparts. The tiny amounts of powder produced would surely have been insufficient for just about anything other than design purposes.

In summary, MSA people at Blombos preferred saturated red earth pigments. These were more likely to be ground and ground intensively, probably involving multiple episodes of use and curation. At the same time, individual episodes of use often only produced tiny amounts of powder (with similar selective criteria), a practice probably inconsistent with anything other than making designs on the face, body, or some other organic surface. Together with the geometric engravings (from c. 100 ky), this provides good circumstantial evidence for the use of typically “blood-red” ochre in the painting of abstract designs on the bodies of ritual performers, from at least 143 ky. That a nearby assemblage shows identical selective criteria from  $\sim 164$  ky (Marean et al. 2007), suggests that this cultural tradition was already established by the time of our speciation, between  $\sim 150$  ky and  $\sim 200$  ky.

#### 4.11 Discussion

With the exception of the tanning hypothesis, all of the theoretical perspectives and inductive hypotheses considered here have some explanatory

merit. The hafting hypothesis partially explains the archeological observations from which it arose. However, until functional properties additional to those of known alternative and cheaper filler/loading agents are demonstrated, it is more parsimonious to infer that this was a case of symbolic considerations influencing a functional choice. As a general explanatory hypothesis, hafting cannot account for large assemblages, the hue and chroma-based selective criteria, or pieces with solitary, small grinding facets.

The cross-cultural findings associated with BCT theory are fairly robust, and few doubt that biology constrains both color categorization and naming. What is contested is whether biology provides sufficient constraints for coordinating perceptually grounded categories codified in language (e.g. Deacon 1997; Jameson 2005; Steels and Belpaeme 2005). The paradox of dark, saturated red being selected as exemplary of what is glossed as a “light/warm” term in Stage I color lexicons remains inadequately addressed. Although not designed to address archeological data, the Middle and earlier Late Pleistocene record of pigment use presents several challenges to BCT theory. Why the overwhelming use of just one color rather than the predicted binary opposition? Why a focus on one term of the predicted pair rather than the other? Why red rather than white or yellow? And why the focus on relatively dark reds?

Deacon’s qualified innatism opens the door to cultural factors—specifically ritual—impinging on color lexicalization, but it does no more than this. The projection of wedding rituals back into the Plio-Pleistocene—and with it the implicit antiquity of BCTs—makes it implausible that the pragmatics of color terminology in extant cultures have any bearing on the evolution of color terms.

Hovers and colleagues are oblivious to any contradiction in presenting a thoroughly innatist model, and then (in discussion) invoking Deacon’s arguments about the role of ritual in learning symbols. As with Deacon’s hypothesized rituals, the pigments probably deployed in Qafzeh mortuary rituals could as readily have been black or white as red.

Like BCT theory, Durkheim’s theory of collective representations is non-Darwinian. However, his predictions regarding the form of early ritual performance—involving the painting of geometric designs on the bodies of ritual performers with red ochre—seem remarkably prescient in view of the Blombos engravings. A necessarily circumstantial case has been made for these predictions being met in the late Middle Pleistocene and

early Late Pleistocene African record of ochre use. What is missing is an evolutionary account that might account for this blood symbolism.

The five archeological predictions made by the FCC model are met by the evidence outlined above. Several mid-Middle Pleistocene lineages may have engaged in something like “sham menstruation,” but habitual collective ritual can only be inferred among our immediate African ancestors, perhaps initially restricted to local populations within the tropics, but becoming generalized across Africa towards the end of the Middle Pleistocene. Not only is there an almost exclusive focus on reds, but blood reds seem to have been especially esteemed. When not locally available, people would go some distance to procure them.

To summarize: Because collective rituals are costly, they demonstrate commitment. A consequence of commitment is the generation of trust. Once you have a ritual community within which there is sufficient trust, you no longer need costly signals for internal use—you can afford to develop cheaper, coded forms of communication. Costly ritual continues to be required for signaling to an “out-group” (e.g. potential mates), and for the incorporation of new members (e.g. girls reaching reproductive age) into the ritual coalition. Human speech communities were born out of the regular performance of such costly displays.

# 5 Theoretical underpinnings of inferences about language evolution: the syntax used at Blombos Cave

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RUDOLF BOTHA

## 5.1 Introduction

In empirical work, any inference about what the evolution of language involved needs to be underpinned by a range of theories. This idea is subscribed to at a general level in a substantive body of modern work on language evolution. But, the present chapter argues, the idea is not executed sufficiently fully. Hence, potentially interesting inferences about language evolution are less well-founded than they appear to be at first glance. To illustrate its argument, the chapter analyzes in some detail inferences drawn about language evolution on the basis of archeological findings made at Blombos Cave. The chapter will show, in addition, that the argument holds likewise for inferences drawn about language evolution in areas other than archeology. These include linguistics, musicology, and genetics.

Which brings us to a first question: What are the theories needed for underpinning inferences about language evolution? To identify them in a general way, a little conceptual anatomy will do. This involves laying bare the componential structure of the inferences which are typically drawn in empirical work on aspects of language evolution. The basic components of this structure are three: data or assumptions about a phenomenon believed to be related in some way to language evolution, a conclusion about an aspect of language evolution, and an inferential step by which the latter

This chapter draws on work that was done during my stay at the Netherlands Institute for Advanced Study (NIAS) in 2005–6. I would like to express my gratitude to NIAS for its generous support of that work, financially and otherwise. I am also much indebted to Maggie Tallerman and Eric Reuland for valuable comments on an earlier draft of this chapter, and to Walter Winckler for his expert editing of the text.



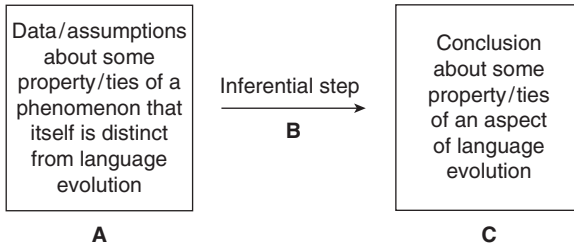


FIG. 5.1 Basic structure of non-compound inferences about language evolution.

conclusion is drawn from the former data or assumptions. In the case of non-compound inferences, only one inferential step is used in drawing a conclusion; in the case of compound inferences, two or more steps are taken consecutively in arriving at a conclusion.

The skeleton of non-compound inferences about language evolution can, accordingly, be represented as in Figure 5.1.

The theories under consideration can now be identified as those needed for underpinning, firstly, the data or assumptions represented in Figure 5.1 by box A; secondly, the inferential step represented by arrow B; and, thirdly, the conclusion represented by box C. The need to provide theoretical underpinnings for the three components will be shown below to arise from basic conditions which these three components have to meet.

## 5.2 Underpinning the conclusions

From the properties of a number of Middle Stone Age (MSA) shell beads excavated at Blombos Cave near Still Bay in South Africa, Henshilwood, d’Errico, and other members of their group have drawn the following conclusion:

- (1) The humans who inhabited Blombos Cave some 75,000 years ago had “fully syntactical language” (Henshilwood et al. 2004: 404; d’Errico et al. 2004: 17–18).<sup>1</sup>

The inference which yielded this conclusion is a compound one, i.e. one that uses various inferential steps to arrive at its conclusion. Reconstructed

<sup>1</sup> In some formulations – Henshilwood and Marean (2003: 636), d’Errico et al. (2004) the conclusion is couched in terms of the less qualified expression “syntactical language.”

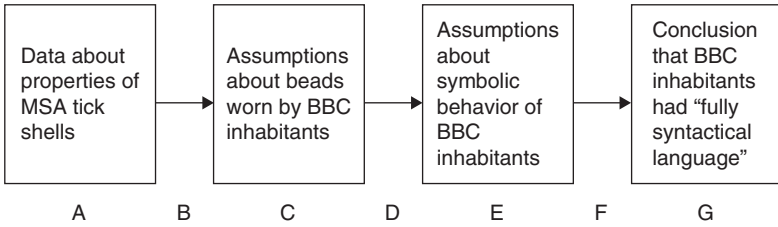


FIG. 5.2 Structure of the compound inference about the language used by the inhabitants of Blombos Cave (“Blombos Cave” is abbreviated in boxes C, E, and G as “BBC”).

on the basis of accounts such as those listed in (1), these steps form the inferential chain represented schematically in Figure 5.2.

Stated non-schematically, the inferential steps in question involve the following:

1. B represents the step by which it is inferred from data and/or assumptions about properties of a number of MSA tick shells that these shells were beads worn by the humans who inhabited Blombos Cave some 75,000 years ago. [Elucidation: The data are about properties of 41 shells of the scavenging gastropod technically known as *Nassarius kraussianus* and include the following: (a) The shells are about 75,000 years old. (b) The shells were found kilometres away from the estuaries in which the molluscs were likely to have occurred. (c) As regards their physical properties: (i) the type of perforation in the shells is rare in nature and difficult to explain as the result of natural processes; (ii) the shells have flattened facets; and (iii) four of the shells show microscopic traces of red ochre on their insides and surfaces. (d) Thirty-three shells were found in six groups of two to twelve, with all six groups being discovered in a single excavation day and all six coming either from a single square or from two adjacent subsquares in the cave. (e) Shells found in the same group are similar in regard to their adult size, their shade, and their type of perforation.]
2. D represents the step by which it is inferred from assumptions about these beads—or rather the beadworks of which they formed part—that these humans engaged in symbolic behavior. [Elucidation: In terms of one of these assumptions, the beads were worn as personal ornaments.]

3. F represents the step by which it is inferred from assumptions about this symbolic behavior that these humans had “fully syntactical language.” [In terms of a core assumption—to which I will return in section 5.3 below—the symbolic behavior involved transmitting and sharing of symbolic meaning.]

On an alternative reconstruction of the inference, inferential steps B and D are collapsed (Botha 2008a: section 4); this possibility is immaterial, however, to the argument to be developed below.

Like all conclusions drawn in empirical work about an aspect of language evolution, conclusion (1) needs to meet a particular condition: the Pertinence Condition:

- (2) Conclusions about language evolution need to be pertinent in being about (a) the right thing and (b) the right process.

If the right thing is taken as language, and the right process as language evolution, this condition seems obvious enough: so obvious, indeed, that it may seem hard to imagine how it will ever fail to be met. And yet, there are several possibilities of failure. First, what are presented as conclusions about language evolution can fail subcase (a) of the Pertinence Condition by being about an entity that is not actually language or by being unclear as to what entity they are meant to be about. Second, conclusions about language evolution can fail subcase (b) of the condition by being about a process that is not actually the evolution of language or by being unclear as to what process they are meant to be about.

Considered from the perspective of the Pertinence Condition, the following question arises about conclusion (1): What exactly is the entity that it is about? More specifically: What are the distinguishing properties of “fully syntactical language”? Or: How does “fully syntactical language” differ from “syntactical language” that cannot be portrayed as fully so? These are not questions with mere terminological import; they are about a factual matter. For, on a widely held view, syntax evolved gradually in terms of steps or stages. For instance, Jackendoff (2002: 238) makes provision for the evolution of syntax to have started from protolanguage, possessing no or only rudimentary syntax, and to have moved from there up to modern language through five partially ordered steps.<sup>2</sup> He portrays these steps schematically as in Figure 5.3.

<sup>2</sup> For an earlier version of this gradualist scenario, see Jackendoff (1999). For other accounts on which language, including syntax, evolved gradually, see Pinker and Bloom

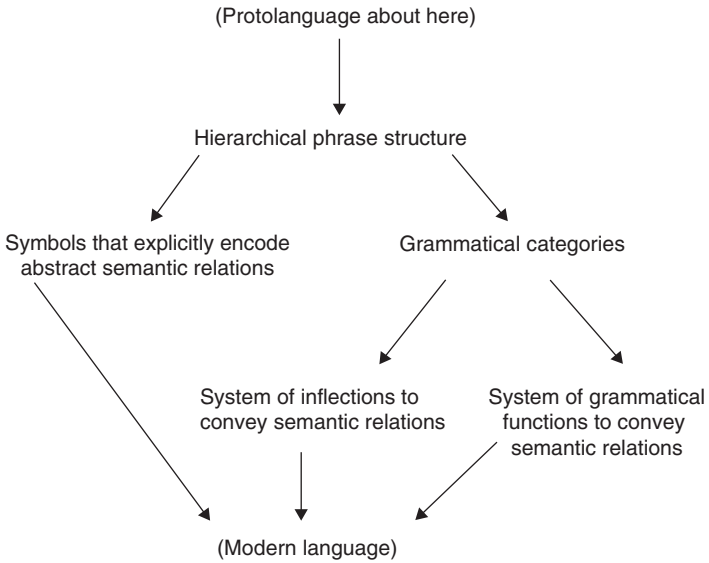


FIG. 5.3 Steps in the evolution of syntax according to Jackendoff (2002: 238).

The steps provided for in Jackendoff's scenario each contributed distinctively to what he refers to as "the precision and variety of expression." Now, if accounts positing various stages in the evolution of syntax are plausible, the question is: What kind or degree of complexity would syntax have had to possess to be able to serve the communicative function(s) that are being attributed to the language used at Blombos Cave some 75,000 years ago? Alternatively: Which one of the stages distinguished by gradualists such as Jackendoff would correspond to what is referred to by the expression "fully syntactical language"? The answer to this question is crucial for *ruling out* the interesting possibility that the stage of syntactic evolution underlying the sentences uttered by Blombos inhabitants some 75,000 years ago may have been one of the earlier, and hence less complex, stages in the evolution of syntax. The question would not dissolve if the specifics proposed by Jackendoff or others turned out to be incorrect in regard to the number or make-up of the stages. It will continue to be

(1990), Newmeyer (1998: 317), Botha (2003: 39–41), and Pinker and Jackendoff (2005: 223). In their account of the evolution of syntax, Calvin and Bickerton (2000: 136, 146–147) likewise provide for more than one step.

pertinent for as long as there are respectable gradualist accounts to the effect that syntax did not emerge in one fell swoop in its full modern complexity.<sup>3</sup>

Henshilwood, d'Errico, and their associates do not themselves offer an explicit characterization of “(fully) syntactical language.” Referring to work by Wynn (1991), Henshilwood and Marean (2003: 635) invoke an entity which they label “syntactical language use” and which they characterize as “a combination of grammar, semiotic ability, and its pragmatic application.” In the relevant article by Wynn (1991: 191–192), one finds the following characterization of the entity he speaks of as “language”:

Language, of course, consists of more than just grammar; indeed, it is probably best to think of language as a very complex behavior that involves the interweaving of many components (Lieberman 1984; Chomsky 1980). These include, in addition to grammar, a symbolic (semiotic) ability, knowledge of how to use language (Pragmatics), and the biomechanical and neural structure of speech.

This characterization of “language” is problematic in various ways. Thus it does not distinguish between language as a cognitive entity and the use of language as a form of behavior. Moreover, the reference to Chomsky (1980) is puzzling since one of Chomsky’s most fundamental claims is that language is not a form of behavior. And the characterization as such is not informative about the nature of “syntactical language” (and that perhaps was not its purpose in the first place). An obvious paraphrase would be “language of which syntax is a distinctive component.” But this paraphrase, in turn, is uninformative in the absence of a characterization of what syntax is. This still leaves us without a characterization of the entity denoted by the expression “syntactical language.”

In being unclear about what “fully syntactical language” is, conclusion (1) is problematic from the perspective of subcase (a) of the Pertinence Condition (2). This problem cannot be solved by simply giving one or another definition of the term “syntax.” In terms of modern linguistic theories, syntax is a central component of grammar or grammatical competence. And even a cursory glance at the literature reveals the existence of a large number of modern theories of syntax. To make the conclusion that the inhabitants of Blombos Cave had “fully syntactical language” properly pertinent, one would have to explicate it by underpinning it with an adequate modern theory of syntax. The same applies to

<sup>3</sup> For accounts on which syntax did originate in the latter way, see Berwick (1998).

the other expressions—e.g. “complex syntactical language” and “modern language”—used for characterizing the language of the Blombos inhabitants. The point is in fact more general: Conclusions about language evolution couched in terms referring to components of language such as syntax, semantics or meaning, phonology, and the like need to be underpinned by modern theories giving an adequate characterization of these components or their relevant subcomponents or the central properties of these subcomponents. In empirical forms of inquiry, the question as to what is involved in any one these aspects of language is of a kind that simply cannot be settled by stipulation.<sup>4</sup>

Subcase (a) of the Pertinence Condition clearly applies to conclusions drawn about language evolution in areas other than archeology too. And modern work in some areas offers instances of potentially interesting conclusions which do not meet this condition. Consider in this regard the following conclusions:

- (3) (a) Language and music evolved from a common precursor, namely “musilanguage” (Brown 2000: 272, 277).
- (b) Language arose as a result of a genetic change that introduced a new principle of brain function some 100,000 years ago (Crow 2000, 2002a: 3).
- (c) Early hominin motherese, i.e. infant-directed affective vocalizations, formed the prelinguistic substrates of protolanguage (Falk 2004: 491).

From the perspective of pertinence, these three conclusions are problematic in the same way: In all three of them two distinct entities, namely language and speech, are conflated. This has been shown for Crow’s conclusion (3b) by Annett (1998, 2000). Thus, she cites evidence about children with cerebral palsy showing that (i) the gene that was implicated in the change postulated by Crow is “for” speech and not language (Annett 2000: 1–2), and (ii) cerebral dominance is not “for” high level language but “for” speech (Annett 2000: 3). For Falk’s conclusion (3c), it

<sup>4</sup> For a forceful argument to the effect that progress in work on the evolution of language demands an adequate theory of syntax, see Bickerton (2003: 87–91). See Botha (2003: 39–41) for an illustration of the way in which someone’s theory of syntax colors her/his theory of the evolution of syntax. Recursion is a good example of a property of syntax which is currently treated in an insufficiently careful way in discussions of language evolution, as is clear from, amongst others, Pinker and Jackendoff (2005: 229–231) and Jackendoff, Liberman, Pullum, and Scholz (2006).

has been independently shown by Bickerton (2004: 405), by Spiezio and Lunardelli (2004: 523), and by Botha (2006a: 139, 2008b) that she does not draw a principled distinction between “language” and “speech” and arbitrarily switches between these notions in her discussion.<sup>5</sup> As for Brown’s conclusion (3a), he too seems to switch arbitrarily between the notions of “language” and “speech” and, moreover, attributes properties of speech to language. Thus he (2000: 273) maintains that the discrete units of language are acoustic elements and that the basic acoustic properties of language are modulated by expressive phrasing.<sup>6</sup> To conflate language and speech is to say that a cognitive capacity or system on the one hand and its use in a particular modality on the other hand are one and the same thing. Or, it is to say in essence that in the linguistic domain, cognition and behavior are identical.<sup>7</sup>

Which brings us to the second kind of theory by which inferences about language evolution—and their conclusions in particular—need to be underpinned: a linguistic ontology. This is a theory of the large-scale entities that are believed to populate the linguistic domain. These entities include language, languages, the language capacity or faculty of language, tacit knowledge of language, language behavior, speech and other forms of language use, linguistic skill, and so on. The function of a linguistic ontology is to draw a principled distinction among such entities, characterizing them in a non-arbitrary way. The distinction drawn recently by Hauser, Chomsky, and Fitch (2002: 1570–1571) between the language faculty in a broad sense (FLB) and the language faculty in a narrow sense (FLN) is an example of a finer distinction that can form part of a principled linguistic ontology.<sup>8</sup> Chomsky’s distinction between

<sup>5</sup> Falk (2004: 259) has denied this without argument, however.

<sup>6</sup> See Botha (in press) for further analysis of Brown’s conflation of the notions of “language” and “speech.” Bickerton (2003: 80) has likewise criticized Mithen (2000) for using “language” and “speech” interchangeably. Mithen (2005), following Brown, has also argued that music and language had a common precursor. For an appraisal of Mithen’s and Brown’s argument, see Botha (in press).

<sup>7</sup> To avoid this pitfall, Henshilwood and d’Errico (d’Errico et al. 2003) would have to distinguish in a principled way between the entity they refer to as “modern language” (pp. 17, 31, 55) and those they denote by expressions such as “language behaviors” (p. 17), “form of verbal exchange” (p. 30), “articulate speech” (p. 30), and “articulated speech” (p. 48).

<sup>8</sup> On Hauser, Chomsky, and Fitch’s (2002: 1570–1571) characterization, the FLB includes an internal computational system (or FLN) combined with at least two other organism internal systems, which they label “sensory motor” and “conceptual intentional.” The FLN is taken by them to be the linguistic computational system alone, representing what they refer to as “language in a restricted or narrow sense.”

I(nternalized)-language and E(xternalized)-language would be another good candidate for membership of such a theory of large-scale linguistic entities.<sup>9</sup>

Conclusions about language evolution will bear on language, as opposed to other linguistic entities, by mere accident, if at all, unless they are underpinned by a principled linguistic ontology. An ontology of that sort should make it impossible for two linguistic entities that have different properties—e.g. the entity language and the entity speech—to be arbitrarily treated as if they were one and the same thing. And conversely, where two linguistic entities have the same properties, an ontology of that sort should make it impossible for the two entities to be treated as distinct in any more-than-terminological way.<sup>10</sup> To draw a principled distinction between language, on the one hand, and entities such as language behavior and speech, on the other hand, is not to deny that these other entities may also have evolutionary histories that can be properly investigated in their own right. It is to deny, though, that the evolution of language can be insightfully studied by adopting a concept of “language” in which language and those other entities are simply collapsed into one entity with a single evolutionary history. Even language as a monolithic whole, it has been argued, is too complex an entity to be studied insightfully from an evolutionary perspective. Thus, in terms of a recent statement of Fitch, Hauser, and Chomsky (2005: 179), “profitable research into the biology and evolution of ‘language’ requires its ‘fractionation’ into component mechanisms and interfaces.”

The question that now arises is: What kind of theory is a principled linguistic ontology? In essence, it is a theory which is the product of concept formation as that is conventionally practiced in empirical science. Take the concept of “language” as an example of a core component of a principled linguistic ontology. To be useful in empirical inquiry, this concept has to meet conditions like those put in question form in (4).

<sup>9</sup> An I language, on Chomsky’s (1986: 19–22) characterization, is an element of the mind of a speaker listener. It is acquired, known, and used by a person. An E language, by contrast, is an object that exists outside the mind of a speaker listener as, for example, a collection of utterances, words, sentences, or speech events. Chomsky has drawn various other distinctions that may be included in a linguistic ontology. For an explication of some of these, see Botha (1989: ch. 2).

<sup>10</sup> Problematic, in this regard, would be drawing a distinction between the entity referred to by Chomsky (1988: 21) as “knowledge of language [as] a cognitive state” and the entity he calls “‘language’ as an abstract object, the ‘object of knowledge.’”



- (4) (a) Is the concept needed for giving a systematic account of a body of linguistic facts, including facts about structure, acquisition, variation, change, contact, loss/death, behavior, pathology, diversification, and so on?
- (b) Does the concept provide a good basis for interlinking language with other linguistic entities—capacities, processes, behaviors, etc.?
- (c) Does the concept make it possible to give an account of how language is interrelated with non-linguistic entities of a cognitive sort, a perceptual sort, a neurological sort, etc.?

Opinions may differ about specifics of the conditions that should be accepted as governing the concepts of a principled ontology. Yet, when the situation is viewed in sufficiently general terms, at least two conditions emerge clearly enough. One: Constructing the concepts of a principled ontology cannot be a matter of arbitrary stipulation. Two: Adopting some of these concepts rather than others cannot be a matter of personal preference or disciplinary bias.<sup>11</sup>

Turning to subcase (b) of the Pertinence Condition (2): What does it mean to say that conclusions about language evolution need to be about the right process? This process is generally taken to be a phylogenetic process—or a cluster of such processes—which includes both the emergence of the first form of language in the human species and its subsequent development to full language. This means that conclusions about language evolution are not about any of the processes of diachronic change to which full languages are subject—except, of course, if it is first shown that at least some of the diachronic processes did in fact feature in the phylogeny of language too. To ensure that conclusions about language evolution are about the phylogeny of language, it is accordingly necessary

<sup>11</sup> It is not possible to discuss the evolution of language in a coherent, connected way if participants are free to define language each in his/her own way. For an analysis of discussions in which participants depicted language as different kinds of entities, see Botha (2003: 33–36). In those discussions, language was taken to be, amongst other things, an “aspect of human behavior,” a “group behavior,” “hard wired individual competence,” a “special human skill,” an “activity,” a “meta task,” an “application of social intelligence and a theory of mind,” a “species specific capacity,” a “sort of contract signed by members of a community,” and an entity “spontaneously formed by itself.” In conceptually well founded work on language evolution, the concept of “language” has been used restrictively to include the human language capacity or faculty and the first form of language that evolved in tandem with this capacity (Klein 2001: 85–87).

to underpin them by a theory drawing a principled distinction between the various processes to which language on the one hand and individual languages on the other hand have been subject in their respective developmental histories. For doing this, the concept of the “language faculty” can be invoked, as has been done by Bickerton (2007b: 263) for instance:

Two distinct things are at issue here: (i) the process of biological evolution that yielded the language faculty; and (ii) the subsequent cultural recycling of variants possible within that faculty, more accurately described as language change.

Consider now against this background the status of grammaticalization, the putative process or cluster of processes by which grammatical categories are claimed to develop out of (i) lexical categories, e.g. auxiliary verbs out of lexical verbs, and (ii) other grammatical categories, e.g. aspect markers out of auxiliary verbs. It would be interesting to see what status was assigned to grammaticalization on a theory which drew this distinction. For, on a recent account by Heine and Kuteva (2007), grammaticalization played a central role in what they portray as the “evolution of human language.” At the same time, they (2007: 49) are averse to “dividing the evolution of human language into two phases: one that covers the last two thousand years, or any larger period for that matter, and another for the rest of the evolution.” Not only do they use the notions “the evolution of language” and “the evolution of languages” interchangeably; they also use the notion of “language genesis.” But they nowhere explicate their notion of “the evolution of language/languages” or that of “language genesis” with reference to the distinction between the phylogenetic evolution of language as a biological phenomenon and the non-phylogenetic change of individual languages as a cultural phenomenon. Their (2007: 4) characterization of so-called early language, moreover, says nothing in substantive terms about the distinctive linguistic properties of “early language,” making it an elusive entity. As a result, the claim that grammaticalization played a central role in “language evolution” is less than transparent from the perspective of subcase (b) of the Pertinence Condition.<sup>12</sup>

<sup>12</sup> Some linguists have challenged the assumption that grammaticalization is a unitary process or force. Thus, Lightfoot (2003: 106) has observed that “Grammaticalisation is a real phenomenon but it is quite a different matter to claim that it is a general, unidirectional or an explanatory force.” Campbell (1999: 244), likewise, considers it possible that grammaticalization is “derivative, perhaps an intersection of these various sorts of change – reanalysis, semantic change and sound change – but with no special status of its own.”

### 5.3 Underpinning the data or assumptions

The data or assumptions from which conclusions about language evolution are drawn are about a varied range of phenomena, including fossil skulls, prehistoric sea crossings, prehistoric symbols or symbolic behavior, so-called language genes, pidgin languages, modern homesign systems, modern motherese, similarities between modern language and music, and so on. Because these inferences are drawn in the context of empirical work, they need to meet the Groundedness Condition stated as (5).

- (5) Inferences about language evolution need to be grounded in accurate data or empirical assumptions about phenomena that are well understood.

It is evident that one cannot learn anything about language evolution from properties of a phenomenon that are poorly understood. And, important, most of the phenomena from the properties of which inferences have been drawn about language evolution cannot be understood by being subjected to direct observation or simple forms of inspection. In empirical work, the only means of getting to understand phenomena such as prehistoric symbolic behavior, sea crossings, or pidgin languages is to form appraisable theories about them. And it is these theories which are needed for underpinning the data or assumptions from which the inferences at issue proceed. For ease of reference, these theories may be dubbed “grunder-theories.”

In modern work on language evolution, the need for grunder-theories is well understood on the whole: The view that phenomena speak for themselves or that they carry their explanation on their sleeves seems not to be subscribed to widely. This is not to say, though, that grunder-theories are always presented in an explicit way or that the theories which have been presented are all equally adequate. Consider in this regard the inference from assumptions about the putative symbolic behavior of the inhabitants of Blombos Cave that they had “fully syntactical language” some 75,000 years ago, i.e. the inference represented by box E, arrow F, and box G in Figure 5.2. To be able properly to ground the inferential step represented by F, it is necessary to know what this symbolic behavior

Other linguists e.g. Newmeyer (1998, 2006) have questioned the view that grammaticalization played a central role in the evolution of language as such, a view advocated by Heine and Kuteva. But see also Heine and Kuteva’s (2007: 46–53) rejoinder.

involved. The relevant literature, however, does not give an explicit characterization of it, portraying it rather obliquely as something that is manifested in the following:

- (6) (a) Sharing of symbolic meaning (Henshilwood et al. 2004: 404; Henshilwood et al. 2002: 1279);
- (b) Transmitting of symbolic meaning (Henshilwood et al. 2004: 404; Henshilwood et al. 2002: 1279);
- (c) Creating symbolic codes (d'Errico et al. 2004: 17–18);
- (d) Transmitting of symbolic codes (d'Errico et al. 2004: 17–18);
- (e) Creating of the material expressions of symbols (d'Errico et al. 2003: 6);
- (f) Transmitting of the material expressions of symbols (d'Errico et al. 2003: 6);
- (g) Maintenance of material expressions of symbols (d'Errico et al. 2003: 6);
- (h) Decoding of symbolic referents (Henshilwood and Marean 2003: 636);
- (i) Decoding the meaning of a design (Henshilwood and Marean 2003: 636).

To be able to ground the inferential step represented by F in Figure 5.2, however, one needs to be able to adduce specifics of how activities such as (6)(a)–(i)—or at least a subset of these—were manifested in the behavior of the inhabitants of Blombos Cave. Thus in the case of (6)(a) and (6)(b) questions such as the following need to be addressed:

- (7) (a) What are the symbolic meanings that were shared and transmitted by the inhabitants of Blombos Cave?
- (b) What did the sharing and transmitting involve?
- (c) Were these meanings shared by all these inhabitants or only by a particular group of individuals?
- (d) By whom were these symbolic meanings transmitted—all the inhabitants of the cave, only a particular subgroup of them, or only certain individuals?

In the absence of specifics such as those referred to in these questions, it is not clear which of the inhabitants “fully syntactical language” can be properly attributed to.<sup>13</sup> To find answers to these and related questions

<sup>13</sup> For further discussion of the notions of “symbol” and “symbolic behavior” that feature in the relevant literature, see Botha (2008a: section 4).

an empirical theory of the MSA symbolic behavior under consideration needs to be constructed. If there turned out to be sufficient support for the claims expressed by such a theory, the theory could then be used for underpinning the assumptions represented by box E in Figure 5.2. Yet another need will be pointed up in section 5.4 below: the need for the notions of “sharing” and “transmitting” to be given appropriate empirical content.

The need for adequate grounder-theories can be further illustrated by an inference about language evolution drawn in linguistic work. Elaborating on ideas of Bickerton (1990: 187), Jackendoff (1999: 275, 2002: 249) has drawn the conclusion that the ancestral form of language known as “protolanguage” used Agent First and Focus Last as two principles for ordering the elements making up utterances.<sup>14</sup> The inferential step by which this conclusion is reached is grounded in data or assumptions about pidgin languages: Agent First and Focus Last are used by such languages as ordering principles. There is a problem, though, with this grounding: It is constructed with the aid of an insufficiently restrictive concept of “pidgin language.” This concept refers to an internally undifferentiated range of contact varieties, including those that have been labeled “pre-pidgins,” “incipient pidgins,” “prototypical pidgins,” and “elaborated pidgins.” Prototypical and elaborated pidgins, however, are in all likelihood structurally too complex for their properties to serve as analogs of the properties of protolanguage. Properly to ground the inference in question one needs, amongst other things, data or empirical assumptions about pidgins that are underpinned by a theory of pidgin languages which is more highly articulated and restrictive.<sup>15</sup>

<sup>14</sup> As a stage of ancestral language, protolanguage used arbitrary, meaningful symbols which were strung together in utterances that lacked any kind of syntactic structure, in Calvin and Bickerton’s (2000: 137, 257) view. Agent First is the ordering principle which says that, in strings, Agent is expressed in the subject position. In terms of this principle, the string *hit Fred tree* means “Fred hit the tree” and not “the tree hit Fred” (Jackendoff 1999: 275, 2002: 247). Focus Last says that the informationally focal elements appear last in a string. In accordance with this principle, in the utterance *In the room sat a bear*, the subject appears at the end for focal effect (Jackendoff 1999: 276, 2002: 248).

<sup>15</sup> For a fuller discussion of what it requires to make inferences about properties of protolanguage on the basis of data or assumptions about properties of pidgin languages, see Botha (2006b).

## 5.4 Underpinning the inferential steps

We consider next the theories needed for underpinning the inferential steps shown as B in Figure 5.1. To see more clearly what is involved here, one has to keep in mind that the data or assumptions which these steps start out from are always about one thing, but that the conclusions which these steps end up in are always about another thing. To put the same point more succinctly: As opposed to the latter conclusions, the former data or assumptions are not about language evolution. Instead, as noted above, they are about other phenomena such as Middle Stone Age shells, beads or symbols, about pidgin languages and homesign systems, about modern motherese, about similarities between modern language and music, and so on. As a consequence, a question arises: When the data or assumptions are always about something other than language evolution, then what makes it proper to draw inferences about language evolution from them? This question points to the need for a third basic condition on inferences about language evolution: the Warrantedness Condition stated as (8).

- (8) The inferential steps leading to some conclusion about what language evolution involved need to be suitably warranted or licensed.

This condition requires that the inferential steps should be underpinned by what can be called “bridge theories” (Botha 2003: 146ff., 2006a: 137). Such theories warrant or license the inferential steps by giving an account of the way in which properties of phenomena that the steps start out from are interlinked with properties attributed to language evolution. To the extent that they have merit, these theories serve as the bridges over which to move inferentially from the “departure” properties to the “destination” properties. And to have some merit, bridge theories need to be made up of hypotheses that are explicitly stated, non-ad hoc, and supported by empirical evidence. It clearly would not do if the warrant for an inferential step were a mere stipulation: an arbitrary statement to the effect that data or assumptions about a phenomenon other than language evolution bear on the correctness of claims about some aspect of language evolution.

The need for bridge theories has been explicitly recognized in substantive work in which conclusions about language evolution are drawn. For instance, in a recent article assessing the archeological evidence for the

emergence of language, symbolism, and music, d’Errico and his co-authors (d’Errico et al. 2003) emphasize the role of such theories, referring to them as “general interpretive models” (p. 51), as “analogies” (p. 50), and as “frameworks of inferences that can establish a link between the primary archaeological evidence and its wider implications” (p. 54). It is therefore interesting to look at the way in which some of these authors have executed the idea of bridge theories in work such as that done on the basis of findings made at Blombos Cave. Consider in this regard the inferential steps—represented by B, D, and F in Figure 5.2—by which they have arrived at the conclusion that the humans who inhabited the cave had “fully syntactical language” some 75,000 ago. Each of these three inferential steps needs a warrant drawn from an appropriate bridge theory, a point that can be illustrated with reference to the third step, F.

To be able to provide the required warrant for inferential step F, an appropriate bridge theory needs to give an answer to the question: Why is it permissible to infer from data about the symbolic behavior in which Blombos Cave inhabitants might have engaged that they had “fully syntactical language”? This question is not addressed directly, though, in the relevant literature. In an article in *Science*, only the core assumption of such a theory is alluded to in the following statement (Henshilwood et al. 2004: 404): “Fully syntactical language is arguably an essential requisite to share and transmit the symbolic meaning of beadworks and abstract engravings such as those from Blombos Cave.” Other articles by members of the group use similar formulations in terms of which “syntactical language” or “fully syntactical language” is claimed to be: “the only means of,” “essential for,” “necessary for” or a “direct link to” the symbolic meaning or behavior at issue.<sup>16</sup> But the bare assumption that the presence of “fully syntactical language” is an essential requisite for certain aspects of symbolic meaning or behavior cannot constitute the entire bridge theory needed for warranting the inferential step F.

First, to establish what the content of this theory is—the “transmission theory” for short—one needs to know what it claims about matters such as those listed in (9).

<sup>16</sup> See, for example, d’Errico et al. (2003: 6), d’Errico et al. (2004: 17–18), Henshilwood and Marean (2003: 636).

- (9) (a) What are the specifics of the meanings that were shared or transmitted?
- (b) Why was “fully syntactical language” necessary for sharing or transmitting these specifics or specifics of this kind?
- (c) Why could these specifics not have been shared or transmitted by means of a less fully evolved stage of (“syntactical”) language or by some non-verbal means of communication?
- (d) How do meanings for the transmission of which “fully syntactical language” is a requisite differ in essence from meanings that can be transmitted by less fully evolved language or by non-verbal means?
- (e) What are the cognitive processes to which the expressions “sharing” and “transmitting” ultimately refer?

Second, to be able to judge whether the claims expressed by the transmission theory are non-ad hoc, an answer to the question stated in (10) is needed.

- (10) How do the claims expressed by the transmission theory relate to—i.e. cohere with, conflict with, draw support from, etc.—claims expressed by other theories about the relevant aspects of the cognition of MSA and modern humans?

Third, in order to assess the epistemological status of the transmission theory, answers to the questions listed in (11) are required.

- (11) (a) To what extent is the transmission theory testable-in-principle and testable-in-practice?
- (b) To what extent is the transmission theory supported by empirical evidence or considerations?
- (c) Is there independent evidence—or converging evidence—for the transmission theory?

The *Science* article referred to above is a one-page communication; so, understandably, it does not address questions (9)–(11) in an explicit way. Nor, however, do related articles on the significance of the shell beads found at Blombos Cave. It may be that the *Science* article and the others all draw implicitly on a well-articulated transmission theory which forms part of a more general research paradigm or intellectual tradition. But questions such as (9)–(11) seem to receive no explicit



answer even in literature dealing with fundamental assumptions of such a paradigm or tradition.<sup>17</sup> This means that, though the idea of bridge theories is understood well at a general level by Henshilwood, d'Errico, and their colleagues, it is not executed fully in the work under consideration.<sup>18</sup>

In the case of some work on language evolution, it is hard to tell whether the idea of bridge theories is less than fully understood or simply is not fully executed. This goes, for instance, for work in which inferences about language evolution have been drawn from data or assumptions about pidgin languages. Consider again in this regard the conclusion that protolanguage used the ordering principles Agent First and Focus Last. This conclusion has been drawn from, amongst other things, data or assumptions in terms of which these principles occur in pidgin languages. The question, however, is: Why is it warranted at all to draw conclusions about aspects of language evolution from data or assumptions about properties of pidgin languages? The answer should take the form of a bridge theory of why properties of protolanguage and properties of pidgin languages are rightly considered interlinked and that specifies, moreover, the nature of the interlinkage between the two sets of properties. To date, no such theory has been proposed in an explicit form in the relevant literature.<sup>19</sup>

## 5.5 Conclusion

Figure 5.1 in section 5.1 lays out the basic components of non-compound inferences drawn in empirical work about aspects of language evolution.

<sup>17</sup> The view held by Henshilwood, d'Errico, and their co authors on the link between MSA symbolic behavior and language seems to be similar in general terms to a view expressed by Mellars (1998a: 95–96, 1998b: 89), Wadley (2001: 215), and McBrearty and Brooks (2000: 486). The nature of this link, however, is considered by archeologists such as Graves (1994: 158–159), Chase (1999: 47), Mithen (1999: 153–154), and Davidson (2003: 141–143) to be too complex to be captured in a straightforward way.

<sup>18</sup> From this work it is possible, though, to deduce the assumptions making up the bridge theory which is needed for underpinning the first inferential step – represented as B in Figure 5.2 – of the compound inference under consideration. For some discussion of this point, see Botha (2008a).

<sup>19</sup> For further discussion of this matter, see Botha (2006b: 12). Inferences drawn about aspects of language evolution from data or assumptions about motherese are similarly problematic, as is shown in Botha (2008b).

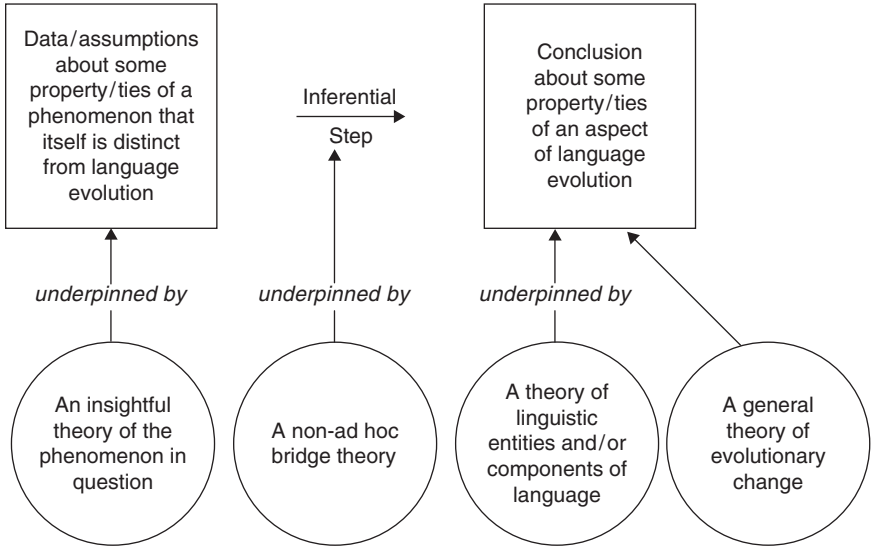


FIG. 5.4 Filled-out structure of non-compound inferences.

In sections 5.2–5.4, a fuller picture of the componential structure of such inferences has emerged, the upshot being Figure 5.4.

Invoking or constructing various kinds of theories, then, is an essential part of making inferences about language evolution. In empirical work, neither arbitrary stipulation nor unconstrained speculation can be an alternative for this recourse to theory.

## 6 Fossil cues to the evolution of speech

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W. TECUMSEH FITCH

### 6.1 Introduction

Speech (complex, articulated vocalization) is the default linguistic signaling mode for all human cultures, except for small populations (e.g. the deaf) for whom the audiomotor modality is unavailable. However, signed languages of the deaf are full, complex, grammatical languages, independent of, but equivalent in all important respects to, spoken languages (Stokoe 1960; Klima and Bellugi 1979), demonstrating that *speech is not the only signaling system adequate to convey language*. Therefore, a crucial distinction in language evolution is that which exists between speech (a signaling system) and language (a system for expressing thoughts, which can incorporate any one of several signaling systems). Additionally, speech can be decoupled from meaning, in certain circumstances, and be treated as a signal, pure and simple (other examples of complex articulated vocalization include infant babbling or jazz scat singing). And of course, additional and more ancient non-linguistic (non-propositional) communication via facial expression and “body language” is also found in all human populations. Nonetheless, speech remains the default *linguistic* signaling system for all unimpaired human cultures, and there is no evidence that this has ever been otherwise, until the advent of writing. Thus, the vast majority of linguistic constructions were conveyed via a speech signal during human evolution. Language evolution may well have been influenced or constrained by its reliance on the auditory/vocal modality.

Many authors have argued for the “special” nature of speech, either at the level of production or perception, suggesting various aspects of speech

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as part of a uniquely human endowment for language. Aristotle, already noting the apparent intelligence of the dolphin, suggested that its inability to speak resulted directly from limitations of its vocal anatomy: “its tongue is not loose, nor has it lips, so as to give utterance to an articulate sound (or a sound of vowel and consonant in combination)” (Aristotle 350 BC). The notion that the critical “missing ingredient” preventing animals from speaking is some aspect of their peripheral morphology has since been repeatedly discussed (Camper 1779; Darwin 1871; Lieberman et al. 1969) and is probably the oldest and most persistent hypothesis in the entire field of language evolution. Other researchers have taken human vocal tract reconfiguration as *the* crucial change which spurred other aspects of language, including syllable structure or syntax (e.g. Carstairs-McCarthy 1999). Thus, important issues in language evolution hinge on the evolution of speech.

Despite its antiquity, the idea that peripheral limitations were a crucial hurdle in language evolution was not fully fleshed out until the 1960s, when a specific, credible hypothesis was proposed concerning the descent of the larynx (Lieberman and Crelin 1971; Lieberman et al. 1972). Possession of a larynx low in the throat is a difference between humans and animals that is both anatomically well defined and acoustically important, and which might also leave some trace in the fossil record. In what has been one of the most discussed hypotheses in the field of language evolution, Lieberman and Crelin argued that the anatomy of the cranial base provides a clear indication of the position of the larynx in the throat, and that even very recent hominins such as Neanderthals lacked a descended larynx. The latter hypothesis unleashed a storm of controversy that continues unabated today (Arensburg et al. 1990; Boë et al. 2002). The controversy was fueled by the rarity of this trait, which was believed uniquely human, until recently, when broad comparative investigations showed otherwise.

### 6.1.1 *The necessity for a broad, comparative approach*

It is sometimes assumed that the student of human evolution need look no further than the order Primates for insights, or worse, that primate biology alone is relevant to human evolution. This misconception is reflected academically by the fact that primatologists typically teach in

Anthropology departments (study of other organisms is relegated to Biology). This odd state of affairs is probably a remnant of incorrect pre-Darwinian “scala naturae” conceptions of phylogeny dating back to Aristotle, in which all living things can be arrayed on a line leading from the simplest and most primitive to the most complex and “highly evolved,” with humans at the apex of this scale (Hodos and Campbell 1969). Although widespread, assumptions that primates are uniquely informative about human evolution are fallacious, for several reasons:

1. Evolutionary biology strives for general theories applicable to all organisms, not for specific theories that apply to only primates or hominids (or birds or beetles);
2. Many aspects of human biology are shared with all mammals, all vertebrates, or all eukaryotes, not just primates, and thus better understood from a broader perspective;
3. For such traits, primates are rarely the most convenient model species for experimental work, and depending on the level of the homology, mice, fish, or yeast may be the most appropriate model organisms;
4. A number of key human traits are unique *among primates* (bipedalism, relative hairlessness, vocal learning, . . . ), but nonetheless shared with non-primate species (dinosaurs, cetaceans, birds, . . . ). In these cases the *only* model species are non-primates;
5. Statistics requires independent events, and rigorous testing of adaptive hypotheses thus requires independent *evolutionary* events. Homologous traits present due to common ancestry represent a single evolutionary event, regardless of the number of extant species sharing the trait.

The last point is particularly relevant for language evolution, because so many of the core characteristics of human language render it unique among primate communication systems. Anyone interested in testing adaptive hypotheses for such traits is therefore forced to look outside the primate order if they wish to avoid the “Panglossian” caricature painted by Lewontin and Gould (Lewontin 1978; Gould and Lewontin 1979), where untestable adaptive hypotheses are generated post-hoc, each as (im)plausible as any other. *Testing* hypotheses involves making predictions tested with *independent* data points, with each independent evolutionary origin of the trait a single data point (Harvey and Pagel 1991).

Although some tests based on such independent contrasts are possible with primates (e.g. putative adaptive correlations between color vision and frugivory, testes size and mating system, or brain size and social complexity), many others require a far broader dataset for any valid test to be conceivable. For all of these reasons, modern biologists seek all of the relevant comparative data, whatever the clade, imposing no *a priori* limits on any particular phylogenetic dataset. Despite its lamentable frequency among psychologists and anthropologists, there is no logical or empirical basis for the stipulation that “only primate data are relevant” to the study of human evolution.

In this chapter I review multiple possible fossil cues to the evolution of human speech, drawing on comparative data from diverse living species wherever possible. I will focus on the debate surrounding the descended larynx, and touch on other proposals more briefly. My conclusions will regrettably be negative for the most part: Speech does not fossilize, and attempts to reconstruct speech from fossils must therefore build on a chain of inferences and assumptions that are only as sturdy as their weakest link. After careful consideration of various potential fossil evidence to speech, I will conclude that the best of the proposed cues provide imperfect clues to the evolutionary timing of speech evolution, and that most proposed cues tell us nothing. Nonetheless, the exercise of carefully considering such cues is worthwhile, even if they are ultimately rejected, for it offers a clear illustration of the value of hypothesis-testing using the comparative method in a biologically grounded approach to language evolution. I would suggest that this literature provides many valuable lessons for those interested in the evolution of language per se, particularly phonology, syntax, and semantics. Thus, my negative conclusions about specific proposals are nonetheless positive when viewed in the larger context of language evolution treated in this volume.

## 6.2 Comparative background

The debate about speech abilities of extinct hominids is grounded on inferences about the links between anatomy and vocal capabilities, which until recently were based almost entirely on studies of *human* speech, invoking the source/filter theory of speech production. The context for evaluating these claims has changed considerably in recent years, owing to

a considerable increase in our understanding of mammalian vocalization more broadly. Two noteworthy advances are the extension of source/filter theory (originally developed for speech) to other mammalian vocalizations, and a greater understanding of the physiological basis of mammalian vocalization (for a brief introduction see Fitch 2000b). These data have important implications for attempts to reconstruct fossil capabilities, because we must assume that extinct hominids shared any capacities that occur generally in mammals. I will thus briefly review the comparative data here; see Fitch and Hauser (2002) for more detail.

### 6.2.1 *Formants play a role in mammal communication*

Human speech is unusual among mammalian vocalizations in its dependence upon formant frequencies, both the slow-changing formant patterns of vowels, and the rapid formant transitions that constitute many consonants. Thus discussions of speech evolution naturally focus on formant frequencies as the primary acoustic cue to be understood. While any vertebrate has formant frequencies, and numerous species are now known to perceive formants, such *rapid change in formant frequencies* is a distinguishing feature of speech, and it appears to be essentially unique to our species. This is *not* because other mammals are anatomically incapable of manipulating formants. Many animals open or close their jaw in the course of a call (e.g. cats—Carterette et al. 1984), and changes in lip configuration are not uncommon (e.g. Hauser and Schön Ybarra 1994). More complex changes are, in principle, possible for most mammals (Fitch 2000c). Recent data indicate that various animals also perceive formants, without training, in their own species-specific vocalizations (Rendall et al. 1998; Fitch and Kelley 2000; Hienz et al. 2004; Fitch and Fritz 2006). At least in humans and red deer, formants have also been shown experimentally to be used as cues to estimate size (Fitch 1994; Reby et al. 2005; Smith et al. 2005). In general then, formants are present and utilized in animal vocal communication systems, and the questions revolve around the variety of formant patterns produced, not the mere possession or perception of formants.

### 6.2.2 *The mammalian vocal tract reconfigures dynamically*

To understand any peripheral limitations that might inhibit a mammal like a dog from speaking, we ask what speech sounds a dog could produce

if a human brain were in control. The simple observation that dogs do not speak tells us nothing about whether the cause of this deficit is peripheral (vocal tract anatomy, or perhaps other factors) or central (some aspect of neural control of the vocal tract). Virtually all treatises on vocal anatomy and mammals have relied upon dissection of dead animals, from Bowles (1889) to Negus (1929) to Kelemen (1963). Such discussions of vocal potential rest on an implicit assumption: that the anatomy of a dead specimen is an accurate guide to the configuration of the vocal tract in the living animal. This assumption turns out to be unjustified. When x-ray movies (termed *cineradiography*) are used to examine vocal tract anatomy in living, vocalizing animals, vocal tract configuration is found to be highly flexible and dynamic. In particular, the position of the larynx and tongue root in some mammals (such as dogs) changes actively and considerably during vocalization (Fitch 2000c). Thus, static anatomy is an imperfect guide to the physiological potential of the vocal tract in a living organism. This complicates attempts to reconstruct the details of possible articulatory movements from muscle angles in dead or anesthetized animals (e.g. Lieberman and Crelin 1971; Crelin 1987; Duchin 1990), and renders the resulting reconstructions inevitably highly controversial (e.g. Boë et al. 2002; Lieberman 2007a). This is because the main components of the system are all mobile and deformable soft tissue, and these angles can change considerably in the living organism during vocalization. We will explore the consequences for fossil reconstruction of these findings further below, but the most obvious conclusion is that a descended larynx and a “two tube” vocal anatomy, and thus many of the vocal tract conformations required for speech, are at least temporarily attainable by non-human mammals via dynamic vocal tract reconfiguration.

### 6.2.3 A permanently descended larynx exists in non-human mammals

Despite these findings, a *permanently* descended larynx was believed to be unique to humans. Recently, however, a series of anatomical investigations combined with detailed audio-video analyses have revealed a permanently descended larynx in various other mammals. The resting position of the larynx is halfway down the throat, equivalent to its position in adult humans, in adult males of both red deer *Cervus elaphus* and fallow deer *Dama dama* (Fitch and Reby 2001). Mongolian gazelles have an enlarged and permanently descended larynx, much like that of *Cervus* stags (Frey



and Riede 2003). Most significantly, all big cats in the genus *Panthera* (lions, tigers, jaguars, and leopards) also have a permanently reconfigured vocal tract (Weissengruber et al. 2002). Koalas *Phascolarctos cinereus* also appear to have a permanently descended larynx (Fitch 2002b). Crucially, in deer and gazelles, the larynx descends, but leaves the tongue root in a relatively “normal” high position, and is thus of less relevance to human speech. In contrast, in *Panthera*, the larynx and basihyoid—to which the tongue is attached—are bound tightly together, but the basihyoid is connected only by an elastic ligament to the skull (Owen 1834), precisely as in humans. Thus, in big cats, the entire tongue/hyoid apparatus descends along with the larynx, giving them a vocal anatomy that corresponds quite closely with that of humans. These comparative data suggest that the real focus of future discussion should be on the descent of the tongue root, rather than the larynx *per se*, as these two structures may in some cases be decoupled.

Despite possessing a permanently descended larynx, none of these non-human species produces speech-like sounds, with complex dynamic formant patterns. A number of possible functions for elongating the vocal tract seem possible (Fitch 1999; Fitch and Reby 2001; Fitch and Hauser 2002), but size exaggeration is the most plausible candidate explanation. The size exaggeration hypothesis for laryngeal descent holds that lowering formants (for example by retraction of the larynx) functions to increase the impression of size conveyed by vocalizations. The factual basis for this hypothesis has been explored in detail elsewhere (Fitch 2002b). Briefly, the overall pattern of formant frequencies is controlled by vocal tract length, with long vocal tracts producing low and narrowly spaced formant frequencies. Vocal tract length in many vertebrates is closely correlated with overall body size (Fitch 2000a), and formants will thus provide accurate cues to body size (Fitch 1994). This prediction has been empirically tested, and borne out, in many mammal species including monkeys, dogs, pigs, and humans (Fitch 1997; Fitch and Giedd 1999; Riede and Fitch 1999; Vorperian et al. 2005). Once formants provide a cue to size for perceivers, the evolutionary potential arises for signalers to manipulate this cue to their own advantage (Krebs and Dawkins 1984), providing the preconditions for size-exaggerating adaptations. For animals vocalizing at night, or in dense forest, such traits could have significant adaptive advantages. Thus, a plausible hypothesis for the descent of the larynx in other mammals is that it is an adaptation to exaggerate size. Crucially, the same

principles apply equally well to early hominids, suggesting that the *initial* adaptive function of the descended larynx must not have been speech. Thus, even if we discovered (say) a frozen Neanderthal specimen with a descended larynx, we could not necessarily conclude its species possessed spoken language. Furthermore, we now know that this hypothesis applies to *modern* humans at puberty, when males (only) undergo a secondary descent of the larynx (Fitch and Giedd 1999). This secondary descent of the larynx is not plausibly interpreted as an adaptation to improved speech *per se*, since female phonetic abilities are equal to, and if anything greater than those of adult males. Instead, it appears to be a size-exaggerating adaptation, precisely analogous to that found in some other male mammals (Fitch and Reby 2001).

Note that I would not argue that the permanent, primary descent of the larynx in human infants of both sexes can be explained by size exaggeration. This early developmental change is more likely either an adaptation to speech (as argued in Lieberman, Crelin, and Klatt 1972) or a by-product of some unspecified cranial rearrangements of the face and brainstem, as suggested by others (Aiello and Dean 1998; DuBrul 1977). This descent of the larynx in infancy remains the strongest evidence that human vocal anatomy reflects a tailoring of the vocal system to speech. However, we neither have skulls of infants from extinct hominids, nor are there clear skeletal cues that would allow accurate reconstruction even if we found one. The old idea that the details of the basicranium could be used for such a purpose has been rejected by its original author, Philip Lieberman, based on data gathered by his son Daniel Lieberman and colleagues (see below). These facts thus offer little additional hope for fossil reconstructions.

#### 6.2.4 *Conclusions from comparative data*

Together, the two findings of vocal flexibility and permanently descended larynges in non-human animals paint something of a bleak picture for attempts to recover the morphology and phonetic range of extinct hominids from fossils. First and most importantly, cineradiography demonstrates that the skeletal structures surrounding the vocal tract do not provide a clear indication of the shape of the vocal tract during vocalizations. The dynamic reconfiguration of the mammalian vocal tract allows a wide variety of anatomical configurations, including some closely

approximating modern human vocal anatomy, and there is thus no compelling reason to think that “standard plan” mammalian vocal tract is a crucial impediment for producing a wide variety of distinguishable phonemes. Second, the existence of non-speaking mammals with a descended larynx demonstrates that this trait can serve functions other than speech; a descended larynx is thus not necessarily *diagnostic* of speech in any species (including extinct hominids or other primates). It is furthermore clear that chimpanzees show a mild developmental descent of the larynx, unconnected with speech (Nishimura 2005). I conclude that a permanently descended larynx (or more specifically, tongue root) is neither necessary nor sufficient for spoken language. This conclusion, of course, does not entail that human vocal anatomy is irrelevant for speech—it surely is significant, in terms of the detailed phonetic characteristics of human speech. It does, however, imply that the significance of the descended larynx has been overemphasized in discussions of language evolution.

The implications of these data for fossil reconstruction of vocal anatomy are grimmer. Because only skeletal structures normally leave fossil traces, the crucial issue is whether we can predict vocal anatomy from bones. The recently recognized flexibility of the mammalian vocal tract is alone grounds for pessimism. Worse, examination of the skulls of red deer and other mammals with a permanently descended larynx and/or tongue root reveals no skeletal indicators of these soft-tissue traits. One is therefore justified in considerable skepticism regarding the accuracy of past estimates of vocal tract capabilities based on reconstructions of larynx position, and it is thus skeptically that we proceed.

### 6.3 Reconstructing the vocal abilities of extinct hominids

I now come to the core topic of this paper: fossil indicators of the speech capacities of extinct hominids. Most attempts at human vocal tract reconfiguration have focused on the use of the basicranium as an indicator of laryngeal position, but other possible fossil clues to vocal capability have been proposed. I will first address the basicranial hypothesis, and then other more recent suggestions, before summarizing the implications for fossil hominids.

### 6.3.1 *The vocal tract skeleton: the mammalian basicranium and hyoid apparatus*

Most of the muscles and ligaments that make up the vocal tract are attached to the basicranium and/or the hyoid apparatus. The basicranium refers to the bottom of the skull, one of the most complex regions of the body. It is a structure with very ancient affinities: most components of it can be traced back to the earliest jawed vertebrates. A comparison of the basicranium of a shark and a human reveals a remarkable conservation of the pattern of bones, pierced by many holes termed *foramina* (sing. *foramen*) for blood vessels and nerves, and the muscles which attach to them. The core of the basicranium is formed quite early in development, in cartilage, and is thus termed the *chondrocranium*, while most of what you see when you look at a skull (especially the skull cap or cranial vault, and much of the *facial skeleton*, which includes the jaws, cheekbones, and bones surrounding the eyes), develops later directly as bone laid down by skin-like epithelia and is thus called the *dermatocranium*. The largest foramen (helpfully termed the *foramen magnum*) is in the back of the basicranium and forms the passageway where the spinal cord and lowermost brainstem enters the braincase. This opening is flanked on either side by the uppermost joint between spinal column and skull, and it is on this *occipital joint* that the entire skull is balanced in an upright human being. The bone containing the foramen magnum and occiput is termed the basioccipital bone. Moving forward from the occipital we find the temporal, sphenoid, ethmoid, and nasal bones, and finally the vomer, maxilla, and premaxilla which make up the hard palate and upper jaw.

The hyoid apparatus is a derivative of several branchial arches (homologous to the gill bars of fish), consisting of several “loops” of cartilage or bone, the uppermost attaching to the basicranium much like the jaw (which is the frontmost branchial arch derivative). The upper *epihyal* portion of the hyoid apparatus is highly variable between species. In some large herbivores (e.g. horses or sheep) it is extremely robust, resembling a jawbone, and anchors the tongue root solidly to the skull base. In most carnivores, rodents, and bats, the epihyal is a chain of slim bones, surrounded by muscles, that forms a more flexible link to the skull base. In others, including primates and big cats, the epihyal is reduced to ligament and muscle. The lower *basihyal* portion forms the functional core of the hyoid apparatus, centering on the U-shaped basihyoid bone (often simply

termed the “hyoid” in humans). In several mammalian groups, including primates, some bats and rodents, and some big cats, most of the hyoid apparatus is reduced, and only the basihyoid is fully ossified. In such cases the hyoid bone is essentially a free-floating bone, attached only by non-bony tissues, via a three-point suspension, to the rest of the skeleton. The basihyoid is thus a rather unusual bone. This bone forms a solid, bony anchor for the intrinsic muscle of the tongue, as well as most of the other muscles of the vocal tract, and is present in all mammals.

### 6.3.2 *Basicranial angle*

Because the basihyoid bone is the support for both the tongue root and the larynx attached below, it has a special role in attempts to reconstruct fossil vocal tracts. If we could reconstruct its position from cranial bones (for example, using the angle of the styloid process, the upper anchor for the hyoid apparatus) we could determine if the hyoid had descended (Lieberman and Crelin 1971). Unfortunately, the styloid is extremely variable, even in adult modern humans, and does not seem to be well suited for this task (DuBrul 1977). Thus, more general correlations between basicranial shape and hyoid position have been the focus of these discussions. In particular, the basicranial angle, one measure of the configuration of the basicranium, has long been held to provide such a clue (George 1978; Laitman et al. 1978; Crelin 1987). The basicranial angle is measured from several well-defined cephalometric landmarks (the basion, opisthocranion, and nasion). Citing an apparent correlation between this angle and the reconfigured human vocal tract, George and subsequent scholars concurred in placing the hyoid, tongue base, and larynx of fossil hominids high in the throat, in the position found in apes or newborn humans. Thus, it was argued the reconfiguration of adult human vocal anatomy is a very recent evolutionary acquisition. This was a plausible argument, and despite many criticisms, the idea continues to be cited and trusted by many in the contemporary literature, even appearing in textbooks (e.g. Aiello and Dean 1998). It is thus important to understand precisely why it is incorrect.

The central question is whether basicranial anatomy predicts hyoid height. Despite some interesting investigations suggesting that surgical rearrangements of the rat basicranium results in slight laryngeal lowering (Laitman and Reidenberg 1988), the correlation is at best imperfect.

Careful developmental analyses of basicranial angle from longitudinal x-rays of growing children suggest that laryngeal descent is decorrelated from several measures of basicranial angulation (Lieberman et al. 2001), and the additional pubertal descent of the larynx in males does not correlate with any change in basicranial conformation (Fitch and Giedd 1999). Both the initial descent of the infant hyoid, and later pubertal descent, appear to have no significant relation to basicranial angle. Thus, even in our own species, the claimed relationship is not predictive, as has been recently accepted by Philip Lieberman (Lieberman 2007a).

The comparative data reviewed above are even more problematic. First, in the species that have been recently discovered to have descended larynges and/or hyoids, there are no documented changes in basicranium associated with this (indeed, it seems unlikely that the basicranium of a maturing deer stag could make any major rearrangements so late in development). Second, and most crucially, we know that dogs and other mammals can make large movements of the vocal apparatus and tongue base over seconds, during vocalization, so even if the *resting* position of the hyoid could be estimated, this would not determine the actual position of the vocal tract during vocalization. It would be very surprising if such flexibility did not characterize fossil hominids as well. Thus, a Neanderthal or Australopithecine might have had a high resting hyoid and larynx, with the advantages for breathing, chewing, and/or swallowing that this “standard” mammalian position presumably confers. But this high position would not preclude them from lowering these structures into a modern human conformation during vocalization, just like other extant mammals (Fitch 2000c), and thus to produce a wide variety of formant patterns. For all of these reasons, there appears to be little remaining empirical basis for reconstructing the phonetic abilities of fossil hominids (or other mammals) from their basicrania.

### 6.3.3 *Other proposed fossil cues to larynx height*

I will only briefly review some other potential fossil indicators of larynx height, which appear to me less plausible.

#### 6.3.3.1 Bipedalism

An oft-repeated idea is that the simple attainment of upright bipedalism is alone enough to drive the larynx downward (e.g. Negus 1949; Falk 1975).

This suggestion is implausible at several levels, as bipedalism and upright posture have evolved in parallel in many animal species, including all birds, without any concomitant descent of hyoid or larynx. Mechanistically, we know that experimentally enforced bipedalism in rats does not cause vocal tract rearrangements (Riesenfeld 1966). If gravity could pull the larynx down due to bipedal walking, over evolutionary time, one would certainly expect the vigorous bipedal hopping of kangaroos and other macropodid marsupials to do the same, but none of these species appear to have descended larynges. They do, however, show other convergent anatomical traits with modern humans such as a closed inguinal canal (Coveney et al. 2002), traits which appear to result directly from the forces experienced during bipedal locomotion. Many non-human primates adopt an upright posture while resting and feeding, and arboreal primates like gibbons or spider monkeys spend most of their lives in a vertical position, but none have a descended larynx. This suggests that neither upright posture nor bipedalism alone is enough to drive hominid vocal tract reconfiguration. Another unconvincing idea is that tongue length must remain constant, and that facial shortening during hominid evolution “pushed” the larynx and hyoid downward (DuBrul 1958). However, there is no reason to believe tongue length must remain constant over evolutionary time: Various dog and cat breeds with highly shortened snouts (such as bulldogs) have been subjected by breeders to radical skeletal changes over a very short evolutionary period, but do not appear to undergo any correlated hyoid or laryngeal descent. Thus, from a purely mechanistic or geometrical perspective, there is no reason to think that bipedalism automatically enforces laryngeal descent. If bipedalism influenced human vocal anatomy, it must have done so in concert with some other more specific trait, and over a longer stretch of evolutionary time.

#### 6.3.3.2 Bipedalism “plus”

A more persuasive hypothesis combines two independent factors, suggesting that the reconfiguration of the human skull, partially associated with bipedalism, combined with facial reduction to force the larynx downward. Although their rationales have varied, several researchers have suggested that these skeletal changes conspired to “squeeze” the tongue root and larynx downward (Aiello 1996). The most important change in modern human cranial anatomy relative to other apes, and most fossil hominids, is a retraction of the facial skeleton relative to the rest of the skull. While the

face and jaws of a chimpanzee jut far forward from their braincase, those of humans are pulled backwards and are almost flush with the forehead. This change has far-reaching consequences for skull form, including a relative shortening of the oral cavity (the main cause of our frequently impacted wisdom teeth) and a lessening of the space between the back of the palate and the front of the spinal column (Aiello and Dean 1998). The latter change is exacerbated by the forward movement of the foramen magnum and spinal column to the more “balanced” position associated with fully upright bipedalism. While a chimp’s foramen magnum points backwards (reflecting the forward-jutting head posture), ours points almost directly downward, so the cervical spine moves closer to the facial skull and internal nares. This might suggest that there is insufficient room in the posterior bony oral cavity for the typical mammalian naso-laryngeal seal to be formed. It is difficult at present to refute this hypothesis based on comparative data, for I know of no non-human species that combines facial shortening with bipedalism, and we are thus left with an *N* of one which allows no hypothesis testing. However, I find this idea unconvincing, because in ordinary mammals the larynx is never inserted directly into the actual bony nares. The naso-laryngeal “seal” is formed by the soft tissues of the velum (= soft palate) and epiglottis, not the bony nares and thyroid cartilage. The relevant velar aperture can thus be larger than that observed on the skull itself. I conclude that there is at present little reason to believe that bipedalism, whether alone or combined with other factors, would drive a descended larynx or reconfigured vocal tract, either automatically or via some possible respiratory advantage.

### 6.3.3.3 Cervical vertebrae

Since the collapse of the empirical foundation for basicranial cues, Lieberman has provided a new hypothesis concerning Neanderthal vocal limitations (Lieberman 2007b). He suggests that a two-tube vocal tract with approximately equal dimensions of oral and pharyngeal tubes would require a Neanderthal to have its larynx across from the first thoracic vertebra, and thus a larynx in its thorax, like no other primate, and concludes that such vocal anatomy would be impossible. This conclusion seems to be based on the mistaken assumption that the opening to the thorax is directly across from the first thoracic vertebra. But the opening is below this, because the first rib slopes downward. The upper portion of the sternum, which marks the thoracic inlet, and to which attach the



laryngeal retractor muscles, the sternothyroid, and sternohyoid, is across from T3 or T4, and a quick examination of any adult male shows that there is ample room for further laryngeal retraction in modern humans, to compensate for an oral cavity 1 cm longer than our own. The claim that a larynx within the thorax is “impossible” is falsified by the male hammer-head bat *Hypsignathus monstrosus* which has a larynx contained entirely within the thorax. More tellingly, the attachments of the sternal retractor muscles (sternothyroid and sternohyoid) are to the thyroid and hyoid cartilages in all mammals, not to the base of the larynx (and in some species, like the koala, the sternal insertions are lowered to inside the thoracic cavity as well). These anatomical facts suggest that a mammal, including a Neanderthal, could retract its larynx base to slightly below the sternal head while it vocalizes. Thus both human anatomy and comparative data cast doubt upon Lieberman’s new hypothesis.

#### 6.3.4 *The hyoid bone and the loss of air sacs in hominid evolution*

Another new clue to the vocal anatomy of fossil hominids was provided by the discovery of a Neanderthal basihyoid at Kebara, Israel (Arensburg et al. 1989, 1990; Arensburg 1994). The Kebara hyoid is quite robust (as is the entire Neanderthal skeleton), but otherwise appears modern in structure, and was thus argued to provide support for the notion that Neanderthals had a modern vocal anatomy and low larynx. But, as critics quickly pointed out (Laitman et al. 1990), this argument is a *non sequitur*. The morphology of the hyoid bone does not itself determine its position in the primate vocal tract; this is determined by the muscles and ligaments that form its three-point suspension. If the sternohyoid muscles are tensed, the hyoid will move downward (as seen during dog barking), while if the digastric and stylohyoid are tensed it will move upward. The anatomically modern hyoid of a human infant is consistent with its high position, and no changes in hyoid structure are entailed by the secondary pubertal descent of the hyoid in human males. Thus, the modern morphology of the Neanderthal hyoid provides no indication of its position, high or low, in the Neanderthal throat.

However, the hyoid bone *does* provide an interesting clue concerning the loss of air sacs in the hominid lineage. Because all great apes possess laryngeal air sacs (Fitch and Hauser 1995; Hewitt et al. 2002), a firm comparative inference is that these structures were lost during human

evolution. The chimpanzee or gorilla hyoid is very different from that of a modern human, or the Kebara hyoid. In these apes, the basihyoid balloons into a thin-walled shell (the hyoid bulla), into which the laryngeal air sacs extend. Such bullae are typically observed in primate species with air sacs (e.g. in most Old World monkeys), including the huge bulla of the howler monkeys in the genus *Alouatta* (Kelemen and Sade 1960; Hilloowala 1975). Based on their modern hyoid anatomy, Neanderthals had probably already lost their laryngeal air sacs, as had their presumed ancestors *Homo heidelbergensis*, whose hyoids recovered recently at Atapuerca are also non-bullate (Martínez et al. 2008). The recent discovery of an Australopithecine basihyoid bone in Dikika, Ethiopia, closely resembling that of a chimpanzee (Alemseged et al. 2006), strongly suggests that Australopithecines retained the air sacs seen in other great apes. Unfortunately, however, the bulla/air sac correlation is imperfect: Orangutans have very large air sacs, but do not have a hyoid bulla (Aiello and Dean 1998); and some colobines (genus *Procolobus*) have a bullate hyoid and no air sac (Hill and Booth 1957). Nor is the occasional pathological appearance of laryngeal air sacs in humans (termed *laryngocele*) associated with changes in hyoid structure (Micheau et al. 1978). Despite this, the combination of the Dikika, Atapuerca, and Kebara finds provides the best indication of when air sacs disappeared in the hominid lineage: sometime in the last 2 million years. We can only hope that *Homo erectus* and/or *ergaster* hyoid bones can be recovered to further clarify this question. For now, this is probably the firmest conclusion that can be drawn, based on fossil data, about vocal tract anatomy in the hominid lineage leading to humans.

#### 6.4 Neurally-based fossil cues to speech evolution?

Most commentators agree that regardless of any peripheral limitations on vocal production, human speech production requires changes in neural control of vocalization (Darwin 1871; Fitch 2000b; Lieberman 2007b). The failure to find skeletal clues to vocal anatomy has led several authors to propose alternative cues based on fossil-based estimates of the size of neural structures. While there is a large literature attempting to link brain size and form to the evolution of language per se (again highly controversial), this literature is beyond the scope of the present review. Briefly, we

have excellent fossil data about how overall brain volume expanded (for review see Holloway et al. 2004), but no solid cues to language more specifically are accepted in the paleoneurological literature. However, some potential cues are available at more peripheral levels.

#### 6.4.1 *Hypoglossal canal size*

The hypoglossal canal is the foramen in the skull base through which the nerve supplying most tongue muscles passes. Potentially, its size could be used to estimate the degree of tongue control (Kay et al. 1998). This is a plausible, logically well-founded hypothesis, given the central importance of tongue control in speech, and the fact that tongue movements appear to play little role in most other mammal vocalizations. Kay and colleagues' initial measurements suggested that humans have a disproportionately larger canal than other great apes, and suggested smaller canal sizes in Australopithecines. Unfortunately, later more detailed measurements showed that there is in fact great variability in canal size in humans, along with substantial overlap between humans and other apes (DeGusta et al. 1999). This variability appears to result from the fact that considerable non-nervous tissue (including blood vessels) passes through this opening. But even the size of the hypoglossal nerve itself seems to provide no clear clues to vocal ability, with chimpanzee nerve size safely within the human range. The authors of the original study now concur that the empirical basis for their original conclusion is weak (Jungers et al. 2003). The speed with which Kay and Cartmill's initial plausible proposal was falsified, and the admission of this by its original proponents, represents a commendable and refreshing exception to the usual pattern of debate in this field, and shows how the presentation of strong hypotheses that make testable predictions can lead to real scientific progress, even when the original proposal is rejected. However, it should be noted that important unknowns remain about the issue of tongue control that can be easily addressed comparatively. Although the gross musculature of the tongue itself is very similar in apes and humans (Takemoto 2001), the ratio of nervous motor fibers to muscle fibers might be different, and this would represent a relatively peripheral source of constraint on ape vocal abilities. Surprisingly, both tongue size and the relative distribution of nerve fibers to different components of the complex tongue musculature remain unstudied across primates, to my knowledge.

#### 6.4.2 *Thoracic canal size*

The final proposed fossil clue to hominid vocal control I will consider here is the only one that appears, at present, to be plausible. This is the enlargement of the thoracic canal in modern humans (and Neanderthals), relative to other primates or earlier fossil hominids (especially early African *Homo erectus* a.k.a. *Homo ergaster*). This difference has been carefully documented by comparative anatomists Ann MacLarnon and Gwen Hewitt (MacLarnon and Hewitt 1999, 2004). In general, the muscles of the body are controlled by motor nerves contained within the spinal cord at the corresponding level. So the muscles controlling arms and hands are controlled by motor neurons in the cranial spinal cord, while the neurons controlling the legs and toes are ensconced in the lumbar portions of the cord. Crucially, many of the muscles involved in breathing in tetrapods are fed by neurons in the thoracic spinal cord. These include the intercostal muscles, which expand and contract the rib cage and form the main breathing muscles in reptiles and birds, and important accessory muscles in mammals, along with the abdominal muscles that are important in powerful expiration in all of these amniote groups. Based on a large number of careful measurements of thoracic canal diameter in extant primates, and controlling for overall body size, MacLarnon and Hewitt found that humans have significantly enlarged thoracic spinal cords, and thus enlargement of the thoracic canal in the spinal bones that surround this portion of the cord. Given the well-documented role of the intercostals and abdominal muscles in fine control of lung pressure during speech and singing (Ladefoged 1967), these researchers suggested that the enlargement of the thoracic canal represents an adaptation to fine vocal control and speech.

MacLarnon and Hewitt's data provide a nice example of how laying a careful foundation in comparative data can support inferences from fossils. They carefully examine several alternative hypotheses for why the thoracic cord might have expanded (e.g. increased control of throwing, or better breath control during walking) and convincingly reject them. Thus, their hypothesis that the thoracic canal provides a fossil cue relevant to breath control seems both plausible and well supported at present. This is the good news. The bad news is that our fossil evidence for thoracic canal size is quite limited, as vertebrae are not typically well preserved in the fossil record. The truly solid data come from living primates and modern

humans, and the fossils from one *Homo ergaster* skeleton (the “Nariokotome Boy”) and several Neanderthal specimens. We can thus only say that thoracic expansion occurred sometime in the million-year period of evolution between these specimens, e.g. in post-*ergaster* *Homo erectus* or later (e.g. in the common ancestor of modern humans and Neanderthals, sometimes thought to be *H. antecessor* or *H. heidelbergensis*). The vast trove of fossil *heidelbergensis* bones uncovered in the Atapuerca region of Spain at the “Sima de los Huesos” offer promise in this regard (Gómez-Olivencia et al. 2007). If the newly discovered fossils dubbed *Homo floresiensis* turned out to represent a new hominid species (Brown et al. 2004; but see Martin et al. 2006), it would be interesting to know if they had an expanded thoracic canal. We can hope that over time additional vertebral specimens will be discovered to help fill the current fossil gap.

Accepting provisionally the hypothesized link between breath control and thoracic canal expansion, what further inferences can be drawn about speech and language? First, it is important to note that the thoracic canal does not control all breathing muscles. The diaphragm, which is the most important inspiratory muscle in mammals, and is unique to mammals, is controlled by neurons higher in the spinal column, rather than the thoracic column (this is correctly noted by MacLarnon and Hewitt but sometimes misstated in the literature, e.g. Mithen 2005). During normal quiet breathing, and quiet speech, the diaphragm is the only muscle required to inflate the lungs, and elastic recoil of the lungs is adequate to power respiration. Thus, the thoracically controlled musculature is both phylogenetically more ancient, and not as central to normal breathing as the diaphragm. However, control over airflow during speech (or singing) does seem to depend more on thoracic musculature.

More importantly, the increased breath control associated with the thoracic musculature has at least two possible functions other than (or in addition to) speech. The first alternative follows from the suggestion that long-distance running has played an important and neglected role in human evolution (Carrier 1984; Bramble and Lieberman 2004). These authors note that biomechanical coupling between breathing and running plays an important role in determining the efficiency with which quadrupeds can run. This leads to a biomechanically constrained optimal rate of running for most species, and deviations from this optimum are quite costly to quadrupeds (there are narrow “tuning curves” of breathing rates

for trotting, cantering, and galloping). Bipedal running relaxes these constraints to some degree, allowing us bipeds much more flexible running and breathing paces. Bramble and Lieberman suggest various fossils cues indicating that sustained running first became a major factor early in the evolution of the genus *Homo*, though the precise point remains unclear due to inadequate fossil coverage (Bramble and Lieberman 2004). Because running typically involves deep respiration, with the accessory muscles recruited, the need to “override” biomechanical constraints on breathing rate could require an expansion of the motor neurons controlling the thoracic respiratory apparatus. Thus, the “sustained running” hypothesis is another possible alternative explanation for the expansion of the thoracic canal in early *Homo*.

MacLarnon and Hewitt consider this hypothesis, but reject it based on the claim that *H. ergaster* was already an endurance runner but lacked thoracic enlargement (MacLarnon and Hewitt 1999: 349). However, since the exact time of appearance of leg anatomy required for endurance running remains unclear, I think this hypothesis deserves continued consideration. It could potentially be tested by comparing highly cursorial bipedal birds (e.g. ostriches or roadrunners) and those dependent almost entirely on flight (e.g. swifts), examining their control over vocalization (e.g. in an operant situation), or a proxy such as the complexity of vocalization. Another crucial comparative perspective could be provided by diving mammals, because breath control is also presumably a prerequisite for aquatic mammals. The MacLarnon/Hewitt hypothesis would thus predict an expansion of the thoracic canal in seals, otters, and polar bears, or in aquatic rodents, relative to their more-terrestrial relatives. Does this too result in increased vocal control? This seems plausible, but it is another case where the comparative data relevant to central questions in human evolution are presently unavailable.

A second alternate possibility for thoracic enlargement is provided by the fact that the breath control required for singing is at least as demanding as that for speech. Johan Sundberg has convincingly argued that singing in fact requires finer respiratory control (Sundberg 1987; see also Fitch 2005). In normal conversational speech, the rate of airflow is around 0.2 liters/second (0.1 to 0.3 l/s) and approximately 2 l tidal volume are utilized. With no involvement of the intercostals, and simple passive lung deflation, this would give 10 s of normal speech. But speakers normally breathe every 5 s. In contrast, phrases over 10 s are common in song, and

singers often use nearly all of their approximately 5 l vital capacity. Furthermore, much greater subglottal pressures are generated during singing than speech (6–70 cm water relative to 6–15 cm water in normal speech). Most importantly, the finer control over amplitude and pitch necessary in singing requires singers to use all major respiratory muscles (including both sets of intercostals, the diaphragm, and the abdominal muscles), while speech typically requires the use of only one set of intercostals for compensatory maneuvers. Thus, an increase in fine respiratory control would seem to be more important in singing (where, in modern practice, maintaining a constant and accurately controlled subglottal pressure for consistent amplitude and pitch is a necessity) than for speech (where pitch is in any case varying continuously over a wide range). This fact is directly relevant to those hypotheses for the evolution of speech that, following Darwin, hypothesize that true meaningful speech was preceded phylogenetically by a song-like system (Darwin 1871; Livingstone 1973; Brown 2000; Marler 2000; Mithen 2005). If such hypotheses are taken seriously (as I believe they should be, see Morley 2003; Fitch 2006), the enlargement of the thoracic canal might just as clearly signal the evolution of human *singing* as of speech.

## 6.5 Conclusions

In this chapter I have discussed the various attempts that have been made over the years to reconstruct the anatomy of the vocal tract based on fossil remains. It is a simple but unfortunate fact that the key tissues of the vocal tract do not fossilize, and any possible reconstructions are perforce based on quite indirect lines of evidence. Most of these attempts at vocal tract reconstruction fail to stand up to empirical scrutiny. Traditionally, attempts to do this paid little serious attention to comparative data, and recent comparative data have rendered any potential fossil clues to vocal tract anatomy even more tentative. If these combined data on mammalian vocal production are taken seriously, the alluring dream that scientists can reconstruct the vocal anatomy of an extinct hominid from skeletal remains appears to be unrealistic. Furthermore, even if by extraordinary luck we discover a frozen Neanderthal in the melting ice of a glacier, the mere presence of a descended larynx or tongue root would not necessarily demonstrate the possession of spoken language (any more than the

reconfigured vocal tract of a lion demonstrates speech in that species). Nor, given the flexibility of the mammalian vocal tract, would a high laryngeal position demonstrate that the Neanderthal didn't speak: He or she might have lowered the larynx and tongue root dynamically during vocalization, as do many other mammals today. Although there seems, in principle, to be more hope of reconstructing potential neural control structures, even the most solid current example (MacLarnon and Hewitt's thoracic canal hypothesis) can only support limited phylogenetic inferences (though as one source of converging data it is quite important).

In a previous briefer review of this literature, I concluded that "this line of inquiry appears to have generated more heat than light, and diverted attention from alternative questions that are equally interesting and more accessible empirically" (Fitch 2000b: 263), and I stand by this conclusion today. Despite a voluminous literature, no confident assertions can be made about hominid speech anatomy and/or speech motor control beyond the obvious one based on comparative data: Human vocal anatomy and, more crucially, motor control have undergone important changes, sometime since our divergence from chimpanzees about 6–7 million years ago. Precisely when this happened, and why, remains unclear. What is furthermore clear from the comparative data is that the significance of the descent of the larynx for these evolutionary changes has been overestimated. More generally, the venerable hypothesis that the inability of most mammals to communicate via spoken language results from limitations in their peripheral morphology does not appear sustainable. By process of elimination, the crucial changes in the evolution of speech appear to be neural rather than peripheral.

Thus, my conclusion is regrettably a negative one: Little can be firmly concluded about the timing of the evolution of speech based on fossil cues. In my opinion, approaches based on DNA analysis offer far more promise for dating key events in the evolution of speech and language (Carroll 2006; Enard et al. 2002; Krause et al. 2007a). It is worth emphasizing, however, that this negative conclusion represents real scientific progress, and that this progress is thanks directly to researchers like Lieberman and Crelin, or more recently Cartmill and Kay, who were unafraid to put forth bold, creative hypotheses specific enough to be testable. Without such hypotheses, science would go nowhere. My own observations of vocal tract reconfiguration in mammals would never have occurred if not for Lieberman's hypotheses about the descent of the



larynx. When testable hypotheses drive scientists to notice otherwise obscure details, or to gather new data, science progresses forward, even if some beautiful theories get trampled by new facts. Thus the conclusion of this chapter is negative only in a limited sense. For the field of language evolution writ large, these hypotheses and data provide a model for how empirical progress can be made: via the generation and testing of strong, falsifiable hypotheses.

# 7 Evidence against a genetic-based revolution in language 50,000 years ago

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KARL C. DILLER AND REBECCA L. CANN

## 7.1 Introduction

Africa was more than just the “cradle” of language, a place of nurture for language and for the anatomically modern humans who have had full capacity for language for more than 200,000 years. From an evolutionary viewpoint we would think of Africa also as something of a “womb” for language, nurturing the embryonic beginnings of language as it emerged in archaic species of *Homo*, and fostering the gradual co-evolution of language and the complex brain structures necessary for speech and fully modern language. Form and function usually evolve together. We would expect the beginnings of speech and language to undergo positive natural selection. Brain structures which improved speech and language would thereby be selected for. Control of the vocal apparatus is highly complex, involving tongue, lips, vocal cords, breathing, etc.; gaining the neural ability to exercise conscious control of all this was not likely the result of a single mutation. But many people speculate or argue that there was a revolution in language, one important genetic mutation for language, about 50,000 years ago, that brought about a revolution in culture and allowed modern humans to leave Africa for Europe and the rest of the world.

Revolution in language vs. co-evolution of language and brain—this has been a serious issue in linguistics and anthropology, especially since 2002 when an argument from genetics supported the revolution point of view (Enard et al. 2002), while recent fossil evidence is more consistent with co-evolution. We argue here for the long-range co-evolution scenario, and we present genetic evidence that the mutations in FOXP2, the gene at issue, may actually have occurred some 1.8 million years ago, when *Homo habilis*

and *Homo ergaster* were appearing in the fossil record, and as the human brain began gradually to triple in size from the 450 cc of chimpanzee and australopithecine brains to the 1,350 cc of modern human brains.

One year after we presented this argument at the Cradle of Language Conference in November 2006, dramatic support came in evidence that Neanderthals shared the modern human mutations in FOXP2 (Krause et al. 2007a), evidence that we discuss below in section 7.7. Our argument would have predicted this result.

## 7.2 A revolution in language?

The case for a revolution in language causing a revolution in culture has been made most forcefully by Richard Klein (1999). It is a position that Klein maintains even though the FOXP2 evidence no longer supports him (Wade 2007). In Europe there was a big discontinuity between the minimal culture of the Neanderthals and the modern culture of the Cro-magnon immigrants. Looking at Africa, Klein argued that around 50,000 years ago there was a similar revolution in modern behavior, as seen in a dramatic increase in the number of cultural artifacts that required symbolic thinking, and in the migration of modern humans into Europe, where they produced their spectacular cave paintings. Then Klein posited a biological cause for this behavioral change. In a lecture posted on the internet he says, “I imagine that what happened 50,000 years ago was a highly advantageous mutation that produced a brain in which these things, these different parts were now very much better wired together, something of that sort. And then we have language as we understand it and this rapid spread from Africa and all the cultural innovations that obviously depended upon language and that allowed this spread from Africa” (Klein 1997).

Spreading from Africa was not, in fact, quite that difficult. Neanderthals went to Europe well before modern humans did, and *Homo erectus* had reached Dmanisi in Eurasia between the Black and Caspian seas ~1.7 million years before *Homo sapiens* left Africa.

Migration of modern humans out of Africa to South Asia and Australia was also earlier than 50,000 years ago, and much earlier than migration to Europe. The Andaman Islands in the Indian Ocean were settled some 65,000 years ago (Macaulay et al. 2005), and Australia may well have been

settled by 62,000 years ago (Thorne et al. 1999). The Andaman Islanders have been genetically isolated for 65,000 years, and have full capacity for language. Any mutations important for modern language must have spread through the human population before the migrations to Asia and Australia and before the genetic isolation of the Andaman Islanders.

There is strong evidence from archeology and physical anthropology that there was no sudden revolution in symbolic culture 50,000 years ago. McBrearty and Brooks exhaustively documented this fact in their article “The revolution that wasn’t” (McBrearty and Brooks 2000). Artifacts that imply symbolic culture, such as engraved pieces of ochre (Henshilwood et al. 2002) and shell beads (d’Errico et al. 2005), are found as far back as 77,000 years ago at Blombos Cave in South Africa along with bone tools showing formal production techniques, fine bifacial points crafted well beyond mere utilitarian needs, and evidence of sophisticated subsistence strategies (Henshilwood et al. 2001b). Similar shell beads in a similar cultural context are found at the other end of Africa, in Morocco, dating to 82,000 years ago (Bouzouggar et al. 2007), showing that humans and symbolic human culture were widespread in Africa at that time. Use of pigments in a human cultural context goes back some 270,000 years (McBrearty and Brooks 2000).

Two skulls of anatomically modern humans, *Homo sapiens*, have been dated to  $195,000 \pm 5,000$  years ago (MacDougall et al. 2005). It seems unlikely that 150,000 years after the emergence of anatomically modern humans we would have a major change in neuroanatomy that is undetectable in the fossil record. The most recent female common ancestor of all living humans is also dated to about 200,000 years ago as calculated through comparison of mitochondrial DNA (Cann et al. 1987). It also seems unlikely that a major mutation for language would come after the date of our most recent common ancestor in the female line.

Language, after all, is a defining characteristic of *Homo sapiens*. An evolutionary point of view would argue that the capacity for language was fully developed in the first anatomically modern humans at least 200,000 years ago. And if the capacity for language co-evolved with languages themselves, then we would expect to have fully modern languages at least 200,000 years ago.

The archeological evidence may push the beginnings of modern humans and human culture back even further. As McBrearty and Brooks sum it up, “The appearance of Middle Stone Age technology and the first

signs of modern behavior coincide with the appearance of fossils that have been attributed to *H. helmei*, suggesting the behavior of *H. helmei* is distinct from that of earlier hominid species and quite similar to that of modern people. If on anatomical and behavioral grounds *H. helmei* is sunk into *H. sapiens*, the origin of our species is linked with the appearance of Middle Stone Age technology at 250–300 ka” (McBrearty and Brooks 2000).

The genetic evidence purporting to support Klein’s date of 50,000 years ago comes from an article by Enard et al. (Enard et al. 2002), “Molecular evolution of FOXP2, a gene involved in speech and language.” Interestingly, science writers have used this paper to support both the dates of 50,000 years ago and 200,000 years ago. Nicholas Wade in the *New York Times* writes that “The finding [of Enard et al.] supports a novel theory advanced by Dr. Richard Klein, an archaeologist at Stanford University, who argues that the emergence of behaviorally modern humans about 50,000 years ago was set off by a major genetic change, most probably the acquisition of language” (Wade 2002). But Richard Dawkins, in *The Ancestor’s Tale* (2004) says, “And the answer for FOXP2 is less than 200,000 years ago. A naturally selected change to the human version of FOXP2 seems roughly to coincide with the change from archaic *Homo sapiens* to anatomically modern *Homo sapiens*. The margin of error in this sort of calculation is wide, but this ingenious genetic evidence counts as a vote against the theory that *Homo ergaster* could talk.”

What, then, was the date that Enard et al. calculated as most likely for the human mutations in FOXP2? The most likely date, they say, is zero years ago. Zero years ago with a 95% confidence interval stretching back to 120,000 years ago.

If the date of zero years ago doesn’t raise some eyebrows, then the suggestion that the date of zero supports any date we choose between now and 200,000 years ago should. It is clear that we need to look at the fine print. And we need to start by asking what is special about FOXP2.

### 7.3 What is so special about FOXP2?

In three generations of the KE family in London, about half of the family members had a severe speech and language defect (Hurst et al. 1990). The pattern of inheritance suggested that it was caused by a dominant

autosomal allele form of a gene some called the grammar gene, though it quickly became apparent that the speech and language defect was not primarily one of grammar, but of control of the oro-facial muscles necessary for the articulation of speech (Watkins et al. 2002). It is not surprising to find the association of speech production problems and grammar; that's what we find in Broca's aphasia. In the KE family, however, much more was involved. An MRI study found changes (positive and negative) in a variety of brain structures including subcortical structures and the cerebellum (Watkins et al. 2002). The gene was identified as FOXP2 because of its similarity to a family of the forkhead box transcription factors that are involved in turning on and off the expression of other genes. FOXP2 is expressed in a variety of adult and fetal tissues in addition to the developing brain. The one mutation in that gene found in the KE family essentially prevented that copy of the gene from functioning. Since we each have two copies of every gene, one from our mother and one from our father, the affected members of the KE family had only half the usual amount of the FOXP2 protein expressed.

The link of FOXP2 to vocalization is found also in birds, mice, and echolocating bats as well as in the KE family. The expression of FOXP2 is upregulated during the post-hatch learning of birdsong (zebra finch) or during singing season (canary) (Haesler et al. 2004). Ultrasonic vocalization in infant mice is significantly decreased if one copy of FOXP2 is deleted; if both copies are deleted, there is no vocalization and premature death (Shu et al. 2005). Echolocating bats (as opposed to non-echolocating bats) have an unusually high number of non-synonymous mutations in FOXP2 (Li et al. 2007).

It turns out that FOXP2 is a highly conserved gene with no amino acid changes in the chimpanzee line going back some 90 million years to the common ancestor with the mouse. This resistance to change suggests that FOXP2 is extraordinarily important for vertebrate development and survival. But there are two mutations that change amino acids in FOXP2 in the 6 million years of the human line since the common ancestor with the chimpanzee (Enard et al. 2002). Does this suggest that these two mutations might have something to do with the development of the capacity for speech and language in humans? One of the human-specific mutations is shared with wolves, tigers, and all members of the order *Carnivora* (Zhang et al. 2002). Of the two mutations, this is the one which Enard et al. suggest might make the most difference in protein function by

providing a potential site for phosphorylation. Phosphorylation is a post-translational modification of a protein and is often associated with functional activation.

But in spite of the counter example of wolves and tigers, it could still be reasonable speculation that the human mutations in FOXP2 may have been important in the evolution of the human capacity for language because of the link of FOXP2 to vocalization in birds, mice, and in the KE family. Still, this is speculation, and we have to recognize that FOXP2 has important vital functions in all vertebrates—non-linguistic functions so vital that there have been no amino acid changes in FOXP2 in the last 90 million years of the chimpanzee line.

All humans, even the members of the KE family, share the two mutations in FOXP2 that distinguish us from chimpanzees. And whether there is any connection or not, we can't help noticing that we are also distinguished from chimpanzees in the ability to speak. A chimpanzee infant, Vicki, was raised in diapers in the household of two psychologists and subjected to rigorous behaviorist training to get her to speak (Hayes and Hayes 1952). After months of training, Vicki succeeded not in speaking but in being able to whisper only four words, mama, papa, cup, and up, using her hand to close her lips for the labial sounds and making gyrations with her body showing great effort. It seems clear that chimpanzees do not have the neuromuscular control to speak human languages or even to imitate human speech.

If we accept the speculation that the human version of FOXP2 might be important for the development of the capacity for speech, then an interesting question is when these mutations occurred and whether they might have been responsible for a revolution in culture 50,000 years ago.

#### 7.4 Did the mutations of FOXP2 become fixed zero years ago?

If humans and chimpanzees are distinguished by the two mutations in FOXP2, did these mutations occur zero years ago as Enard et al. suggested? Or was it 6 million years ago at the time of the split between the human and chimpanzee lines? Or some time in between, such as the 1.8 or 1.9 million years ago that we suggest below?

Enard and Svante Pääbo were among the authors of the paper (Krause et al. 2007a) which announced that Neanderthals share the

human mutations in FOXP2, pushing the dates of these mutations back to some time before the common ancestor of modern humans and Neanderthals who lived some 660,000 years ago (Green et al. 2008). Nobody supports a recent date for the human mutations in FOXP2 any more. It is important, however, for us to reiterate the arguments we presented at the Cradle of Language Conference to understand how Enard et al. got such a wrong answer.

The date of zero years ago as the most likely time when everyone in the world was finally able to speak a fully modern language cannot be literally true: In the historical period, there are no credible reports of any widespread inability to speak a human language. But this date is associated with a 95% confidence interval of zero and 120,000 years (on a chi square distribution). At first glance one might think that 50,000 years ago would be well within this confidence interval, and that this date is compatible with Klein's theory of a language and culture revolution. The evidence for this is very weak.

If one could simply arrange the dates of zero and 120,000 years ago on a chi square distribution with 120 ky at the point of the 95% confidence interval, one would see that 50,000 years ago was at the 79% confidence interval—meaning that there would be a 79% chance that the mutations were between zero years ago and 50,000 years ago, not in time to cause Klein's revolution. The 99% confidence interval would coincide with 200,000 years ago, meaning that there was only a 1% chance of these mutations occurring by that date. The statistics, however, are not that simple as we are dealing with the likelihood of simulated data.

There is no strong statistical evidence in the Enard et al. paper for any date associated with the unique human mutations in FOXP2. Using a summary likelihood method to find the most likely date of zero years ago, their first confidence interval was zero to 4,000 generations (80,000 years) ago. Concerned that this approximation was not accurate in that context, they ran 100 additional simulations to examine the distribution of the estimated time ( $T\text{-hat}$ ) when the true time  $T$  is equal to their maximum likelihood estimate of  $T = 0$ . In this case the confidence interval was zero to 120,000 years. Then noting that their model did not include population expansion, they suggest that if they took population growth into account their estimate would be pushed back maybe 10,000 to 100,000 years. Then they say, "In any case, our method suggests that the fixation occurred during the last 200,000 years of human history, that is, concomitant with



or subsequent to the emergence of anatomically modern humans” (Enard et al. 2002: 871). This does not generate great confidence in the estimate of zero years ago with a 95% confidence interval stretching back 120,000 years.

What does it mean, then, to say that the most likely date for the human mutations in FOXP2 is zero years ago? It means that there is almost no likelihood that two amino acid changing mutations would have appeared at all in the human line—not surprising when there have been none in the chimpanzee or gorilla lines in the last 90 million years, and only one in mouse. Saying zero years ago is pretty close to saying that it hasn’t happened yet.

Statistical models in general are problematic when dealing with small numbers like one or two. Can we really talk about “accelerated evolution” when there is an increase of only one or two changes? Since there are no amino-acid-changing mutations in the chimpanzee line in the last 90 million years, we can say that there are infinitely many more such mutations in the human line even though there are only two. Likewise there are infinitely many more amino-acid-changing mutations in mouse than in chimpanzee, though there is only one in mouse. With two mutations in the human line and one in mouse, we have a 100% increase in the number mutations in the human line compared with mouse, even though there is only one more. Zhang et al.’s statistical model allowed them to figure that there was a 6340% increase in the rate of mutation in the recent human line compared to a line from our common ancestor with chimpanzee to mouse (Zhang et al. 2002). Enard et al. also combine ~85 million years of evolution along the human line (going back from the common ancestor of chimpanzee and human) and ~90 million years on the mouse line for comparison with ~5 million years of recent human evolution. It makes a huge difference if we compare two changes in 5 million years with one change in 175 million years, as opposed to two changes in 90 million years (human line) with one change in 90 million years (mouse); it’s a thirty-five-fold increase versus a two-fold increase.

In addition to the serious problem of small numbers in a statistical calculation, and the choice of a domain for comparison, one has to make many assumptions about matters that are not really known in building a likelihood model like the one of Enard et al. For example, Enard et al. state that “several additional parameters are in this selective sweep model: the distance to the selected site, the effective population size of humans,

the strength of selection, the mutation rate, and the recombination rate. It is not computationally feasible to co-estimate all these parameters, and we proceeded by assuming that the values of most nuisance parameters are known exactly.” To designate these important parameters of their model as “nuisances” suggests to us extremely low confidence in reality regarding their simulations.

Another frequent assumption in population genetics, and one used in this model, is the assumption of random mating. Random mating sometimes works in a model, for example if it is impossible to determine a particular genotype based on a phenotype. It is clear, however, that random mating does not apply to humans even in small communities. Assortative mating would tend to retard the spread of favorable alleles for language acquisition across the globe, since women do not generally choose a different mate for each pregnancy. In fact the element of choice implies assortative mating. On a worldwide level there has been a certain amount of ethnic mixing, but no chance for random mating, especially if we can say that the Andaman Islanders were genetically isolated for 65,000 years. The model to calculate the date of the FOXP2 mutations may use sophisticated algorithms, but built into these calculations are assumptions that are highly questionable. The reliability of what goes into the model affects the reliability of what comes out. If one does not assume random mating, the estimated time for the origin of mutations fostering language acquisition and their spread throughout the species must be even older than anticipated. The likelihood model produced by Enard et al. got the wrong answer because it has fundamental flaws and was clearly inappropriate for dating the human mutations in FOXP2.

## 7.5 A digression on mutations, SNPs, and selective sweeps

We have been talking of mutations as if we were talking of SNPs, single nucleotide polymorphisms or changes in one particular letter of the genetic code. Strictly speaking, SNPs are only one kind of mutation. There can be insertions, deletions, duplications, and rearrangements in the genetic code as well. When one of the four nucleotides, or letters, of the genetic code is mutated into another one, as substitutions of one nucleotide for another, we have variation in the human species, because this mutation happens in only one person and is carried on by the

descendants of that person. This variation is a single nucleotide polymorphism (SNP, pronounced snip)—a place where a single nucleotide, or letter of the code, differs in different groups of people.

When we talk of disease genes, we are really talking about disease-causing or disease-linked variants of a gene. A particular variant in a single SNP can cause a disease, such as the mutation of a single nucleotide of FOXP2 in the KE family or the mutation that causes hemophilia in some of Queen Victoria's descendants. The human genome, as we call it, is a generalized version of the genetic code shared by all people, but all individuals have their own individual variations on the genome. No two individuals are genetically identical except for identical twins.

Although there is strong evidence of selection at FOXP2 (Clark et al. 2003), we can find no evidence to corroborate a date for a recent selective sweep in the last 100,000 or 200,000 years. A robust method for finding recent selective sweeps in the human genome—selective sweeps that occurred in the last ~200,000 years—failed to find a recent selective sweep in or near FOXP2 (Williamson et al. 2007). So what is a selective sweep?

A recent selective sweep is defined by what Maynard Smith and Haigh called “genetic hitchhiking” (Maynard Smith and Haigh 1974). When a favorable mutation is passed on to the next generation, the adjacent area of the chromosome hitchhikes along with it, so everyone who has the new mutation also has an identical area of chromosome surrounding it. Any individual variations in this area are caused by new mutations that arose after the mutation causing the selective sweep. A selective sweep is characterized, then, by an area of chromosome with a small amount of variation. For rapid genotyping, this equates to an area with very few SNPs. When a favorable mutation is passed on from parent to child, the whole chromosome actually goes along with it. But then when the children mature and form eggs or sperm their maternal and paternal chromosomes cross over and recombine to form a new variation of the chromosome to be passed on. In this process of crossing over and recombining, the hitchhiking area of the original chromosome gets shorter and shorter as the generations go by, so the length of the area of the selective sweep depends on the strength of selection, the length of time since the mutation occurred, and the rate of crossing over in this part of the chromosome.

## 7.6 The selective sweep at FOXP2

At FOXP2 there is too much variation in the adjacent area of chromosome 7 to be consistent with a recent selective sweep in the last 200,000 years (Williamson et al. 2007). Enard et al. note this lack of the sign of a classic selective sweep, and suggest that this may be caused by an elevated rate of crossing over and recombination at FOXP2. They calculate that the crossover rate might be five times the average at FOXP2. But the 2006 human genome assembly shows that the crossover rate at FOXP2 is actually half the average rate.<sup>1</sup>

Dramatic acceleration of genomic changes due to selection does not necessarily correlate with recent selection. For example, Pollard et al. (2006) found that the genomic region with the most dramatic acceleration of selection, Har 1, had characteristics that suggested that the changes in the human line took place more than a million years ago. In their study FOXP2 was not among the 49 genomic areas with the most significant acceleration of selection.

In an article accompanying the publication of the draft of the human genome (Sachidanandam et al. 2001), the International SNP Map Working Group prepared maps of sequence variation for each human chromosome, mapping the number of SNPs per 10,000 bases in each consecutive bin of 200,000 base pairs. It has been argued that the bins with the lowest SNP density are likely places to look for selective sweeps and for genes which differ between modern humans and chimpanzees (Diller et al. 2002). FOXP2 is not in one of those areas.

The SNP density map for chromosome 7 does show a very interesting pattern for the area around FOXP2 (see Figure 7.1). There is a long stretch of about 4 million bases in which there is a relatively even amount of variation compared with similar stretches in the rest of the chromosome. There are no bins of extra low SNP density and no bins of high SNP density in this section of chromosome 7. The section as a whole has a SNP density somewhat below average, approximately 70% of average. This may be the sign of an unusually strong ancient selective sweep.

<sup>1</sup> Enard et al. also calculate Fay and Wu's H statistic (2000) to predict strong selection at FOXP2. But the H statistic measures an excess of high frequency SNPs hitchhiking with the mutation under selection and in the presence of recombination. It does not apply when the selected mutation has gone to fixation – a case in which there are no longer any high frequency SNPs in the area of the selective sweep.

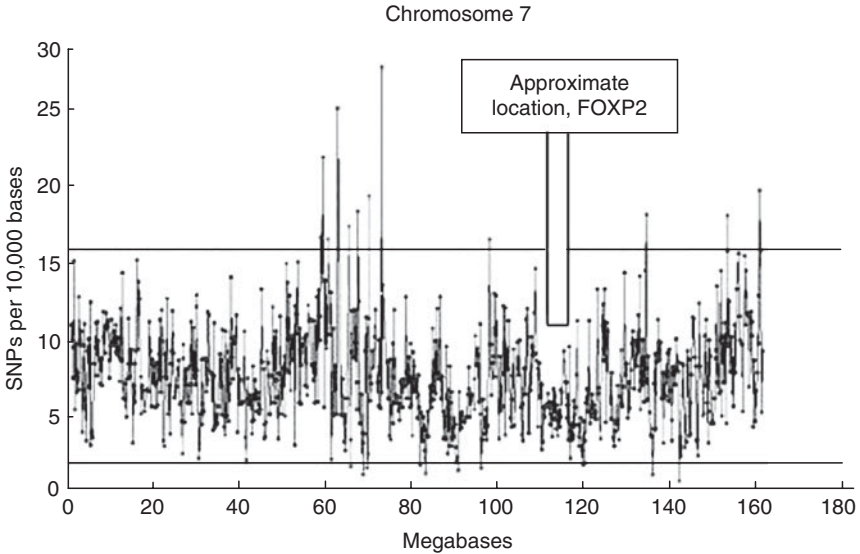


FIG. 7.1 SNP frequency map for chromosome 7 adapted from The International SNP Consortium (Sachidanandam et al. 2001). Each dot represents a bin of 200,000 bases, 95% of which lie between the two lines. The dots below the lower line are bins with the fewest SNPs, bins where we might expect to find recent selective sweeps. The remarkably cohesive area around FOXP2 does not show extremes of variation, either high or low, and may be the signature of an ancient selective sweep.

Over time, new mutations and the continual turnover of neutral variation obliterate the sign of a selective sweep. The random and neutral generation of mutations will bring an area devoid of SNPs up to 50% of normal variation in about 1 million years, assuming that the average time for a new neutral mutation to become fixed in the genome, if it is going to be fixed, is  $4N$  generations (Hartl and Clark 2007) and taking the rule of thumb that the effective population size,  $N$ , is 10,000, and the generation time to be 25 years. In 2 million years, this process would bring this area up to 75% of normal variation. If our ancient selective sweep around FOXP2 as seen in Figure 7.1 is 70% of normal variation on average, then  $\frac{70}{75}$  of 2 million years brings the date of this selective sweep to about 1.8 or 1.9 million years ago, the time of the Plio-Pleistocene boundary, corresponding to the approximate time when *Homo habilis*, *H. ergaster*, and *H. erectus* emerged in the fossil record. Along with the appearance of the oldest Acheulian tools and the oldest *H. erectus* fossils outside Africa,

it also corresponds to a point in human evolution when the brain size of early *Homo* began to expand (eventually to triple) from the 450cc of chimpanzee and australopithecine brains.

The beauty of this evidence is its simplicity and transparency. The pattern is so strong that it can be seen by the naked eye. The calculations are so simple that they can be done by hand. There are no error-prone algorithms or arcane calculations hidden inside a computer. The parameters in the calculation are transparent and can easily be adjusted. For example, one might argue that 20 years would be more appropriate for the generation time in archaic humans, even if 25 years, the midpoint in the prime childbearing years of 15 to 35, may be an appropriate generation time for modern people. Changing the generation time to 20 years would bring the date closer to 1.5 or 1.6 million years ago, still a very interesting time for the evolution of the brain in archaic *Homo*.

That is the other beauty of this result. If the change in FOXP2 really was involved in an important way in the evolution of the neuromuscular control of the speech organs, then it is more likely that this change occurred toward the beginning of this evolutionary process rather than at the end. The date of 1.8 or 1.9 million years ago puts the mutations in FOXP2 near the beginnings of the adaptive radiation within genus *Homo*, at the time of explosive evolutionary experimentation, and provides a satisfying correlation between the genetic evidence and the archeological record.

## 7.7 Direct DNA evidence

If we could obtain and sequence DNA from ancient fossils we would have direct evidence of when the human mutations occurred. Evidence from Neanderthal DNA published in November 2007 (Krause et al. 2007a) supports our arguments, above, which we presented a year earlier.

Unfortunately, under normal environmental conditions DNA degrades rather easily, and one would have to be extremely lucky to find good DNA from fossils as old as *Homo erectus* from the period between 1 and 2 million years ago. Neanderthals, however, were still alive in Europe 28,000 years ago, and in 2006 two teams succeeded in sequencing small amounts of Neanderthal nuclear DNA. A mostly American team sequenced 65,250 bases (Noonan et al. 2006), and a mostly European team claimed to have

sequenced a million bases (Green et al. 2006). The sequences were mostly from non-coding DNA and did not include any interesting genes. The two studies were inconsistent, however. The European study seemed to show that Neanderthal and modern humans were much more similar in their DNA. One of the most serious problems in sequencing ancient DNA is to avoid contamination by modern DNA, and it turned out that on close analysis, about 78% of the DNA from the European group was contamination from modern humans (Wall and Kim 2007). Teams studying Neanderthal DNA instituted stricter procedures and tests and began to focus on specific genes, and in October/November 2007 sequences were published for FOXP2 (which showed that Neanderthals had the modern human mutations at FOXP2) (Krause et al. 2007a) and for a melanocortin receptor gene MC1R (which showed a mutation unique to Neanderthal but which would have caused red hair and light skin) (Lalueza-Fox et al. 2007). We can have confidence in the red hair result because the Neanderthal DNA differed from modern human. For the FOXP2 result, the new safeguards and the sequencing of the DNA in two separate labs give us confidence that the result is probably valid, but since the Neanderthal and modern human DNA were identical we cannot entirely rule out contamination. Finding one gene in a degraded genome of more than 3 billion nucleotides is like finding a needle in a haystack. Previous validated Neanderthal sequences were largely repetitive sequences. This consideration also calls for skepticism until the results are corroborated by more evidence. If valid, however, this direct evidence from Neanderthal DNA supports our arguments.

### 7.8 Conclusion: was there a gene for speech and language?

If a phenomenon such as a cultural revolution occurred 50,000 years ago based on a genetic mutation for language, then the evidence must be consistent across all the relevant fields—especially archeology, anthropology, neurolinguistics, and genetics. The archeological and anthropological evidence accumulating in the last decade and a half does not support the claim that there was a revolutionary behavioral change in humans 50,000 years ago. It was “The revolution that wasn’t,” as McBrearty and Brooks (2000) demonstrate.

The evidence from genetics and neurolinguistics is more complex. There is the intriguing but still speculative possibility that the two human mutations in FOXP2 may have been important for the evolution of the capacity for speech. The study by Enard et al. (2002) gave only very weak support to the date of 50,000 years ago for the mutations in FOXP2. The most likely date by their model was zero years ago. Beyond that, the model used to derive that date is flawed and inappropriate. If it is true that while there was selection at FOXP2 (Clark et al. 2003), there was no recent selective sweep at or near FOXP2 in the last 200,000 years (Diller et al. 2002; Williamson et al. 2007), then Enard et al. clearly got the wrong answer. The evidence from that genetic model does not support the hypothesis of a mutation for language occurring 50,000 years ago. The direct evidence from Neanderthal DNA is also inconsistent with the Enard et al. model, and suggests that the human mutations in FOXP2 occurred some time before the split between Neanderthals and modern humans. That is, these mutations occurred some time before ~660,000 years ago.

From a neurolinguistic point of view, if FOXP2 really is fundamentally important for the evolution of language by helping to establish neuromuscular control of the organs of speech, then we would expect the human mutations in FOXP2 to have been at or near the beginning of the process of dramatic brain growth, so that spoken language and the biological capacity for language could evolve together, reinforcing each other. We would expect that the million and a half years of brain growth in the genus *Homo* would have coincided with this co-evolution of language and the biological structures required for language. Language is immensely complex, as are the neural and anatomical structures which serve language. We cannot expect that there was only one mutation which served language. The capacity for modern language needed a long time to evolve before anatomically modern humans emerged some 200,000 years ago.

The date we present of 1.8 or 1.9 million years ago for the selective sweep at FOXP2 supports this neurolinguistic scenario of the co-evolution of speech and language with the neural and anatomical substrates of language. This date is also consistent with the archeological and anthropological evidence, and now, with the direct genetic evidence.



# 8 A “language-free” explanation for differences between the European Middle and Upper Paleolithic Record

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WIL ROEBROEKS AND ALEXANDER VERPOORTE

## 8.1 Introduction

On current evidence, the archeological record starts at around 2.6 million years ago, when hominins were making stone artifacts in eastern Africa, probably in the context of the exploitation of large ungulate carcasses (Domínguez-Rodrigo et al. 2005). These first stone tools were produced approximately 4 million years after the split of the human lineage from the chimpanzee line (Stauffer et al. 2001; Cela-Conde and Ayala 2003). Together with the hominin fossil record, the archeological record constitutes a unique archive of long-term changes in hominin behavior in various domains, such as dietary strategies, range expansion and contraction, and material culture.

Some workers have tried to exploit this record to make inferences on the evolution of one of the defining characteristics of modern humans, human language, usually in order to construct a chronological framework for its emergence and development based on specific archeological “proxies” for language (e.g. Mithen 1996; Noble and Davidson 1996; d’Errico et al. 2003), but in some cases to test the strengths and weaknesses of competing hypotheses on the emergence of language (e.g. Buckley and Steele 2002).

Language, however, is a catch-all term for a combination of characteristics that mediate human communication in a way that is different from other primates. Given the sharp distinctions between human communication and that of other animals, human communication has for a long time been studied as an isolated phenomenon unique to our species.

Currently it is increasingly seen as the result of “a complex reconfiguration of ancestral systems that have been adapted in evolutionary novel ways” (Fisher and Marcus 2006: 9). Capacities for conceptual knowledge and memory, semantic selection processes, motor control, analysis of raw acoustic signals, and so on have been integrated in a rich computational system that we simply call language. Rarely do we, archeologists, make explicit which facets of language we are addressing when discussing its origins, and what elements of the archeological record we might consider to be indicative of the former presence of a specific “building block” of that complex system (Hauser et al. 2002). Language is an abstraction, and relating abstractions to the dirt of the archeological record is an interpretive challenge, to say the least.

This has not deterred archeologists from pointing out archeological phenomena as indicative of the presence of linguistic capacities in the deep past. Amongst these are the systematic use of coloring agents, the presence of personal ornaments and notation/arbitrary symbols, the production of representational art, and the presence of humans in challenging environments such as the high north or the arid parts of Australia. The colonization of Australia has repeatedly been interpreted as testifying to the presence of fully modern language, as the peopling of the landmass of Sahul could only have occurred with some form of seafaring technology, and hence the construction of compound tools such as rafts or boats. Noble and Davidson, for example, “believe that the breakthrough which enabled humans to make the sea-crossing involved the abilities to plan ahead that are made possible by language” (1996: 184).

The South African Middle Stone Age record from sites such as Blombos Cave, Klasies River, and Diepkloof has been at the center of recent discussions on language origins. This record contains a range of small, carefully shaped geometric stone tools, probably once part of multi-component hafted tools, extensively worked bone tools, large quantities of red ochre, “decorated” items, and perforated sea shells interpreted as beads. According to Mellars, the interpretation of these items “in terms of complex symbolic communication systems now seems beyond question” (2005: 17), whereas Henshilwood, excavator of Blombos Cave, argues that fully syntactical language was an essential requisite for the sharing and transmitting of the meaning of Middle Stone Age beadwork and engravings (Henshilwood et al. 2004).

As language in any sense of the word does not leave any direct traces in the fossil record prior to the invention of writing—not in the way that for instance hunting behavior or tool use does—one could propose that *rather than the archeological record indicating the presence of language, we archeologists have been using the concept of language to explain (changes in) the archeological record.* Such language-based interpretations of changes in the record are indeed almost uniquely used to explain the emergence of more “complex” forms of behavior, and have repeatedly been launched for interpreting the differences between the record of the Middle and the Upper Paleolithic of Europe. Such interpretations are more often than not somehow framed in “cognitive” terms, i.e. in complex ways stating that Neanderthals were basically more cognitively challenged (i.e. stupid) than modern humans, as will be detailed below. The absence of “fully modern” linguistic skills in Neanderthals is seen as one of the main explanations for the differences in the records of the two species.

We will show in this chapter that in the case of the Neanderthal record, alternative, more mundane interpretations are possible, for which we do not need to use the concept “language” at all. In order to do so we will first give a review of the Neanderthal record, then briefly provide some current explanations for its differences to the archeology of modern humans and finally come up with an alternative explanation, based on one of the most fundamental characteristics of any species: its energetic needs.

## 8.2 The Neanderthal record: a very short review

Neanderthals are by far the best-studied extinct hominins, with a rich fossil record sampling dozens of individuals, all but the ones from Engis and Gibraltar discovered in the one and a half centuries since the famous Feldhofer Grotte find, in August 1856. There is considerable agreement that the Feldhofer Grotte individual and its contemporaries formed the end product of a long evolutionary lineage, the first representatives of which colonized Europe somewhere in the first half of the Middle Pleistocene. Genetic studies suggest that the date of separation between the recent human clade and Neanderthals is about 500,000 to 800,000 years ago, with new studies opting for the older side of this estimate (Pennisi 2007). The rich Sima de los Huesos assemblage from Atapuerca has been interpreted as being near the beginning of the Neanderthal evolutionary

lineage (Arsuaga et al. 1997), with new dates suggesting that the 28 individuals discovered thus far there died minimally half a million years ago (Bischoff et al. 2007). If these dates are correct, we are dealing with approximately half a million years of Neanderthal existence, with most of the fossils from this species having been unearthed in the western parts of their former range. In fact, the eastern, southern, and northern limits to their former distribution are poorly documented because of imbalances in research intensity. Recent genetic studies suggest a Neanderthal DNA-profile for some ambiguous southern Siberian fossils expanding the current estimate for their distribution with some 2,000 km further to the east (Krause et al. 2007b).

Within the known “fossil” range of western Eurasia, the Neanderthal range varied, resulting in an ebb and flow of Neanderthal presence. The exact nature of this ebb and flow has been the subject of some debate, which has led to a range of studies of the habitats occupied by Neanderthals and their environmental limits (Gamble 1986; Roebroeks et al. 1992; Roebroeks and Gamble 1999; van Andel and Davies 2003; Stewart 2005). Such studies have shown that most Neanderthal sites are associated with faunal remains indicative of so-called mammoth steppe environments (Guthrie 1990). Compared to present-day tundras or polar-deserts, this Pleistocene mammoth steppe was a highly productive habitat that supported a rich and diverse grazing community, with the mammoth as its characteristic species. Ice-core studies indicate that climate instability dominated the Neanderthal time range. It is also for this reason that within the monolithic concept of the mammoth steppe one can uncover a great deal of chronological and spatial variation. Individual species ranges would have expanded and contracted constantly, to a large degree in the rhythm of climate changes, leading to strange community associations of floras and faunas and occasionally to the extinction of species (this may have included the repeated local extinction of Neanderthals in the northern parts of their range).

Neanderthals and earlier hominins also were able to survive in full interglacial forested environments, where a large part of the biomass was locked up in forms not readily available for hungry hominins: forest vegetation. On the mammoth steppe, grass supported the grazers and the grazers were hunted to support the hominins. In interglacial environments large mammals were considerably thinner on the ground, but in spite of this Middle and Late Pleistocene hominins were able to survive

during full interglacial periods, from their very first presence in Europe onward (Parfitt et al. 2005; Preece et al. 2007; Roebroeks 2007). Neanderthals were present in a wide range of environments, but they probably did not colonize high-latitude environments, as thus far no Neanderthal fossils or unambiguous Lower or Middle Paleolithic finds have been reported from above 55°N.

Zooarcheological studies have established that Middle Paleolithic Neanderthals were capable hunters of medium-sized and large mammals, a view now widely shared, including by former proponents of the hypothesis that scavenging was a very important part of Neanderthal subsistence practices (e.g. Stiner 2002). There is only very limited evidence from the earliest sites in Europe for Lower Paleolithic hunting activities, but taphonomic “miracles” such as Schöningen in Germany (Thieme 1997; Voorمولen 2008) show that we work with a very biased record and that early Neanderthals must have developed basic adaptations for large mammal hunting around the middle part of the Middle Pleistocene, if not earlier (cf. Villa and Lenoir, in press). Evidence from Middle Paleolithic Neanderthal sites indicates prime-adult harvesting of bovids and cervids by the later Middle Pleistocene, with a strong focus on high-quality mammals and parts thereof (Gaudzinski 1995; Gaudzinski and Roebroeks 2000; Stiner and Kuhn 2006). In the southern part of their range, Neanderthal hunting activities may occasionally have led to a decline of red deer and aurochs populations, as recently suggested by Speth (2004) and Speth and Clark (2006). Middle Paleolithic exploitation of small prey has also been documented, especially in the circum-Mediterranean, where this is largely confined to easily gatherable, sessile or slow-moving animals: marine molluscs, tortoises, legless lizards, and ostrich eggs (cf. Stiner 2002; but see also Villa and Lenoir, in press).

Whereas archeozoological studies show that Neanderthals were hunting and what their prey animals were, isotope studies of European Neanderthal fossils suggest that they were top-level carnivores, with the bulk of their dietary protein coming from animal sources (Bocherens et al. 1999; Richards et al. 2000). Though limited in numbers thus far, these studies and the zooarcheological evidence suggest that Neanderthals were at the top of the food chain, and hence probably existed at low densities, like other large carnivores (Stiner and Kuhn 2006).

Neanderthals carved their predatory niche with a small range of simple hunting weapons, including wooden thrusting and/or throwing spears, as

illustrated by the Schöningen evidence (Thieme 1997; Rieder 2000). It is unclear whether they tipped their spears with stone points, though there is some evidence suggestive that they may have done so (see Shea 2006; Villa and Lenoir 2006 for a review). However, the archeological record of tool use is heavily biased towards cutting tools rather than projectile weapons. These cutting tools consist of simple flakes and blades produced by a wide variety of techniques, such as Levallois, discoid, Quina, laminar, and bifacial technology. The tools display little patterning in Lower and Middle Paleolithic space and time, probably testifying to the versatility of the Neanderthal toolkit.

Hafting of stone tools was probably a common practice in Middle Paleolithic times. The small number of finds testifying to Middle Paleolithic hafting practices is certainly the result of taphonomy, again, but they cover the whole Middle Paleolithic time range. Tar pitch is known from two flakes from about 250,000-year-old Campitello quarry deposits in Italy (Mazza et al. 2006), from two flakes (more than 80,000 years old) from Koenigsau in Germany (Koller et al. 2001) and from the Umm el Tlel site in Syria, at approximately 40 ky (Boëda et al. 1996). Koller et al. (2001) and Boëda et al. (1996) suggest that very high temperatures as well as good temperature control must have been involved in the production of the bitumen. Indeed, given the abundant presence of burnt flints and bones at many Middle Paleolithic sites, it is safe to assume that Middle Paleolithic Neanderthals controlled the use of fire. Their hearths were usually very simple ones, as from the whole Lower and Middle Paleolithic of Europe only three (!) cases of very simple stone-lined or stone-delimited fireplaces have been reported, all from the late Middle Paleolithic: Les Canalettes (Meignen 1993) and La Combette (Texier et al. 1998) in southern France and Vilas Ruivas in Portugal (Raposo 1995). At most sites where fire seems to have been present we are at best dealing with shallow depressions filled with ashes and charcoal fragments. Usually some burnt materials are recovered among the debris of stone tool production and bone fragments resulting from butchery.

Likewise, indications for investments in site architecture, such as the construction of dwellings or windbreaks at locales, are almost completely lacking, which shows that, if once present, such constructions must have been very ephemeral (Kolen 1999). Neanderthals invested very little in the spatial layout of their camp sites, and this is also reflected in the transport of raw materials. We know that occasionally stone artifacts were transported

over large distances in the Middle Paleolithic (in both west and central Europe up to a few hundred kilometers, cf. Slimak and Giraud 2007, but these observations relate to very exceptional finds). The almost exclusive use of locally (< 5 km) available rocks is the rule in the Lower and Middle Paleolithic (Geneste 1985; Roebroeks et al. 1988; Féblot-Augustins 1999).

Finally, there is the well-known problem of the production of notational and representational art. The Lower and Middle Paleolithic record has the occasional piece with some regular incisions (e.g. from the Middle Pleistocene site at Bilzingsleben, Germany, see Mania and Mania 1988) and other modifications (e.g. the Tata plaque, Vértes 1964) and sporadic use of ochre is known from the early Middle Paleolithic of Europe onward. Colorants such as ochre and manganese become a more common phenomenon in the late Middle Paleolithic, however (Soressi et al. 2002). Claims for Lower and Middle Paleolithic *figurative* art exist, but these are all contested. For instance, the Middle Paleolithic Berekhat Ram figurine from Israel (Goren-Inbar 1986; d’Errico and Nowell 2000) appears to be a humanly modified natural object rather than a deliberately carved figurine. Only with the later Neanderthals in western Europe, including the creators of the Chatelperronian (about 40,000–35,000 radiocarbon years ago), the record includes personal ornaments, regular use of red ochre and other colorants, and occasional “notational” pieces (d’Errico et al. 1998), but representational art seems to be absent.

### 8.3 The modern human yardstick

The Neanderthal data briefly reviewed here are usually interpreted in comparison to the record of Upper Paleolithic modern humans, which is (often, but not always) strikingly different in some of the domains discussed above. For example, as far as range expansion is concerned, in the Upper Paleolithic humans colonized high-latitude environments, and were already present around and above the arctic circle in eastern Russia around 35,000 radiocarbon years ago (Pavlov et al. 2001) as well as in Siberia at 71°N 27,000 radiocarbon years ago (Pitulko et al. 2004). In contrast, Neanderthals and other archaic hominins always stayed south of 55°N. After the last glacial maximum (at 18,000 years ago) Upper Paleolithic humans entered the Americas via the Bering Strait, a range extension from their northern Siberian “stronghold.”

Though Neanderthals were very successful hunters, the range of prey species was somewhat narrower than the one exploited by Upper Paleolithic humans. As far as (the thus far limited number of) stable isotope studies of skeletal remains go, most Upper Paleolithic humans have isotope signals that are comparable to the Neanderthal “top carnivore” ones, but some modern human fossils display different values. From the early Upper Paleolithic onward, some were consuming large amounts of aquatic resources, not known from the small sample of Neanderthals studied thus far. Good examples are the Kostienki 1 individual from the Don Valley, southern Russia, dated to 32,600 radiocarbon years ago (Richards 2007) and the individual known as “Il Principe” from Arene Candide in Italy, dated to 23,440 radiocarbon years ago (Pettitt et al. 2003). Archeozoological studies also show that many Upper Paleolithic humans exploited a significantly wider range of species than Neanderthals, including fast-moving small-sized game and birds (Stiner et al. 1999).

As far as use of space is concerned, Upper Paleolithic groups invested significantly more in site structure and furniture. Even though many Upper Paleolithic sites in Europe yield carbon copies of Middle Paleolithic find distributions, including largely “invisible hearths” (Sergant et al. 2006), stone-lined and dug-out fireplaces are more numerous and are known from many Upper Paleolithic sites. Unambiguous remains of dwellings have been documented all over Europe from the mid Upper Paleolithic (30,000–20,000 radiocarbon years ago) onwards. Examples include stone rings at Villerest, Étiolles, and Pincevent in France and Gönnersdorf in Germany, postholes and pit clusters at Dolní Věstonice and Pavlov in the Czech Republic and Grub-Kranawetberg in Austria, mammoth bone structures at Mezin and Mezirich in the Ukraine, housepits at Kostienki on the Russian plain, and stone floors from Magdalenian localities such as Cerisier in France and Ölknitz in Germany. These archeological features have no Middle Paleolithic parallels whatsoever.

The Upper Paleolithic record also testifies to the importance of projectile technology, with a wide range of lithic as well as bone, ivory, and antler points and a temporal change of the morphology of these points at a—compared to the Neanderthal record—fast rate. These typochronological changes allow archeologists to pinpoint assemblages to fine slices of time, less than ten thousand years, within the Upper Paleolithic. This investment in projectile points must have set constraints on the quality of the raw materials used to produce the long and straight blanks for these points.



These quality constraints may to some degree explain the larger investment in the transport of high-quality raw materials compared to the Middle Paleolithic, when a simple and versatile toolkit was predominantly produced on locally occurring materials (Roebroeks et al. 1988).

The most striking aspect of the European Upper Paleolithic record is undoubtedly the presence of various forms of art in the archeological record, from the Aurignacian onward. This includes representational art (both in its parietal and mobiliary expression) and personal ornaments, some of which were transported over hundreds of kilometers. Both the relative homogeneity of art style and forms over large areas as well as such large-distance transports are interpreted as the material correlates of large-scale alliance networks. These would have served as social safety nets during long-term occupation of highly seasonal and unpredictable Pleistocene environments (Gamble 1986; Whallon 1989). It needs to be stressed though that there were many long periods within the Upper Paleolithic in which major parts of Europe did not see any art production at all.

#### 8.4 Explanations: what makes the difference?

The many explanations developed to account for the differences between the Middle and Upper Paleolithic record of Europe mostly converge on a replacement of the local Neanderthal population by the advent of anatomically modern humans, whose first physical representatives have been unearthed in the Pesteră cu Oase cave in Romania (c. 35,000 radiocarbon years ago), and whose first archeological signal is probably the Aurignacian techno-complex. In most versions, modern humans have developed some advantage in technology, subsistence strategy, social organization, adaptive flexibility, and/or innovative capacity. Ultimately these scenarios implicitly or explicitly assume that cognitive and linguistic developments favored such improvements.

Several aspects of cognition have been put forward as most critical. Fauconnier and Turner (2002) think that the Upper Paleolithic record demonstrates the development of a new ability to perform “conceptual integration,” facilitating the composition and elaboration of concepts to produce new and more elaborate conceptual structures enabling such technological innovations as projectile weaponry, dwellings, and artistic

creativity. Their way of reasoning is somewhat analogous to Mithen’s (1996) *Prehistory of the Mind* argument. Tomasello et al. (2005) argue that the emergence of modern humans would have involved new kinds of social motivations, social emotions, and social cognition, which would have enabled the development of full-fledged shared intentionality involving joint goals, joint intentions, and joint attention (Tomasello et al. 2005: 726). Together with observational learning and imitation, these were preconditions for the so-called *ratchet* effect (Tomasello 1999) that is visible in the relatively fast rate of changes in the Upper Paleolithic, referred to above. Innovations need something to build on, and the process of cumulative cultural evolution requires not only creative invention, but also faithful social transmission that can act as a ratchet to prevent slipping back. Only then can a tool one person makes be improved upon by the other person who learns to use that tool, and then that tool can be improved upon, and so on.

Similarly, several advantages of language have been put forward. Whallon (1989) emphasizes the importance of a “release from proximity,” the ability to communicate about subjects beyond the “here and now.” The integration of past experiences with future plans is a prerequisite to creating the large-scale social networks which provide safety in unpredictable environments. Fully modern language allowed for the displacement beyond the “here and now” and the planning depth manifest in Upper Paleolithic use of space. According to Klein (2000), fully modern language is open and productive, allowing for new messages and new ideas to be formulated. These qualities of language underlie the modern capacity for innovation which is demonstrated by the accelerated rate of change in the Upper Paleolithic.

In more general terms many workers have interpreted the differences between the Middle and Upper Paleolithic records as being the result of Neanderthals lacking “fully modern” language, and the archeological record as clearly showing the emergence of more complex language patterns by the time of the Upper Paleolithic of Europe and during the Middle Stone Age in southern Africa (Mellars 2005).

## 8.5 An alternative explanation?

An alternative interpretation for some of the differences between the Middle and Upper Paleolithic records of Europe may be found in one of

the most fundamental characteristics of any animal: its energetic requirements. Recent studies show that Neanderthals' energetic requirements were considerably higher than those of modern humans. These differences were the result of a range of factors, including Neanderthals' larger body mass and high activity levels (see for example Sorensen and Leonard 2001; Steegmann et al. 2002; Aiello and Wheeler 2003; Churchill 2006). Even the most conservative estimates for Neanderthal daily energetic requirements indicate differences in the range of more than 10% (see MacDonald et al. in press, for a discussion of such estimates). What are the effects of being "big" and energetically expensive?

The important implications of a higher energy budget for hominin spatial behavior can be simply illustrated by a central place foraging model, derived from Kelly (1995; see also Verpoorte 2006). Let us assume a homogenous environment, in which an individual forager travels to a specific resource location, exploits the resource patch and returns back home. The central place foraging model describes the net return rate of foraging activities as a function of the travel costs for a given mean environmental return rate, and net return rates go down with increasing travel costs. The *effective foraging radius* is the distance at which the forager brings back at least the daily energetic requirements that the forager has to meet. Beyond this distance, the forager comes home with less than required, and it is better for the individual to move camp to another location, depending on moving costs such as break-up of the campsite, moving distance and the likely conditions at another location.

The effects of Neanderthal energetics involve two aspects of the central place foraging model: higher daily energetic requirements, and higher travel costs (due to body mass and lower limb length, Weaver and Steudel-Numbers 2005). The need to provide higher amounts of energy means that the effective foraging radius becomes smaller. The increased travel costs lead to a steeper decline of net return rates with foraging distance, and hence to a shorter effective foraging radius. The effect of a smaller foraging radius is in both cases that our theoretical campsite will probably be moved more frequently and over shorter distances.

The model results have important implications for the use of space. Moving more frequently entails that the anticipated use-life of a campsite is shorter. With shorter anticipated use-life we should expect less investment in site features such as dwellings and other structures. Given the short periods of time Neanderthals were present at central places, the lack of

investment in “site furniture” we so clearly see in the archeological record becomes understandable. The absence of dwellings and other structures in the Middle Paleolithic record does not so much reflect a lack of organizational skills, planning depth, and “fully modern language” supporting all this, but it can be explained as an optimal solution for mobility under the high energetic constraints that Neanderthals had to cope with.

Mobility costs make up an important part of the human energy budget and hence selection to reduce these costs may have been a likely force in human evolution (Alexander 2002). Frequent residential moves over short distances resulted in Neanderthals covering fewer kilometers on an annual basis (Verpoorte 2006). They seem to have adopted a strategy to reduce locomotion costs involving a reduction of the amount of mobility (reducing the number of kilometers) rather than the efficiency of mobility (reducing the calories per kilometer).

This energetic perspective can be relevant for other domains of the Middle Paleolithic record too. The differences in range limits between Neanderthals and modern humans mentioned above have also been interpreted as resulting from cognitive deficiencies of Neanderthals, who were not able to survive in the challenging environments of the higher latitudes, for instance because they were not able to create and maintain the large-scale social networks upon which many hunter-gatherers depend in times of scarcity (e.g. Whallon 1989; Gamble 1986). However, Neanderthal energetic requirements set constraints on their range limits too. With increasing distance from the equator, resources tend to become spatially segregated along a gradient of decreasing temperature. As most mammals need larger territories to survive in higher latitudes, carnivore ranges tend to become larger with decreasing temperature. With the decrease in average temperature the distance covered by the average residential move of hunter-gatherers tends to increase. With the decrease of available biomass and the increased costs of locomotion, Neanderthal foraging activities in the north would have balanced on a thin wire. Under equal conditions, their high energetic requirements would have set narrower limits to their range than for modern humans, who would be able to move further north than Neanderthals. The “move less” strategy of Neanderthals is not an option in northern latitudes where larger areas must be covered to obtain sufficient energy.

The energetics perspective may also be of relevance for the interpretation of the relative “stability” in Middle Paleolithic material culture,

referred to above. The higher rate of change in the Upper Paleolithic has been interpreted as testimony to the innovative capacity of modern humans and dependent on the openness and productivity of “fully modern language” (Klein 2000: 591). However, innovation has both costs and benefits. Not all innovations are improvements after all. Neanderthal energetic requirements are believed to have influenced the balance of costs and benefits of different technologies as well as the costs and benefits of invention and innovation in the technological domain (cf. Ugan et al. 2003; Bettinger et al. 2006). In view of the archeologically visible hunting success of Neanderthals their relatively simple technology must have been under considerable pressure to improve. Yet the foraging technology remains relatively stable for hundreds of thousands of years. Why did Neanderthals not innovate their hunting equipment?

We suggest that the answer lies in the costs and benefits of subsistence technology. These costs can be divided into two types: search costs and handling costs (involving pursuit and processing). The technological innovations so visible in the Upper Paleolithic record (spearpoints, spear-thrower, bow-and-arrow, harpoons, snares, nets) are related to handling costs and in particular to the costs of pursuit. From the Neanderthal perspective sketched above, the best investment may have been in lowering search costs rather than in lowering handling costs. Where diet is relatively narrow, as suggested for Neanderthal diets (Stiner et al. 2000; Richards et al. 2001), search costs represent a large part of the foraging costs. Unfortunately, investments in a detailed knowledge of animal behavior and other clues to the whereabouts and predictability of prey are largely archeologically invisible investments. Again, a slow rate of change does not reflect lack of linguistic and cognitive skills, but other investment strategies with regard to innovation, probably constrained by energetic requirements.

We think that such a basic characteristic as energetic requirements was important for the behavior of Neanderthals. Their strategies regarding mobility, inhabiting northern environments, and innovative technologies were selected under energetic constraints that were different from those of modern humans. Given the high costs of Neanderthal bodies and its consequences, one wonders what could have been the benefits of being “big” and energetically expensive? For other mammals, answers have been framed in terms of insulation against the Pleistocene cold; larger muscle power for overpowering prey animals; intra- or inter-species competition;

lowering the costs to females of delivering large-brained babies, and the feasibility of on-body stockpiling of food reserves for lean times, e.g. the winter. Trying to evaluate the relative importance of these factors may give us valuable insight into the ecology of this lineage.

## 8.6 Discussion

As far as energetics and locomotion are concerned, Neanderthals went a different way than modern humans seem to have done. Building on the same *Bauplan* (that of their last common ancestor, about 500,000 to 800,000 years ago), two different lineages emerged in different types of environment. The differences between the archeological record created by the large bodied and brained hominins we call Neanderthals and Upper Paleolithic modern humans have routinely been interpreted in cognitive terms. Such cognitive explanations implicitly or explicitly focus on the absence of fully syntactic language for hominins who did not produce most of the components of the Upper Paleolithic package produced by our own species, the “eloquent ape” (Fisher and Marcus 2006).

Though we cannot prove or disprove the idea of “eloquent Neanderthals,” a focus on the ecology of Middle and Upper Paleolithic hunter-gatherers not only yields alternatives to such cognitive explanations, it can also help us to explain differences *within* the records of modern humans.

The archeological record contains sufficient data to infer that fully modern humans created very diverse archeological signatures, and that sometimes they strongly resemble what Neanderthals left behind in western Eurasian landscapes. For instance, in a paper comparing the Tasmanian Paleolithic record with the Middle Paleolithic of Eurasia, Simon Holdaway and Richard Cosgrove (1997) point to the strong similarities between the two, concluding that “the Tasmanian record has all the hallmarks of what in Eurasia would be identified as a record of archaic behavior.”

Likewise, in a comparison of the North American Paleoindian and early Archaic record to the Eurasian Middle Paleolithic, Speth states that most of these early North American sites

are just “patches” or “scatters” of artifacts and bones, with few if any formal hearths: isolated patches and lenses of ash are more the norm for thermal features. Moreover, most sites from these periods have little or nothing in the way of ornaments or grave

accompaniments; huts are generally absent or very controversial; and art of any non-perishable sort is virtually non-existent . . . in fact we are hard-put in most cases to find anything that even remotely smacks of symbolism. If we were to use the same criteria that we apply to Neandertals, we would have to conclude that the inhabitants of North America up until only a few thousand years ago were “cognitively challenged.” The parallels with the record of the Middle Paleolithic are even more striking if we exclude from consideration the few dry caves in western North America and waterlogged sites in Florida of late Palaeo/Indian and early Archaic age which have miraculously preserved traces of basketry, textiles, and other unusual and highly perishable items. (Speth 2004: 184)

In his view, the changes in the late Archaic, around 5,000 years ago, can be seen as a kind of North American counterpart to the Eurasian Upper Paleolithic “revolution.” These drastic changes in the record are seen by most workers as the results of “gradually increasing populations that were slowly filling in the landscape, reducing people’s abilities to ‘vote with their feet’ when things got tough, and thereby compelling them to begin playing with alternative economic, social and political strategies for maintaining the delicate balance between war and peace—in a word, social, technological, economic and political *intensification*” (Speth 2004: 185).

Brumm and Moore (2005) have made comparable points for the Australian archeological record, where the mid- to late-Holocene exhibits a pattern of changes like that in Upper Paleolithic Europe, including increased diet breadth and intensification of marine and plant food resource extraction, the emergence of very extensive trading networks and changes in artistic representation, religious systems, and burial practices. Demographic changes and greater population densities are seen as one of the possible factors behind this “symbolic revolution” (Brumm and Moore 2005: 167–168); the same is true for the case of the European Upper Paleolithic (e.g. Barton and Clark 1994).

These examples serve to make the point that modern humans did repeatedly and over long periods create archeological records that resemble the ones created by Middle Paleolithic hominins. In these cases, different factors may have been at stake, but as stressed by Speth no archeologist would of course “believe for a nanosecond that in the artless and style-devoid silence of the early Archaic we are dealing with a cognitively impaired proto-human” (2004: 185), and the same point has been made by Holdaway and Cosgrove (1997), Roebroeks and Corbey (2000), and Brumm and Moore (2005).

## 8.7 Conclusion

Both the Neanderthal archeological record and the energetic perspective developed here converge in the suggestion that Neanderthals constituted a species that may often have been very thin on the ground, with their presence stretched to the limits in many parts of their range. The Neanderthal way of dealing with energetic challenges will have set more severe constraints on their population densities than was the case for Upper Paleolithic modern humans.

The Neanderthal way of dealing with energetic challenges may have been a vulnerable route, which nevertheless lasted for a very long period, ending around the time when modern humans started to colonize parts of the former Neanderthal range. The consequences of the energetic differences between the two populations may have been of relevance in this process. Modern humans with a more diverse subsistence base may have had selective advantages over the Neanderthals with their dietary focus on terrestrial mammals, as more diverse diets are linked to lower infant mortality rates and longer life expectancies in humans (Hockett and Haws 2003, 2005). As modeled by Zubrow (1989) a small demographic advantage in the order of a 2% difference in mortality would have resulted in the rapid extinction of the Neanderthals, in approximately 30 generations' time.

In this chapter we have made two points that have implications for the wider field of archeological studies of the origins of language. We have pointed out that language is an abstraction that is very difficult to relate to phenomena in the archeological record, and methodical links between linguistic elements and archeological patterns have not been worked out yet. Instead, archeologists all too often have been using “language” as a concept to explain changes in the archeological record. In this chapter we have shown that, as far as the differences between the European Middle and Upper Paleolithic records are concerned, more “mundane” explanations can be developed. We have also shown that the differences between both records are much more subtle than is commonly acknowledged. The record of Upper Paleolithic modern humans is diverse, and includes records that resemble the ones left by Neanderthals. The diversity of the modern human record also requires explanation, and the presence or absence of “language” will not be very helpful when reviewing the North American Paleoindian, the European Upper Paleolithic, or the Australian



aboriginal record. We suggest here that Neanderthal energetics may to an important degree explain the Neanderthal record, including their disappearance, without us having to refer to cognitive and linguistic differences for its explanation. We do not doubt the communicative skills of Neanderthals (language *s.l.*) or the relevance of the evolution of human language *s.s.*, but in our view, the archeological record is silent on the linguistic capacities of archaic hominins (cf. Fitch et al. 2005; Bickerton 2007a). We hope to have shown here that, as far as the interpretation of the Neanderthal record is concerned, archeologists can tell a good story without “language.”

# 9 Diversity in languages, genes, and the language faculty

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JAMES R. HURFORD AND DAN DEDIU

## 9.1 Introduction

In the literature on language evolution, one frequently finds phrases such as “ancestor language,” “the first human,” and “the language faculty.” The first two of these suggest the existence in the past of single unified entities from which modern languages or humans are descended in their entirety. The third expression, “the language faculty,” suggests a synchronic unity with the implication that it too could have had a single unified source. At the level of expository metaphor, such expressions may have their uses. Here, as a cautionary exercise, we argue that such metaphors widely circulating in both technical and popular scientific discourse are overused and project a too simplified perception of extremely complex phenomena. Our point is quite general, and can be appreciated without recourse to technical detail, although this does not mean that the technical details don’t support our case. In this context, we submit that there are multiple sources (or “cradles”) of:

1. **individual languages**, such as English, Afrikaans and Xhosa—these are varied, there being, for example, as many different varieties of English as there are English speakers; and “genealogical” relations between languages are not consistently divergent;<sup>1</sup>
2. the **human genome**, which is not a single, uniform entity across our species, as shown by the HAPMAP project; in this domain, too,

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<sup>1</sup> A tree diagram is consistently divergent if it is never convergent, that is if there is only ever one path from the root of the tree to any given daughter node.

“genealogical” relationships as revealed by the genetic data are far from simple and tree-like;

3. the **human language capacity**, which is not a single monolithic capacity, but a dynamic, evolving one, resulting from the complex interaction of biology and culture.

Each of these is a mosaic with many sources, and all but the most recent of these was somewhere in Africa, or in the continent from which Africa was formed, but at different times.

A number of theoretical tools and hypothetical concepts circulating in scientific discourse contribute to oversimplified beliefs that the phenomena listed under 1–3 above are unitary and have single sources, which can be pinpointed to a single era in evolution and a single geographical region. Such potentially misleading concepts include: protoworld etymologies implying a single mother language; tree diagrams of language families and of human phylogeny with a single root and no reticulation; “Mitochondrial Eve” and “Y-chromosome Adam” (see below) suggesting a common time when both male and female most recent common ancestors (MRCAs) lived; even speciation, when used to represent a clear-cut evolutionary leap at a particular point in time; the human genome, suggesting uniformity across the species; and finally the human language faculty, as if it were a single monolithic entity uniform across the species. We focus on these because they represent, and generate, the most salient oversimplified ideas about which we wish to encourage due caution.

Mitochondrial Eve existed; there was a woman from whom all extant human mtDNA is inherited. Likewise Y-chromosome Adam existed. An example of the over-simple way in which these labels can be interpreted is found even in a scholarly psychological monograph: “Evolutionists say that there was a first human, and, on the basis of DNA evidence, that this human was a woman” (Paivio 2006: 283). The explicit allusion to the biblical myth, suggesting that this Adam and Eve cohabited, and that we are all the fruit of their union, is of course misleading, as is widely recognized. But even if we are careful to avoid a romantic Garden-of-Eden scenario, the very mention of particular individuals as somehow privileged ancestors of all that is to be found in the modern genome is misleading. For any pair of our roughly 30,000 genes, there is no implication at all that their modern variation can be traced to the same single individual as their most recent common ancestor.

When discussing the relative merits of a metaphor, it is always important to specify in which contexts it is useful and where it starts breaking down. Thus, we do not deny the value of the metaphors of the (human and language evolutionary) cradle, genealogical trees or the human language faculty; these certainly represent contextually valid approximations and operationalizations of a complex reality. *In extremis*, without such metaphors, science would be unthinkable. But continuing to use them beyond their limits risks distortion of reality. Tree diagrams, for example, are seductive. They are a handy way of visualizing relationships. Unfortunately, they are often used in diametrically opposed ways, with time correlated with either divergence or convergence of lines in a tree. Figures 9.1 and 9.2 give two common examples.

Both trees are “family trees,” but note that they relate to the dimension of time in directly opposite ways. The phylogenetic tree branches forward in time; the royal family tree branches backward in time. Both trees show ancestry, and both are oversimplifications. A more realistic diagram would combine properties of both figures. Given a chosen period of time, and some chosen set of related entities existing during that time, be it individual people, or species, or languages, the diagram would show all their ancestors and all their descendants within the chosen time frame, resulting usually in a lattice. In such a lattice, there would be examples of both

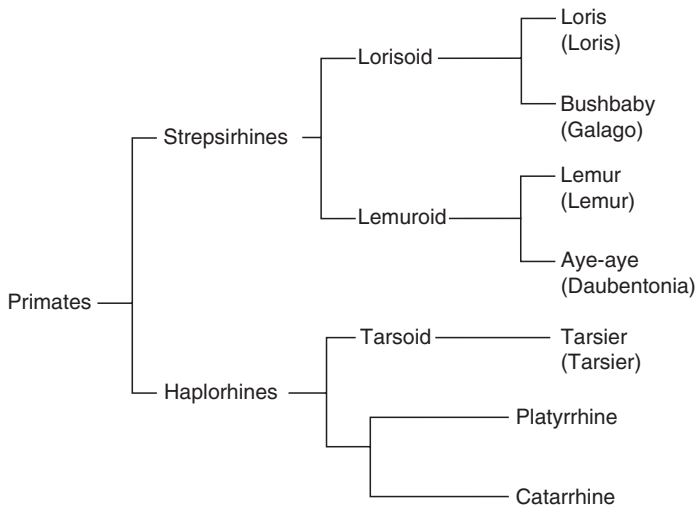


FIG. 9.1 Phylogenetic tree.

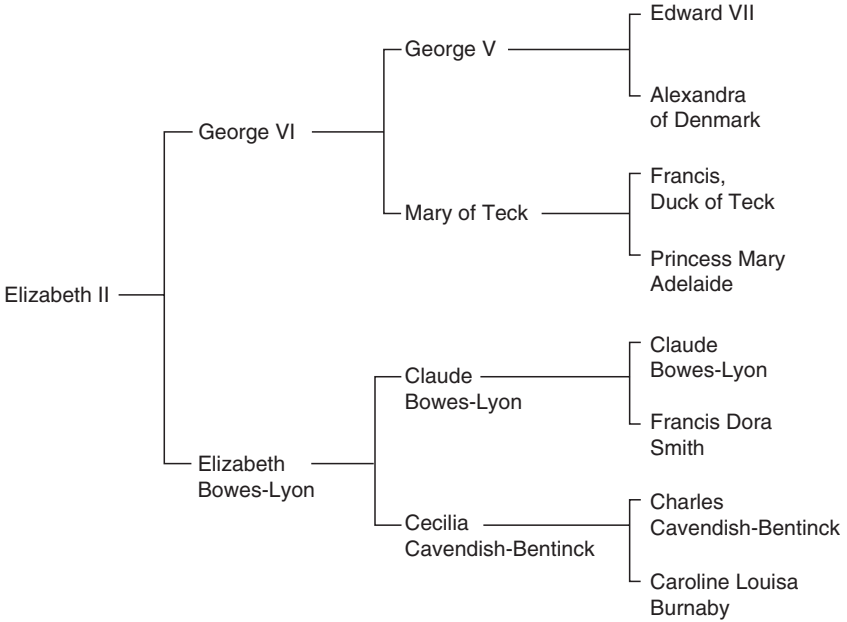


FIG. 9.2 Royal family tree.

divergence (many descendants of one entity) and convergence (many ancestors of an entity) over time—see Figure 9.3 for a simple example. Given the nature of biological species (discussed below), diagrams representing relationships between them would be very well approximated by a tree in most cases and not a lattice, unlike a diagram of family relationships among individuals. Therefore, this metaphor must be used and interpreted in conformity to its actual context, its representational power must be clearly specified in each case, and alternative representational methods must be employed when necessary (Jobling et al. 2004).

There are cases where an element in the lineage of a genome or of a language diverges at some point from other elements and is temporarily (maybe for a long time) passed down along a separate lineage from them, but later rejoins their lineage. In the case of species, this can happen where, for example, a species splits into two populations which have little contact for a long time but then intermix again (hybrid zones due to secondary contact; e.g. Skelton 1993: 382). A possible example relevant for human evolution is represented by a locus on the X chromosome (Xp21.1) for

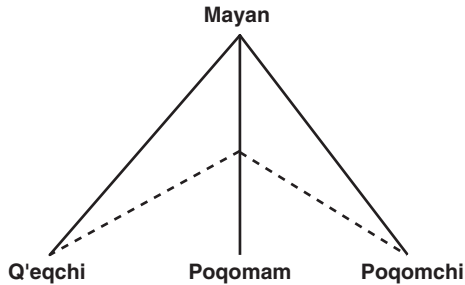


FIG. 9.3 Three separate daughter languages from a common stock. After their separation, a sound change in one is diffused to the others.

which a non-coding sequence of 17.5 kb length has been identified in two African individuals which has not recombined with other lineages for over a million years, suggesting that this X chromosome lineage evolved in isolation from the other lineages (Garrigan et al. 2005a). In the case of languages, an example is given by Campbell (2004: 198): “Q’eqchi’, Poqomam and Poqomchi” [Mayan languages] share change (18) (*\*ts > s*); however, documents from the sixteenth and seventeenth centuries reveal that this change took place long after these three were independent languages and that the change is borrowed, diffused across language boundaries.” This detail of the history of these Mayan languages is shown in Figure 9.3.

The “cradle” metaphor aptly suggests subsequent growth and change of the entity that starts life as the “baby,” with contributions that were in no way present in any original “blueprint.” Another merit is to bring into focus the special (geographically extensive) place and (evolutionary long stretch of) time represented by Africa between about 2 million years ago and 100 thousand years ago for human and language evolution. And yet another undeniable merit is to highlight the adherence to the evolutionary stance, whereby descent with modification from common ancestors due to random or selective factors represents the fundamental key to modern biology.

It would be wrong to overinterpret the “cradle” metaphor as suggesting a particular moment of conception of a single continuing unified entity (the “baby”) which somehow remains “the same thing” despite all the changes and innovations that it undergoes. For practical purposes, societies are prepared to accept that persons remain in some sense identifiably

“the same thing” throughout their lives, sometimes implicitly qualified by a statute of limitations. But, as we will illustrate, it is misleading to assume a single unified source for all the genes that a person carries. Furthermore, those genes did not all appear on the scene at the same time. Even the term *lineage*, applied to a person, has erroneous connotations, suggesting a single line of descent for the totality of a person’s genes, with no tributaries or distributaries.

We suggest that weight should also be given to another powerful metaphor, the “melting pot,” where new entities are forged from multiple sources. The modern USA is a melting pot, whose population comes from all over the world. It makes no sense to speak of the distinctive “ancestors of (all) modern Americans,” in the sense in which it might just be sensible to conjecture about the distinctive ancestors of the Ainu or the Andaman Islanders. To be sure, all modern American humans are descended from the same stock as all other humans, but that stock has branched out and later recombined, at different time depths.

In successive sections of this chapter we will discuss the diversity and multiple sources of human languages, the human genome, and the human language faculty.

## 9.2 Languages are confluences of features from many sources

Let us first recognize that the notion of “a language” is itself no more than a useful simplification.

Dan Dediu and Jim Hurford both speak English, but is it the same language? It depends on how fine-grained you want the answer to be. At a level of fine detail, we don’t speak the same language; Dan has a Romanian accent and Jim has traces of a British regional accent, and there are differences at lexical, grammatical, and pragmatic levels. But for all practical purposes, it is useful to say that the English that we speak is the same language. OK, so we’ll accept this simplifying idealization of “English” as a unified entity. But does this English have a single unified source, as is implied by traditional family tree diagrams showing it as having a single lineage, back through Proto-West Germanic and Proto-Germanic, to Proto-Indo-European? No.

What is the mother language of English, Proto-Germanic, or Proto-Romance? But why not in part Proto-Afro-Asiatic, as English has borrowed

*algorithm*, *alcohol*, and other words from an Afro-Asiatic language? Or why not in part Proto-Eskimo-Aleut, as English has borrowed words (e.g. *kayak*, *igloo*) from languages of this stock? The sensible pragmatic answer is of course that only a few tiny bits of English come from these sources. When using trees, we are neglecting these minor contributions in the interests of highlighting the hypothesis of a single proto-stock that we can think of as representing our *Urmuttersprache*. Therefore, since these few borrowings are such a slim part of English, we will ignore them in the following, and return to the more mainstream question of whether English is Germanic or Romance. The only sensible answer is that it is a bit of both. English, like French and Italian, lacks the case systems and verb-final subordinate clauses of its closest Germanic relatives, German and Dutch. English has vocabulary derived from both Germanic and Romance sources. However, the basic vocabulary of English (including kin terms, numerals, and bodypart terms) is Germanic. But why give those words a privileged status, unless for the purpose of highlighting the Germanic nature of English? That last sentence, all of it impeccable English, had a mixture of Germanic words (*give*, *word*, *the*) and Romance words (*status*, *nature*, and even *Germanic*). Thus, where is the source of English? There is no single source: “English” has some Germanic, some Romance, some tiny Sino-Tibetan components, etc., etc. The received wisdom about English is that it is Germanic, because that is where its basic vocabulary comes from. But in some basic respects it has French-like syntax (lack of cases, SVO word order).

A completely realistic diagram of the historical sources of English would not be a tree, but a lattice showing how different parts of the language had different sources. This is not to deny that large slices of a language can have common sources. With enough graphic ingenuity it is possible to draw a lattice in such a way that the genuine tree-like relationships stand out, perhaps shown as heavier lines. The discussion here echoes, of course, a debate that raged within historical linguistics in the nineteenth century between proponents of family tree (*Stammbaum*) theory and wave theory. Wave theorists (e.g. Schuchardt 1868; Schmidt 1872) proclaimed that “chaque mot a son histoire” (every word has its own history). This unfortunately ignores generalizations across words that, for example, have undergone the same sound change.

But, equally, “genetic relationship, the only thing represented in family-tree diagrams, is not the only sort of relationship that exists among



languages—for example, languages do also borrow from one another” (Campbell 2004: 212). But note even here, in this quotation from a mainstream historical linguist, the presupposed mutual exclusivity of “genetic relationship” and borrowing. The marginalization of borrowing is endemic in the literature, as a quotation from another linguist, diametrically opposed to Campbell on many issues, shows: “Linguists employ a number of well-known techniques to distinguish borrowed words from inherited items” (Ruhlen 1994a: 279). Why is borrowing less “genetic” than other language changes? The only difference is in the source—a feature inherited from a minority source is labeled “borrowing,” while a feature inherited from a majority source is “genetic.” The Celtic population of Gaul switched during the five centuries of Roman occupation from speaking a Celtic language to speaking a mainly Romance language, leaving behind only a few Celtic relics, such as the partly vigesimal numeral system. This conversion process started by some Celtic speakers borrowing some Romance words, then over time more words were borrowed, until almost the whole vocabulary was of Romance origin. The current allegedly “genetic” Romance status of French is a result of wholesale borrowing! Mufwene (2001: 109–112) makes a similar point, also mentioning contact between Romance and Celtic languages, in a section entitled “How language contact has been downplayed.”

It might be argued that whole Celtic and Vulgar Latin languages existed in parallel, and that speakers were either monolingual or bilingual, with a gradual population shift to monolingual Romance speakers. This idealized scenario would preserve the “genetic” integrity of the two systems, but it ignores the widespread phenomenon of code-switching in such contact situations, giving rise to a mish-mash language, which in this case would have been partly Celtic, partly Romance. It is likely that anyone born in Gaul toward the end of the Roman occupation spoke a variety which was a mixture of originally Latin features and originally Celtic features. Over time, the ex-Latin features came to dominate. It might also be argued that a difference between genetic/inherited traits and borrowed traits is that the former are a product of children learning their language from their parents, while borrowed traits are adopted by adult speakers. We doubt that any such sharp distinction can be sustained.

Ruhlen (1994b: 272) writes, “all the world’s languages share a common origin.” This gives the impression that there was once a language, as complete and complex as any extant human language, which was the

Mother Tongue. Ruhlen and his co-author Bengtson have distanced themselves from this view (Bengtson and Ruhlen 1994), but other workers in the same “macro-comparatist” program have used such suggestive titles as “The mother tongue: how linguists have reconstructed the ancestor of all living languages” (Shevoroskin 1990).

Ruhlen’s dominant theme, which he pursues in common with other macro-comparatists, such as Greenberg and Shevoroskin, is classification of languages as if they were all the same kind of entity in all respects relevant to the classification. Features deemed irrelevant to the classification are ignored, or marginalized as borrowing. Both Merritt Ruhlen and his vociferous opponent in matters of linguistic reconstruction, Lyle Campbell, are staunch family-tree men; they both picture the significant relationships between languages as ever-divergent trees. Where these scholars differ, irreconcilably, is in the time-depth at which it is possible to postulate ancestor forms. Ruhlen and colleagues believe that some form–meaning pairings survive recognizably enough and across such a range of language families that one can postulate “proto-world etymologies.” Such claims have been the subject of fierce controversy, on which we take no stand here.

Suppose that Merritt Ruhlen is right and there were indeed at least 26 single protoforms from which words that can be found in most modern language families are derived; then this only tells us about the mothers of forms for those meanings. It does not reconstruct any single (presumably African) mother language of all human languages. The etymologies of many other words that may have co-existed with proto-world \*TIK (= “finger, one”) and proto-world \*PAL (= “two”) would have come to evolutionary dead ends long ago. And many completely new words were coined, in different languages, long after the existence of the proposed proto-world. Ruhlen (1994a) himself is careful to say that he is not attempting reconstruction of proto-world, but only postulating global etymologies; however, the subtitle of his other book published in the same year, “tracing the evolution of the mother tongue” (Ruhlen 1994b), definitely suggests an attempt to describe an actual historic entity, the “mother tongue.”

It is sometimes suggested (Bickerton 1990, 1995) that *Homo erectus* may have had “protolanguage,” i.e. a syntaxless vocabulary-only form of communication, and that fully syntactic language came with *Homo sapiens*. If so, and if Ruhlen’s global etymologies have any validity, then these

ancient form–meaning pairings could conceivably be of far greater antiquity than even Ruhlen has dared to suggest. But in this case, the ancestral form of communication that contained such pairings would not have been a language in a fully modern sense, since it had no syntax.

### 9.3 Multiple sources and heterogeneity of the human genome

Now turning to the human genome, a recent study (Zerjal et al. 2003) suggests Genghis Khan's direct patrilineal descendants today constitute about 8% of men in a large area of Asia (about 0.5% of the world population). Thus the male most recent common ancestor (MRCA) of these men is much more recent than their female MRCA. The MRCA of human mitochondrial genes is probably of greater antiquity than the MRCA of human Y-chromosome genes. The same applies to all characteristic human genes. Some are (much) older than others. mtDNA and the Y-chromosome are a tiny proportion of human DNA. For the general case, Dawkins has made the point that for particular genes, an individual human may be more closely related to some chimpanzees than to some humans. Blood groups are an example; a man may have the same blood group as a chimpanzee but have a different blood group from his wife: "every gene has its own tree, its own chronicle of splits, its own catalogue of close and distant cousins... individuals are temporary meeting points on the criss-crossing routes that take genes through history" (Dawkins 2004).

Of the particular genes affecting human language, they also vary in antiquity. The human variant of FOXP2 is widely claimed to have appeared within the last 200,000 years, although a study presented in the current volume (Diller and Cann this volume) claims much greater antiquity—a claim also supported by the very recent finding that modern humans and Neanderthals share this variant (Krause et al. 2007a). According to a recent study (Dediu and Ladd 2007), variants of two more genes, the derived haplogroups of *ASPM* and *Microcephalin*, may also be relevant to human language. These recently evolved variants are rare in Africa, probably originated outside Africa, and are still under positive selection, not being yet fixed in the human population. Dediu and Ladd claim that there is a correlation between the frequencies of these variants in a population and the usage of tone contrasts in the language(s) spoken by it.

They argue that this correlation is non-spurious, in the sense that it cannot be explained by other factors. The mechanism linking genes and tone could be tiny acquisition and/or processing biases affecting the cultural transmission of language, and thereby influencing the trajectory of language change.

There is no single story the genes can tell; each bit of DNA potentially has something different to say, if properly asked. Each gene can recount its own version of history, its jumping from body to body across generations, its struggle to outsurvive its competitors by making the bodies it inhabited better than the others in innumerable various ways. We must take the intrinsic diversity of these stories into account while trying to create a faithful reconstruction of the past.

Probably the best-known bits of our genome are represented by mitochondrial DNA and the Y-chromosome, the first (mtDNA) being transmitted down the generations exclusively through the maternal line (Jobling et al. 2004; Seeley et al. 2005; Lewin 2004) while the second contains a segment (NRY: the non-recombining part of the Y-chromosome) which is exclusively transmitted through males (Jobling et al. 2004). This property makes them very well suited for evolutionary and historical studies, because their history is the history of each sex, separately: mtDNA tells us the adventures of the females while NRY tells those of the males—at least, as a first approximation.

But even in these simple cases things get very complex. There is a much greater difference in fitness (reproductive success) among men than among women. Due to the special way in which both mtDNA and NRY are transmitted, it is a logical necessity that for any group of humans, living, extinct or a combination thereof, there can be found a single individual (female or male, respectively) from which all the group's variants of mtDNA or NRY originated (Dediu 2007; Relethford 2001). This individual represents the MRCA of the genetic variants present in the specific group under study.

In their seminal study, Cann et al. (1987) reconstructed the MRCA of living humans' mtDNA as dating from approximately 200,000 years ago and probably located in Africa, and ignited the popular imagination with an African mitochondrial Eve from which all mtDNA stems. Shortly afterwards, the parallel concept of a Y-chromosome Adam appeared, which, as expected, has a different age than the mtDNA Eve, approximately 60,000 years (Thomson et al. 2000; Underhill et al. 2000).

When leaving the special cases of these sex-linked genetic systems (mtDNA and NRY) and moving into the realm of recombining genes, the story becomes much more complex, as the history told by such a gene has no intuitive counterpart at all. And, again, these histories do differ, sometimes remarkably so. For example, the vast majority of the genes of living humans seem to come from Africa, but the ages of their MRCA are widely different. Some are fairly recent (the derived haplogroups of *ASPM* and *Microcephalin*, estimated at some 5,000 and 37,000 years ago), others are old (predating the chimp–human split, like some alleles of the major histocompatibility system; e.g. Loisel et al. 2006), and yet others are extremely old (predating the vertebrate splits; e.g. Venkatesh et al. 2006).

The complex and varied histories of genes are further illustrated by this example. A segment of the X chromosome (the Xp21.1 locus) presents a very rare lineage confined to certain African populations which seems to have evolved in isolation from the other lineages for more than 1 million years (Garrigan et al. 2005a) suggesting the existence of long-lasting splits inside our species. Other parts of the X chromosome have even stranger stories to tell, including the *HS571B2* locus (Yu et al. 2002), presenting a variant which is suggested to have arisen in Eurasia more than 140,000 years ago, or the segments of the *Dystrophin* gene analysed by Ziętkiewicz et al. (2003), having three lineages, one of them suggesting a non-African origin earlier than 160,000 years ago. But probably the most striking example is represented by the *RRM2P4* pseudogene (Garrigan et al. 2005b), which has an old MRCA (around 2 million years ago) and probably an Asian origin. Of course, all these examples could in fact be due to statistical error, but if not, then not only does their existence highlight the diversity of points of view carried down the ages by different genes, they also throw some doubt on the standard model for human evolution, which posits a recent African origin for modern humans, followed by a rapid expansion across the world with the total replacement of the pre-existing local archaic forms (for a full discussion, implications and class of most probable models, see Dediu 2006, 2007).

One might argue that there may be diversity among human genes, but still there is a single human genome; after all, we are such a uniform species. And in some fundamental way, this is right. However, as shown by the HapMap project (The International HapMap Consortium 2003; [www.hapmap.org](http://www.hapmap.org)), taking into account the diversity of our species is important not only for understanding our origins and history, but also for fighting

disease and promoting health and quality of life. While it is true that humans are much more uniform than other comparable species (Jobling et al. 2004; Relethford 2001), this does not entail that we are genetic clones. There is a pervasive claim, often cited without any reference, that humans are so uniform and unstructured that the division of *Homo sapiens* into groups is not justified by the genetic data, and people all over the world are much more similar genetically than appearances might suggest. This is formulated by Edwards (2003: 798) as the claim that about 85% of the total genetic variation is due to individual differences within populations and only 15% to differences between populations or ethnic groups, a claim which can be traced to the work of Richard Lewontin (1972). However, this simplifying claim is misleading as it neglects the fact that the structure of the human species is not given by a few independent diagnostic genes, but by the correlations between the frequencies of many different alleles across populations (Jobling et al. 2004; Rosenberg et al. 2002; Bamshad et al. 2003). Thus, there is enough genetic structure to allow reliable prediction of population of origin using a limited number of loci; however, it is not population-specific loci which allow this classification but their correlational structure.

Thus, there is genetic diversity across the human species and each gene has a different history. This inescapable conclusion could potentially have a significant impact on our efforts to understand the evolution of language, suggesting that the evolved language capacity consists of elements with different genetic histories. There has always been a tendency to see language as an all-or-nothing phenomenon, brought into existence by some sort of explosion or sudden revolution. A recent example is Tim Crow's (2002b) effort at identifying a single gene that played a critical role in the transition from a precursor species to modern *Homo sapiens*, hypothesized to be the *protocadherinXY* gene located in the X-Y homologous region. Another theory involving a single gene bringing about language concerns FOXP2, a gene of the forkhead box family which act as transcription regulators (Lai et al. 2001; Scharff and White 2004). Heterozygous carriers of deleterious mutations of this gene develop a complex phenotype including articulatory problems, cognitive impairments, and language impairments (Bishop 2003; Fisher et al. 2003; Vargha-Khadem et al. 1998; Lai et al. 2003; Watkins et al. 2002a, b), which suggested to some that this gene might have something specifically to do with language. Moreover, evolutionary considerations suggested that the

human-specific form of the gene appeared during the last 200,000 years of human history, that is, concomitant with or subsequent to the emergence of anatomically modern humans (Enard et al. 2002), boosting the claims that this might be the gene explaining language, modernity, and everything else. However, it turns out that this story is much more complex (Dediu 2007: 111–120), that the estimation of this age is fraught with difficulties, that the human-specific variant is not that specific to humans after all (Webb and Zhang 2005; Zhang et al. 2002), that in birds and vocal-learning mammals FOXP2 does not seem to explain much (Webb and Zhang 2005; Teramitsu et al. 2004; Scharff and Haesler 2005; Haesler et al. 2004; Shu et al. 2005) and, finally, that the human variant is much older (Diller and Cann this volume; Krause et al. 2007a). In the end, it seems that the effects of FOXP2 are much more subtle than simply enabling language, probably creating a permissive environment in which vocal learning can evolve if other circumstances/factors come into play (Scharff and White 2004: 342).

Alternative models of language evolution, involving the slow, gradual accretion of various aspects of our linguistic capacity, have been proposed before (e.g. Pinker and Jackendoff 2005; Smith 2006; Corballis 2004; Hurford 2003a). Theories of this type require that small genetic changes impacting (not necessarily directly) on language are selected, and increase in frequency until eventually reaching fixation. However, this standard neo-Darwinian account essentially implies population-level genetic variability concerning language, an idea not seriously considered in linguistics and allied disciplines (e.g. Cavalli-Sforza et al. 1994), which might seem unexpected given the amount of data from behavior genetics suggesting high genetic components of inter-individual abilities and disabilities connected to language (Dediu 2007; Stromswold 2001).

The possible nature of this mechanism was suggested in a recent study (Dediu and Ladd 2007), where the inter-population diversity of two brain growth and development-related genes was related to the distribution of tone languages. *ASPM* and *Microcephalin* are two genes whose deleterious mutations cause primary recessive microcephaly (Gilbert et al. 2005; Cox et al. 2006; Woods 2004) and for which two derived haplogroups have been identified (denoted in the following as *ASPM-D* and *MCPH-D*, respectively), showing signs of ongoing natural selection in humans (Mekel-Bobrov et al. 2005; Evans et al. 2005). These



haplogroups have appeared recently (approximately 5,000 and 37,000 years ago, respectively) and *MCPH-D* even seems to have introgressed into the modern human lineage from another archaic form (Evans et al. 2006). In spite of many attempts, the phenotypic effects of these haplogroups which explain the selective pressure have not been found: They seem not to be connected to intelligence (Mekel-Bobrov et al. 2007), brain size (Woods et al. 2006), head circumference, general mental ability, social intelligence (Rushton et al. 2007), or the incidence of schizophrenia (Rivero et al. 2006). The proposal of Dediu and Ladd (2007) is that *ASPM-D* and *MCPH-D* might determine a very small bias at the individual level in the acquisition or processing of linguistic tone, a bias which can be amplified in a population through the cultural transmission of language across generations, and manifested in differences between the languages spoken by such populations. They support this hypothesis by the fact that the population frequencies of *ASPM-D* and *MCPH-D* correlate negatively with the usage of linguistic tone by that population, even after geography and shared linguistic history have been controlled for.

That such biases can work has been suggested previously by both computer models (Smith 2004; Nettle 1999b) and mathematical models (Kirby et al. 2007), but, if confirmed by further experimental studies, this would represent the first case of a genetically influenced linguistic bias manifest at the population level. And this type of bias could represent exactly the mechanism required for gradual, accretionary models of language evolution, whereby small genetic changes appear, influence the capacity for language in various populations, and eventually became part of the universal linguistic capacity. This model suggests that linguistic and genetic diversities are the key for understanding the universal properties of language.

The human language capacity is commonly said to be uniform across the species. Certainly, a baby born of Chinese parents and adopted into a French-speaking family will learn French just as easily as it would have learned Chinese. But the affirmation of uniformity comes with a typical reservation that it excludes pathological cases. The pathological cases are certainly still human, so the language faculty is not in fact uniform, and there is no principled way of separating cases defined as pathological from the tail of a distribution, so it seems likely that even among non-pathological cases there is some variation in the language faculty. It is well



established that there are differences in aptitude for second-language learning (see an extensive literature in applied linguistics with Carroll (1962) as an early example). It would be surprising if some of the differences in second-language learning were not also reflected in differences in first-language learning. If the language faculty evolved by natural selection of advantageous variants (not in reasonable doubt), there must have been variability in the evolutionary precursors of the language faculty. One possible variable is the different dispositions of individuals to innovate linguistically; some language users are more creative with their language than others, pushing it beyond current limits.

Obviously, innovation had to be involved in the evolution of languages to their current complex state. New words, new constructions, and new phonemic distinctions arose. We do not envisage that such innovation was necessarily deliberate or a matter of conscious choice. So a disposition in some individuals to innovate is necessary for a language system to get off the ground. But a disposition to innovate is not necessary to maintain a language in a population, once the system is already up and running. All that is required is a capacity to acquire the language of the community.

This theoretical point is made convincingly by Smith (2002), who computationally modeled various postulated innate strategies for learning arbitrary meaning–form pairings, i.e. vocabulary items. Repeated cultural transmission of the vocabulary is modeled, with one generation producing examples of the form–meaning pairs they have learned, for the next generation to learn from. Initially, at “generation zero,” the population has no common vocabulary, and the whole population is genetically uniform, having the same postulated vocabulary acquisition bias. The learners were modeled with little neural nets mapping between meanings and forms, and the different learning biases investigated were modeled by using different weight update rules. Initially, the members of this artificial population produced random forms for the meanings they were prompted to express, and the observers of these form–meaning pairs responded by internalizing weightings of their preferences of form–meaning mappings, as dictated by their innate learning mechanism (i.e. their weight update rule).

In this way it was possible for Smith to compare the effects of 81 different theoretical innate biases applied to the task of vocabulary learning. And, given that the population always started with no common

vocabulary, it was possible to see under what circumstances a common vocabulary emerged, suitable for consistent communication about the meanings involved. In some cases no system emerged at all, with the simulated agents merely continuing to produce random signals at each other, and not building up a common vocabulary. In other cases, with different innate learning biases, a system got off the ground, and could be used for consistent communication. The difference between the two cases is between a population-wide innate bias enabling the group to construct a communication system, and on the other hand a similarly shared bias which does not enable the group to progress beyond producing random signals which cannot be consistently interpreted by other group members. Smith accordingly labeled a particular subclass of biases as system constructors. In some sense, agents with one of these biases could impose order on chaos, very much in the sense in which, in the Chomskyan picture of language acquisition, children induce a coherent linguistic competence from degenerate data. Other innate biases were ineffective at constructing a system in this way, but Smith showed that a further subclass of them, which he labeled maintainers, could acquire a system already established in the population and use it effectively in communication. The behavior of these maintainers was consistent enough for the system to be faithfully transmitted to the next generation of learners. All constructors are maintainers, but not all maintainers are constructors.

While specifically concerning the vocabulary, this result could have more general implications in that it is quite possible for a population that has in the past developed a consistent system to be genetically heterogeneous (polymorphic) with respect to their language acquisition dispositions. The early stages of evolution need a critical mass of system constructors, but once a system is constructed, maintainers who are not themselves richly enough endowed to be constructors can function communicatively in the group and pass on the system to their children. Given the extent of polymorphism generally, in humans as in other species, some degree of polymorphism in the language faculty should not be surprising. If linguistic innovation is occasional and sporadic, it would not be immediately evident that there were different dispositions in the population. Indeed it is theoretically possible, though unlikely, for the constructors to become extinct, with the continuance of the communication system sustained culturally by the remaining maintainers.

#### 9.4 Varying antiquity of the human language faculty

In this section, after some definitional preliminaries, we discuss various aspects of the human language faculty, making a rough division between recent features which have evolved only in humans to any significant degree, and ancient features which are found in other animals, especially primates. For the more recent aspects of the language faculty, such as a specialized vocal tract, and episodic memory, it seems likely that they evolved during the emergence of *Homo sapiens* and therefore in Africa. For the more ancient aspects of the language faculty, such as basic syllabic organization, mental reference to objects, and the rudiments of propositional form (kept private), they certainly evolved or at least began to evolve long before the emergence of humans, and some are probably so ancient as to predate the formation of the continent of Africa, over 100 million years ago.

Hauser et al. (2002) make a useful distinction between the faculty of language in the broad sense (FLB) and the faculty of language in the narrow sense (FLN). FLN includes only that which is special to language and is found in no other human cognitive domain or animal communication system. Hauser et al. (2002) suggest that FLN may consist of nothing more than the human capacity for recursive computation, and perhaps not even that, if examples can be found of recursion in non-linguistic systems, such as animal navigation. This distinction helps to clarify what researchers are interested in as denoted by the vague term *language*. In the recent history of linguistics, generative linguists have focused on language in the narrow sense, aiming at a theory of FLN. Sometimes they have avoided the overly general term *language* and used *grammar* instead, referring to just the formal organization of the sound–meaning pairing system represented in the brain. Other linguists have cast their net more widely, investigating aspects of language use (e.g. discourse analysts and phoneticians) or the interaction of non-linguistic factors, such as short-term memory, on laboratory examples chosen to highlight grammatical contrasts (psycholinguists). Such researchers are investigating FLB. FLB includes anything involved in the learning, mental storage, and use of language, capacities which may well be also used for non-linguistic purposes. We write here of FLB. It is important to note that even FLB is unique to humans; it is a unique combination of traits that can

be found in other activities and also in some animals. The individual components of FLB are not unique to human language (by definition), but their combination, which makes us unique among animals, is unique.

“Used for nonlinguistic purposes” has a paradoxical ring to it in the context of language evolution, where things in fact happened the other way around. The language faculty, in the broad sense, was assembled out of capacities and traits that initially had nothing to do with language (because language didn’t yet exist), but which were exapted (Gould and Vrba 1982) and became used for linguistic purposes.

The vocal apparatus is a prime example. The lungs, trachea, larynx, tongue, and lips were variously used for breathing and eating. These anatomical structures had their earliest “cradle” in the very ancient past, long before the continent of Africa was formed. The vocal tract, like the brain, has undergone radical evolution since the split from chimpanzees, most plausibly in the service of the capacity to make ever finer phonetic distinctions (Lieberman 1984). In the narrow generative view, the cognitive faculty of language is independent of its output modalities, since, as deaf sign languages teach us, the same expressive power can be achieved without the use of the vocal tract. Nevertheless, the vocal/aural medium is the dominant output modality for language, and the human vocal tract is unique among primates in the range of distinctive sounds it can produce.

The physiological details of the human vocal tract are an example of relatively recent evolution, having happened over the past 3 million years, at the very most. It seems likely that there were also very significant cognitive developments over the same period, perhaps including the advent of a developed capacity for recursive computation. One such relatively recent cognitive development is the emergence of episodic memory. Episodic memory is memory for specific events, located at particular points in time. Episodic memory is what is lost in amnesics, who, for instance, cannot recall where they woke up this morning, or any specific events of their former lives. But such amnesics have good “semantic memory” for timeless facts, such as geographical facts and the relationships between words. There is a large and lively literature on whether episodic memory is unique to humans. Naturally, a lot depends on precise definitions. It is clear that animals who hide food for later use have “episodic-like” memory. Scrub jays can recall what kind of food they hid, where, and how long ago (Clayton and Dickinson 1998; Clayton et al. 2001; Clayton et al. 2003; Griffiths et al. 1999). A chimpanzee has been

shown to remember overnight where food was hidden by an experimenter (Menzel 2005), and a gorilla has been shown to remember quite recent specific events, up to fifteen minutes afterwards (Schwartz and Evans 2001; Schwartz et al. 2004; Schwartz 2005; Schwartz et al. 2005). Nevertheless it is clear that there is a very significant difference between humans and non-humans in their capacity for episodic memory (see Hurford 2007: ch. 3). Episodic memory is a component of the language faculty in the broad sense, FLB. Without a permanent way of mentally storing a record of who did what to whom, and when and where, human language would not be what it is today. And this capacity, being of apparently recent origin in its highly developed human form, almost certainly emerged in Africa, since the chimp-human split.

Just as there are examples of recent evolution at both the phonetic and the cognitive-conceptual “ends” of language, there are also examples of very ancient aspects of the human language faculty at both ends. Here, we will give just one phonetic and one conceptual example. The syllable is a basic unit of phonological organization in all languages. Syllables have a characteristic shape, phonetically defined. The basic syllable shape, found in all languages, is CV, a single consonant followed by a single vowel. It has been persuasively argued that, both in ontogeny and in phylogeny, the syllable is more primitive than either of its components, the phonetic segments analyzed as consonant and vowel (Meier et al. 1997; MacNeilage 1998). The basic CV syllable is produced with an articulatory gesture of opening the mouth from a closed position, accompanied by voicing. The close analog of such a gesture in humans can be seen in the cries and calls of many animals. As MacNeilage (1998: 499) writes: “The species-specific organizational property of speech is a continual mouth open-close alternation, the two phases of which are subject to continual articulatory modulation.” He further suggests that “ingestion-related cyclicities of mandibular oscillation (associated with mastication (chewing) sucking and licking) took on communicative significance as lip-smacks, tonguesmacks and teeth chatters - displays which are prominent in many non-human primates” (MacNeilage 1998: 499). Meier et al. (1997) refer to the “jaw wags” of infants aged between 8 and 13 months. To acknowledge the ancient origin of the syllable as a basic unit of speech is to recognize a continuous aspect of our evolution from non-human animals. This evolutionary foundation was laid down in its most basic

form hundreds of millions of years before humans emerged, and before Africa was formed.

At the other end of a language system from the phonetic syllable, we can look at the meanings expressed in linguistic utterances. The most common simple clause shape in languages involves a predicating expression, typically a verb, and from one to three nominal expressions. Often these nominal expressions are also directly referring expressions, picking out some particular entity in the world. Examples in English sentences are *Mary frightened John* and *Mary put the book on the table*. Such sentences describe “minimal subscenes” (Itti and Arbib 2006). Many non-human animals are clearly capable of observing an event or situation in the world, involving several participants, and analyzing it into its component entities and the relationship between them.

For example, experiments with baboons in the wild have shown that they exhibit surprise when they hear a recording of a dominant baboon making a submissive noise while a subordinate baboon makes a threatening noise (Cheney and Seyfarth 1999; Bergman et al. 2003). Baboons know the dominance hierarchy of their troop, and they can recognize each other’s voices. The surprise reaction shows that the interaction played back to the baboons is analyzed by them into the components of the two actors, the threatener and the submitter, against the background knowledge of the normal dominance relation between them.

The major difference between humans and non-humans is that we have evolved highly elaborate codes (languages) for telling each other in detail about the events that we observe (and now, of course, about much else). Baboons do not have any shared system for publicly reporting to each other who surprisingly threatened whom, and who surprisingly submitted. They keep their analysis of the event to themselves. And, given their lack of significant episodic memory, as discussed above, they probably don’t keep the perceived and analyzed event in memory for long. But the evidence shows that they do mentally perform such an analysis, into the entities involved and the relationship between them. That is, the basic propositional structure is present in the thought of the baboons, though they don’t express their thoughts in structured sentences. This theme is developed in much greater detail by Hurford (2007); see also Hurford (2003b), where it is argued that neural correlates of basic logical predicate–argument structure exist in many non-human animals, certainly primates, but also other vertebrates. This mental organization of

perceived events and situations is the private substrate upon which human public systems of communication evolved their grammatical subject–predicate structure.

The mental organization of perceived events and situations is a fundamental aspect of the organization of language, and it evolved long before the emergence of humans, and very probably before the emergence of the continent of Africa.

## 9.5 Conclusion

The three areas that we have surveyed here tend to suffer in the popular imagination from the same type of creation myth, suggesting a single source and a single moment of origin. It is important to stress the multi-stranded nature of languages, genomes, and phenotypic traits. The strands, throughout history, have diverged and recombined in multifarious ways, and new strands are constantly coming onstream through innovation.

# 10 How varied typologically are the languages of Africa?

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MICHAEL CYSOUW AND BERNARD COMRIE

## 10.1 Investigating typological variety

Our aim in this chapter is to investigate to what extent it is possible to pick up signals of prehistoric events by studying the distribution of typological diversity across the languages of Africa. The chapter is experimental, in the sense that it aims to test a particular method rather than to assume that the method in question is valid. The results of the investigation will show that, while there are clear limitations especially as one goes further back into history, nonetheless there are clear signals of prehistoric events that can be traced in the geography of typological diversity in Africa. Our aim is not to develop a method that will replace other methods, in particular the comparative method in historical linguistics (Campbell 2004), but rather to see what contributions can be made in specific areas by other methods, in this particular case areal typology.

When we speak of the geographical distribution of typological diversity, we are concerned with typological or structural features of languages, for instance whether they have phonemic tone or not, whether in their basic constituent order the attributive adjective precedes the noun or follows it, etc. Crucially, we are concerned with the extent to which languages are typologically, i.e. structurally, similar to one another or different from one another. Until recently, judgments of the typological distance between languages have been largely subjective, or restricted to a very small set of typological parameters. This situation has changed substantially with the publication of the *World Atlas of Language Structures* (Haspelmath et al. 2005, hereafter *WALS*). *WALS* provides detailed information on the geographical distribution of over 130 structural features across the languages of the world. The project relies on a basic sample of 200 languages, although for



some features the relevant data for a particular language are missing, while for others data are provided for more than the 200 languages of the basic sample. WALS comprises both a printed atlas and an online version WALS.info, the latter being particularly useful for carrying out linguistic research.

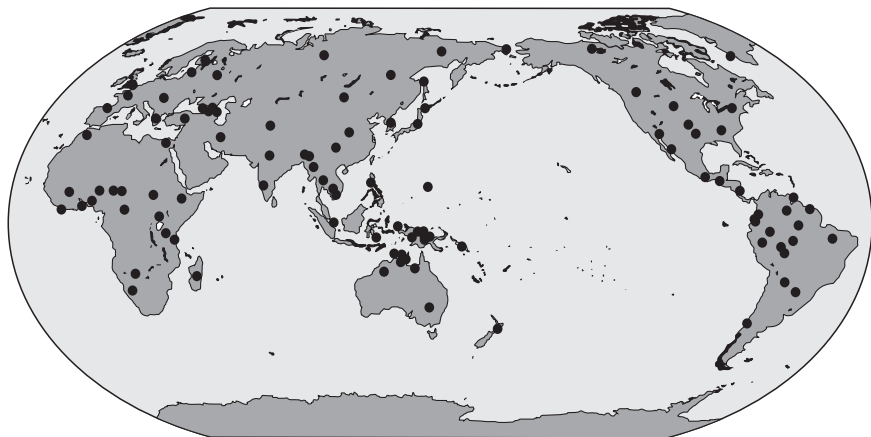
Using WALS, it is possible to measure the typological distance between two languages, essentially by calculating the number of structural features on which the two languages differ in value relative to the total number of structural features for which WALS provides data on both languages (known as the “relative Hamming distance” in biology or the “relativer Identitätswert” (RIW) in dialectology, Goebel 1984). Thus, if the number of features treated remains constant, a pair of languages will be typologically closer the more feature values they have in common, and typologically more distant the fewer they have in common. For the purposes of this exploratory study, we have not made any attempt to weight features differently. Although this is technically easily possible, it is not obvious on which basis linguistic features should be weighted (for attempts to establish weights of WALS features, see Wichmann and Kamholz 2008 for weights related to diachronical stability; and Cysouw et al. 2008 for weights related to the overall typological profile). Further, as noted above, there is the problem that WALS has a rather unevenly filled data table. Different languages that occur in WALS may occur in the treatment of more or fewer structural features. In order to maintain statistical reliability, we restrict ourselves in our various samples in this chapter to languages for which data on a sufficient number of structural features are available.

## 10.2 Africa in relation to the rest of the world

The first question that we pose is whether the languages of Africa, taken as a whole, form anything like a typological grouping, i.e. a set that is internally relatively homogeneous but also relatively distinct from languages spoken in other parts of the world.

### 10.2.1 *Africa and the whole world*

For this purpose, we first constructed a worldwide sample of 102 languages from WALS, as shown in Map 10.1. These 102 languages are chosen



MAP 10.1 A worldwide sample of 102 languages from WALS.

by selecting, first, the languages with the most available data points from each genus to avoid bias stemming from closely related languages.<sup>1</sup> Second, we restricted this “best per genus” sample rather arbitrarily to the 100 best coded languages, but ended up with 102 because various languages had the same number of available data points. We then constructed a NeighborNet (Bryant and Moulton 2004) expressing the degree of typological distance among the languages in the sample, shown in Figure 10.1. In this figure, similar languages are placed closer to each other, sharing parallel lines to the extent that they share linguistic similarities. However, languages are not forced into groups (as is the case in many other clustering algorithms), giving a visual impression of the amount of evidence for many alternative groupings.

The resulting network shows little internal structure, with nearly all languages being at the end of long lines unique to that language. Only few smaller groups of languages are discernible. This indicates that from a worldwide perspective, the structural characteristics from WALS do not show strong evidence for larger subgrouping of languages. Moreover, no distinctively African grouping emerges (the African languages are

<sup>1</sup> A genus plural: genera is a group of languages whose genealogical relatedness is visible by inspection, corresponding to a time depth of up to 2,500–3,000 years, roughly equivalent to the major branches of the Indo-European family, like Germanic or Romance.

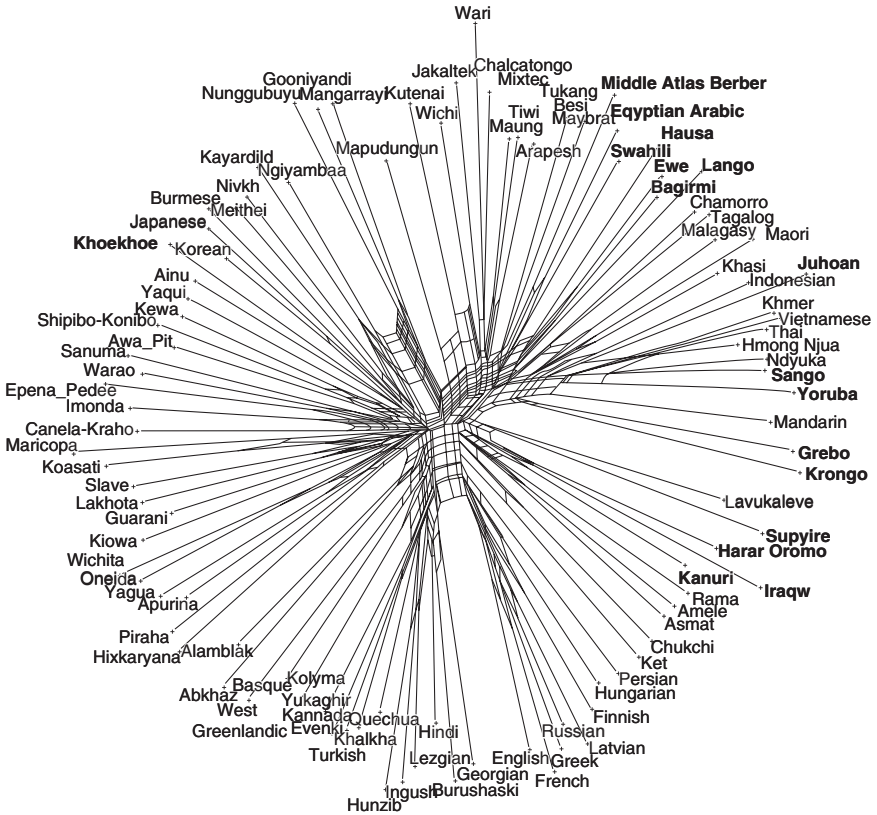


Fig. 10.1 NeighborNet of the 102-language sample; languages from Africa are in bold type.

shown in a larger and bold typeface in the figure). One African language, Khoekhoe, is placed very distant from the others, and even among the part of the network that contains the other African languages there are many intervening non-African languages, i.e. it is not uncommon for an African language to be closer to some non-African language than to some African language. Looking more closely at the smaller-scale clustering of African languages, various clusters are discernible, as summarized in Table 10.1. All these languages are from different genera, because this was one of the grounds on which the languages were chosen. However, even from a deeper genealogical perspective these groups do not show any consistent historical profile (shown in Table 10.1 are the large-scale African families Afro-Asiatic, Niger-Congo, and Nilo-Saharan as proposed by Greenberg 1963).

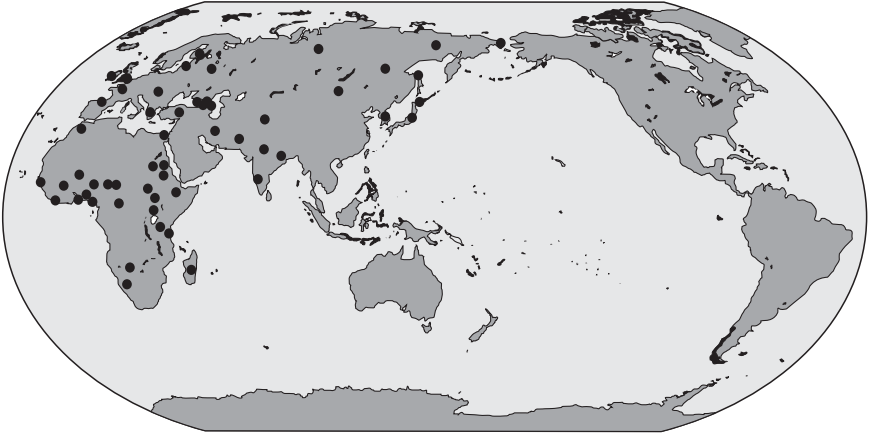
TABLE 10.1 *Small-scale clustering of African languages suggested by the NeighborNet.*

Language	Genus	Family
Middle Atlas Berber	Berber	Afro-Asiatic
Egyptian Arabic	Semitic	Afro-Asiatic
Hausa	Chadic	Afro-Asiatic
Swahili	Bantu	Niger-Congo
Ewe	Kwa	Niger-Congo
Lango	Eastern Sudanic	Nilo-Saharan
Bagirmi	Central Sudanic	Nilo-Saharan
Sango	Adamawa-Ubangian	Niger-Congo
Yoruba	Defoid	Niger-Congo
Grebo	Kru	Niger-Congo
Krongo	Kadugli	(disputed/unknown)
Kanuri	Saharan	Nilo-Saharan
Iraqw	Southern Cushitic	Afro-Asiatic
Harar Oromo	Eastern Cushitic	Afro-Asiatic

### 10.2.2 *Africa and Eurasia*

Second, we carried out essentially the same procedure again, but this time restricting ourselves to languages of Africa and Eurasia, with the 56-language sample illustrated in Map 10.2. The reason for this restriction is that we expect to find more structure in the language similarity when looking at continent-sized areas. Indeed, the resulting NeighborNet in Figure 10.2 shows considerably more structure than did Figure 10.1 and it reveals rather clearly an African clustering (the African languages are shown in a larger and bold typeface in the figure), though Khoekhoe is still in an isolated position relative to the other African languages.

However, before interpreting this result too far, we need to consider other factors. In particular, we know from other studies based on WALS (cf. Cysouw 2006) that typological distance correlates highly with geographical distance, i.e. that languages spoken in the same neighborhood tend to be typologically more similar to one another than languages spoken further apart. For the 56-language sample, this correlation is



MAP 10.2 A sample of 56 languages, restricted to Africa and Eurasia.

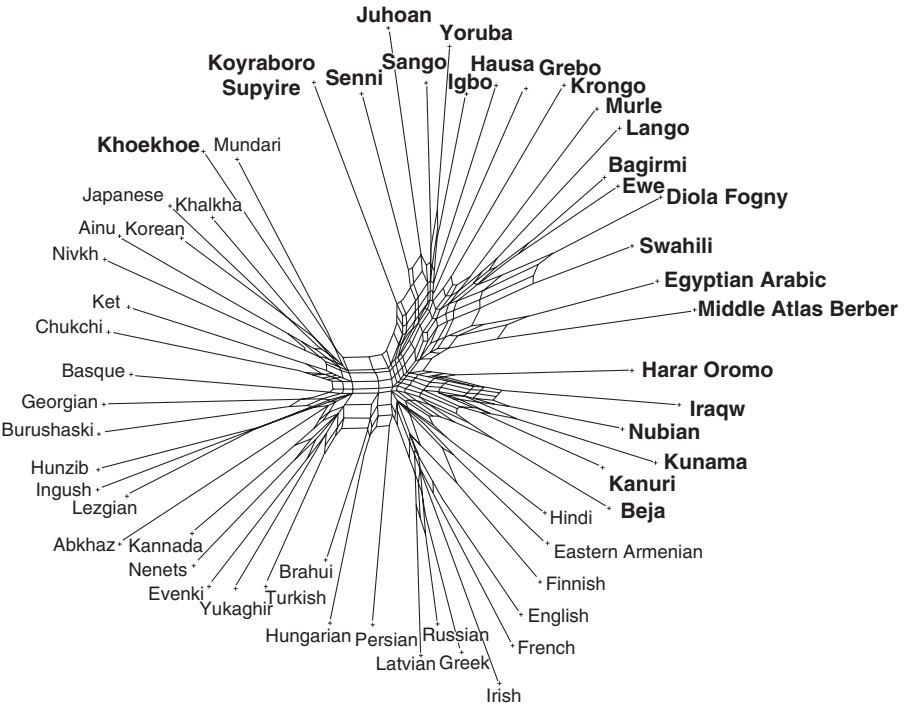


FIG. 10.2 NeighborNet of 56 languages, restricted to Africa and Eurasia.

shown in Figure 10.3, which plots typographical distance against geographical distance. There is a reasonably strong, and clearly significant, correlation between geographical distance and typological distance (Pearson's  $r = .39$ , Mantel Test  $p < .0001$ ). As Africa and Eurasia are geographically nicely separated, at least part of the distinction between African and Eurasian languages as found in Figure 10.2 can be explained by geographical distance.

There are various possible explanations for such a significant correlation between geography and linguistic structure. We favor an interpretation that gives prominence to horizontal transfer (i.e. borrowing). In contrast to biological diversification in the animal kingdom, horizontal transfer plays a very significant role in the history of language. We would like to suggest that the attested correlation between geography and typology is caused to a large extent by convergent evolution through borrowing (which is more likely to happen between geographically close languages). In the case of our language sample an alternative "isolation by distance" approach does not seem fruitful. First, the sample consists of languages that are not obviously related, so any spreads must have been very long ago, and, second, we are talking about massive geographical distances. Finally, note that relatively recent spreads of languages would

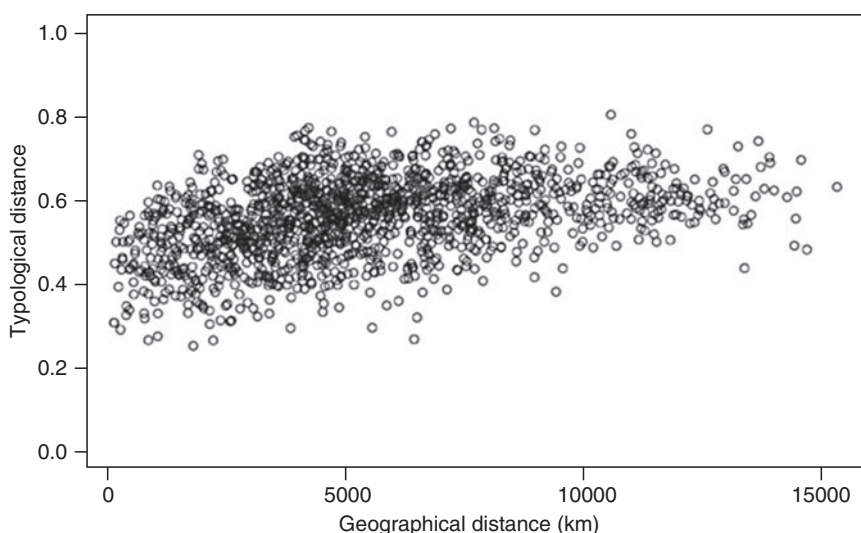


FIG. 10.3 Correlation between geographical distance and typological distance for all pairs of languages from the 56-language sample.

actually result in a less pronounced trend, as even far-away languages would still show strong similarities due to their common origin (cf. the case of the Bantu expansion discussed in section 10.3.2).

Looking somewhat more closely into the groupings discernible in Figure 10.2, it looks like the African languages (apart from Khoekhoe) are separated into two groups (going clockwise through the network: the first cluster ranging from Supyire to Middle Atlas Berber and the second ranging from Harar Oromo to Beja). Likewise, the Eurasian languages also seem to have a major division into two groups (the first cluster ranging from Hindi to Persian, the second from Turkish to Mundari; Brahui and Hungarian being somewhere in the middle). These two separations almost perfectly correlate with the order of object and verb (Dryer 2005) as summarized in Table 10.2. We found the same strong impact of the order of object and verb on the overall typological similarities also in another study based on the WALS data, in that paper focusing on the languages from New Guinea (Comrie and Cysouw forthcoming).

As an interim summary, we may say that in terms of a whole-world comparison, the languages from Africa do not emerge as a typologically distinct subgroup. With respect to the comparison of Africa and Eurasia, things seem to be better, with a clear African subgroup. However, this is at least partially caused by geographical proximity.

TABLE 10.2 *Major clusters from Figure 10.2, characterized by continent and basic word order.*

Clusters (going clockwise)	Continent	Word order	Exceptions
from Supyire to Middle Atlas Berber	Africa	Verb–Object	Supyire, Koyraboro Senni (Object–Verb)
from Harar Oromo to Beja	Africa	Object–Verb	–
from Hindi to Persian	Eurasia	Verb–Object	Hindi, Persian, Eastern Armenian (Object–Verb)
from Turkish to Mundari	Eurasia (+ Khoekhoe)	Object–Verb	–

### 10.3 Relations among African languages

We now turn more specifically to internal relations among the languages of Africa. We proceed as follows. First we consider large-scale genealogical groupings of languages, called language families, which represent a considerable time depth. We then turn to lower-level genealogical groupings, namely genera, which reflect a shallower time depth. In each case, we pose the following question: Are members of pairs of languages within the given genealogical grouping more similar to one another than members of pairs of languages across the relevant genealogical boundary? The answer to this question is then tested against geography, to check whether the patterning could be the result of geographical proximity rather than typological similarity.

#### 10.3.1 *Language families*

To investigate typological diversity within and across language families, we work basically with the four language families posited by Greenberg (1963), namely Afro-Asiatic, Nilo-Saharan, Niger-Congo (the more usual current term for Greenberg's Niger-Kordofanian), and Khoisan. We are, of course, aware that not all of Greenberg's classification is considered robust within African linguistics. In particular, serious doubts have been voiced regarding Nilo-Saharan and, especially, Khoisan. In the case of Niger-Congo, the core of the family is reasonably robust, with discussion centering on the membership of more peripheral branches like Mande. The Kadugli group is considered to be unclassified here.

In Figure 10.4, a NeighborNet of the typological distances between 24 African languages is shown. These 24 languages are the African languages included in the previously used 56-language sample, being thus all from different genera. The Greenbergian families to which these languages belong are indicated in brackets (AA for Afro-Asiatic, NC for Niger-Congo, NS for Nilo-Saharan, Kh for Khoisan, and Ka for Kadugli). As the network in Figure 10.4 shows, no clear genealogical grouping at the level of Greenbergian families emerges from the WALs data. It should be emphasized that this is not in itself a criticism of the Greenbergian families. Indeed, as Greenberg (1963) himself noted, typological similarities are not a reliable basis for establishing genealogical classifications of languages.



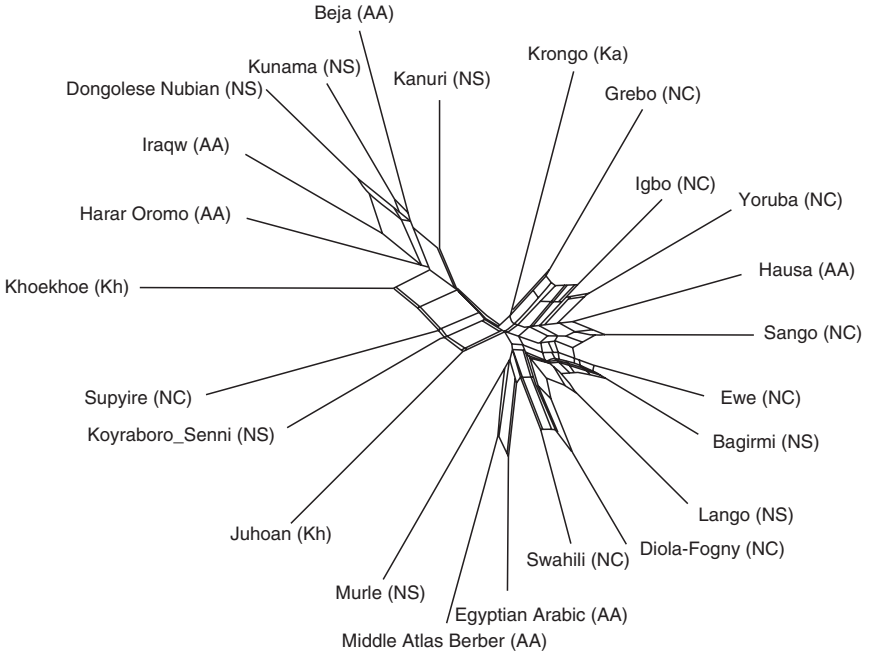


FIG. 10.4 NeighborNet of the 24 languages from Africa in the sample.

Actually, there is a (small) signal of the Greenbergian families to be found in the WALS data. Figure 10.5 shows the significant correlation between geographical distance and typological distance for the 24 African languages (Pearson's  $r = .33$ , Mantel Test  $p < .0001$ ). The two lines in the figure are the regression lines for the pairs of languages that are not related (upper line) and the pairs of languages from the same Greenbergian family (lower line). As can be seen, there is a slight tendency for the languages from the same family to be typologically more similar (viz. the line is lower) and to be less dependent on geographical distance (viz. the line is less steep). However, the differences are very small (regression of upper line:  $typ = .48 + 1.9 \cdot 10^{-5} (geo)$  vs. lower line  $typ = .46 + 7.8 \cdot 10^{-6} (geo)$ ).

### 10.3.2 Language genera

For the investigation of the impact of lower-level genealogical groupings on typological distances among African languages we constructed a sample including multiple languages from the same genus, while still keeping

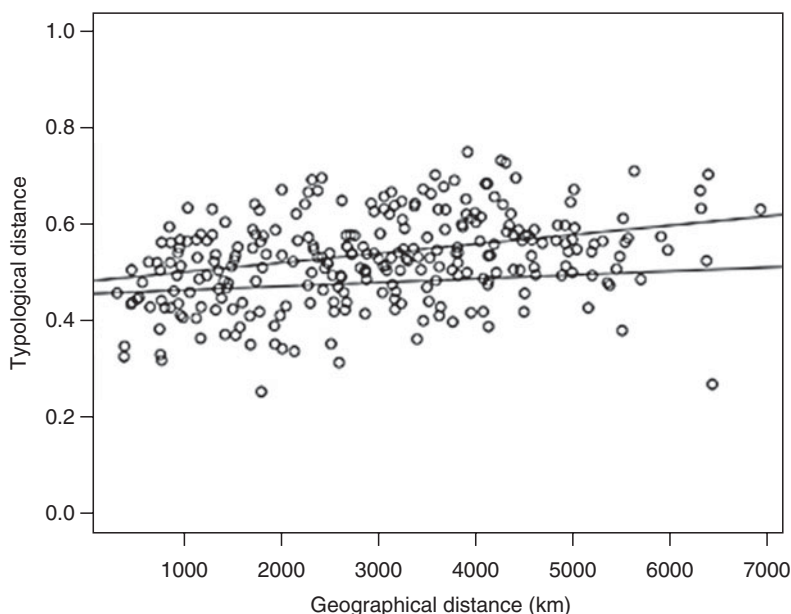


FIG. 10.5 Correlation between geographical distance and typological distance for all pairs of languages from the 24 African languages. Regression lines for pairs from different families (upper line) and for pairs from the same families (lower line) are added.

to a decent amount of available data for all the languages in the sample. Given the data as provided by WALS, it turns out to be impossible to meet both these desiderata. In the end we decided to include more languages (partly with less available data) to be able to investigate within-genera against between-genera diversity. The chosen sample of 77 African languages includes languages from 37 different genera, of which 17 genera are represented by more than one language. To reach such a coverage, the lower boundary for data availability per language had to be set as low as 30% of the available features in WALS.

Figure 10.6 again plots typological distance against geographical distance for all pairs of languages from the 77-language sample. The regression line for languages from different genera (upper line) is now clearly distinct from the regression line for languages from the same genus (lower line). The precise values are: regression of the upper line:  $typ = .47 + 1.8 \cdot 10^{-5} (geo)$ ,

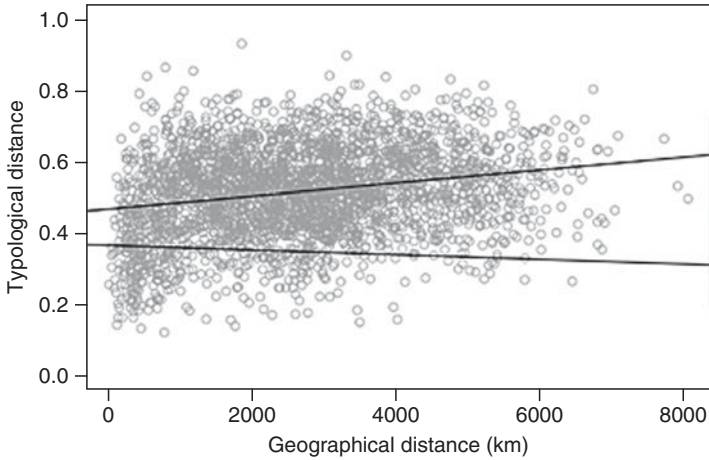


FIG. 10.6 Correlation between geographical distance and typological distance for all pairs of languages from the 77-language sample from Africa. Regression lines for pairs from different genera (upper line) and for pairs from the same genus (lower line) are added.

and regression of the lower line:  $typ = .37 - 6.6 \cdot 10^{-6} (geo)$ . Typological similarity for a given geographical distance is more likely for languages within the same genera than for languages from different genera, i.e. here we do have a clear signal in the geographical distribution of typological diversity corresponding to the time depth at which genera were formed.

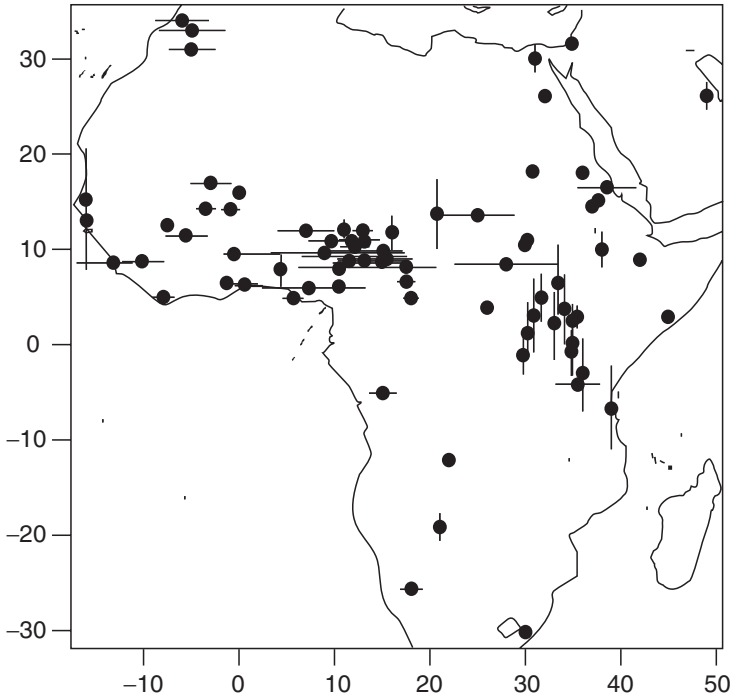
It is important not to overlook one surprising feature, represented by the cline of the lower regression line, namely that for languages within the same genus, typological similarity actually seems to increase with greater geographical distance. On closer inspection, this appears to be caused by the relatively recent expansion of the Bantu languages. The few genera that are more widespread geographically, such as Semitic and Bantu, are the result of recent expansions, which accounts for the large geographical distances relative to rather low typological distance. If one takes two African languages that belong to the same genus but are spoken very far apart, they are almost certain to be Bantu languages, and Bantu languages are typologically very similar to one another. In other words, the clearest signal we may be receiving here is of the Bantu expansion. When removing the Bantu genus from the regression, it becomes  $typ = .37 + 9.3 \cdot 10^{-6} (geo)$ , now with a positive cline.

## 10.4 Geographical distribution of typological variety

Finally, we turn to the question: How is typological variety distributed geographically in Africa? We answer this question by considering, for each language in the 77-language sample, the relation between the distribution of the 10 geographically closest languages and the 10 typologically most similar languages (the number 10 is chosen to be large enough to prevent influence from random factors, but small enough to lead to differentiation in a 77-language sample). If typological and geographical distance were correlated exactly, then these two sets of 10 languages would coincide. However, normally this is not the case. We then ask whether there is a relative preference for the typologically similar languages in either of the two hemispheric directions, east–west or north–south.

For example, the 10 *geographically closest* languages to Swahili have a maximal east–west dispersion of 1,685 km and a maximal north–south dispersion of 904 km. This is compared to the 10 *typologically most similar* languages, which have a maximal east–west dispersion of 2,222 km and a maximal north–south dispersion of 1,976 km. These figures lead to the conclusion that the typologically most similar languages have a relatively larger spread in the north–south direction, because  $1,976/904 = 2.2$  is larger than  $2,222/1,685 = 1.3$ . In other words, Swahili shows a north–south preference for typological similarity, presumably reflecting greater language contact to the north and south, leading to more spread of linguistic features along that axis.

Map 10.3 extends this to all languages in the sample, showing that some languages have more relative typological similarity in the east–west direction (represented by horizontal lines), others more relative typological similarity in the north–south direction (represented by vertical lines). The length of the lines is representative for the strength of the preference in either direction (ultimately leading to a point when there is no preference for either direction). Interestingly, there are parts of Africa, such as west-central Africa, that are predominantly “horizontal,” and other parts, like east Africa, that are predominantly “vertical.” We propose the following tentative explanation for this distribution (following Güldemann 2008 and forthcoming b). The horizontal preference may reflect the greater ease of population movements and contacts on an east–west axis given that climatic and related changes are most significant on the



MAP 10.3 Hemispheric preference for typological similarity for the 77-language sample of African languages.

north–south axis, i.e. migration and contact to the east or west minimizes the need to adapt to new climates, as argued, for example, by Diamond (1998). But under particular circumstances, movement and contact in a north–south direction may be favored, for instance along an east or west coast (cf. the west coast of Africa in Map 10.3), or in an area where valleys running north–south are separated by mountain ranges impeding east–west movement, as with the Rift Valley in East Africa.

### 10.5 Conclusions

Our investigations show that, based on the typological data from WALs, there is no genealogical signal in the African languages as a whole, nor in the typological variety of the Greenbergian language families. Further, typological similarity is correlated with geographical proximity, which

indicates that typological similarity is (possibly strongly) influenced by borrowing (i.e. horizontal transfer). At the level of genera, things are somewhat better for signals from vertical transfer, though still not perfect. In particular, signals of particular recent events, like the Bantu expansion, seem to play a major role in the distribution of typological similarity. Further, we have argued that linguistic similarity, whatever its cause, does show clear geographical structure within Africa.

Being able to identify how typology, geography, and history interact, including being able to identify our limitations in evaluating this interaction, provides an important supplementary insight into prehistoric demographic processes. Our investigation also serves as a warning: Application of statistical methods to data based on typological properties of language may well reflect relatively recent historical events; but it seems rather that such features cannot be used to reconstruct more distant events, such as at the level of large-scale language families. We hope, nonetheless, that further refinement of these methods will lead to finer discrimination of different chronological layers.

# 11 What click languages can and can't tell us about language origins

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BONNY SANDS AND TOM GÜLDEMANN

## 11.1 Introduction: click languages of Africa

Click languages of Africa are often portrayed as ancient languages, with clicks being viewed as probable relics of an ancestral mother tongue. This view espouses a monogenetic origin of clicks, and assumes a linguistic conservatism particular to the languages referred to as “Khoisan” (or “Khoesan”). In this chapter, we challenge these views, presenting synchronic and diachronic linguistic evidence describing the linguistic evolution of clicks and click languages in the relatively recent human past.

Historical linguistic research over the last decade has led specialists to the consensus that the single family origin of the so-called “Khoisan” languages cannot be maintained on any currently available evidence. Using the comparative method, the languages have been grouped into five phyla: Tuu, Ju-≠Hoan, Khoe-Kwadi, Sandawe, and Hadza.

There is no evidence that clicks are more conservative than other speech sounds. We demonstrate that they take part in regular, patterned sound changes and in contact-induced transmission, as do other speech sounds. We discuss evidence for the loss of clicks in certain click languages, as well as evidence showing that at least one Tuu language, ||Xegwi, has borrowed clicks from Nguni Bantu. The presence of clicks in the languages traditionally subsumed under “Khoisan” is better viewed as an areal phenomenon rather than as a direct inheritance from the earliest human language.

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In this chapter, we argue that the widespread use of clicks in paralinguistic utterances (e.g. *tsk-tsk*, *tut-tut*) is not evidence of their linguistic antiquity, but is an epiphenomenon of the fact that clicks are cross-linguistically rare and acoustically salient. This makes them ideal candidates for use in ideophones, special registers, and expressive and directive speech acts. Because they stand out in the speech stream and are part of a shared human vocal toolkit, their use may have multiple origins, some of which may be quite recent. In our view, there is no compelling reason why these languages should be expected to tell us more about an ancestral language than do other linguistic lineages, whether inside or outside Africa. Click languages help us recall that variation and innovation, as well as linguistic ideologies and language contact patterns, are likely to have shaped the origin of language as they continue to shape languages today.

## 11.2 Why are clicks often thought to be ancient relics?

The Out of Africa model of human origins suggests that Africa is the most likely site of the origin of language. This model has led some to infer that certain of the modern languages of Africa might retain some traces of the earliest human language. Clicks would seem to be an ideal candidate for such a linguistic fossil because clicks as regular (i.e. phonemic) consonants are currently found only in African languages. In this paper, we challenge the assumptions that underlie the idea that “clicks were present in the mother tongue” (Alec Knight, cited in Wade 2003). We argue instead that the synchronic and diachronic patterning of clicks suggests clicks are no more likely to be relics from early human speech than are any other consonants.

Clicks were once thought to occur only in Africa, and only in the so-called “Khoisan” language family and in a few Bantu languages which had borrowed them. This misapprehension is in part due to the influence Greenberg’s (1963) classification has held on African comparative linguistics for a generation. Recent classifications, based on the Comparative Method, the standard among historical linguists, show that “Khoisan” must be regarded, so far as evidence to date suggests, as five phyla: Tuu, Ju-#Hoan, Khoe-Kwadi, Sandawe, and Hadza. Attempts to link these into larger groupings have not met with success, though future research may show that these languages form as few as three independent lineages. We now know that clicks also existed as regular consonants, in a number of



Bantu languages, in Damin, an extinct speech register of Lardil (Mornington Island, Australia), and in the Cushitic language Dahalo. Clicks are found in many languages of the world as paralinguistic utterances (e.g. tsk-tsk, tut-tut), where they cannot be said to pattern phonologically as regular consonants.

The idea that clicks are an old type of sound has been suggested many times in the past (e.g. van Ginneken 1939; Chatterji 1939; Stopa 1972). Perhaps the staunchest supporter of this position, Roman Stopa, further believed that physical and cultural traits of southern African San were also relics:

In our languages of nowadays incomplete clicks play some marginal role similar to interjections; they are “survivals” of olden periods of language evolution, in the languages of Bushmen, who themselves as to their physique and culture represent some remnants of the upper Paleolithic. (Stopa 1972: 12)

Stopa went against contemporary linguistic opinion in viewing some click languages as primitive (Stopa 1972: 13–15), but his views were in accord with colonial-era accounts in which clicks were described as “primitive” sounds, with great similarity to animal noises (cf. discussions in Lindfors 1983; Breckwoldt 1978). Stopa even made a comparison of words from various click languages with sounds made by chimpanzees (Stopa 1972: 34–35, 50–57). Linguists on the whole have rejected these arguments (Catford 1997; Pesot 1983); most would agree with Ian Catford’s position:

It seems clear, then, that there is no reason at all to assume that clicks and other “exotic” sounds—particularly lateral obstruents, implosives, ejectives—must necessarily be relics of a very ancient stage in the development of human speech, or that clicks themselves are the primordial speech sounds, from which all the rest have been derived. As we have seen, none of the supposed evidence for the primordial status of clicks, presented or implied by van Ginneken, Stopa and others is at all convincing. (Catford 1997: 64)

The idea of clicks as relics has, however, resurfaced in an article recently published in “Current Biology” (Knight et al. 2003). The authors discuss phylogenetically relevant genetic<sup>1</sup> data (regarding both Y chromosome and mtDNA) of two African populations speaking languages with clicks, namely Hadza in eastern Africa, and Ju|’hoan in southern Africa (sometimes referred to in the paper by the generic term “San”), and conclude

<sup>1</sup> The term “genetic” will be reserved here for biology. As far as family relationships among languages are concerned, the term “genealogical” will be used.

that the two groups are genetically maximally distinct with respect to human genetic populations. Following from this, they also propose a second major hypothesis: “The deep genetic divergence among click-speaking peoples of Africa and mounting linguistic evidence suggest that click consonants date to early in the history of modern humans” (Knight et al. 2003: 464).

In more concrete terms, they conjecture that clicks “may be more than 40,000 years old” (p. 470). Under the usual (though not uncontroversial) assumption that all modern languages descend from a single common ancestor, the simplified hypothesis that clicks were a feature of the ancestral mother tongue has been expressed especially in popular science (see, e.g., *New York Times* of 20 March 2003, *Die Zeit* of 27 March 2003, *Academic Press—Daily inSCIght* of 22 October 2003).

We do not dispute Knight et al.’s analysis of the genetic evidence but neither do we uncritically accept it. Their evidence regarding Hadza and Ju|’hoan is not unproblematic (Güldemann and Stoneking 2008), and the general scenario for the origin and proliferation of clicks in Africa in genetic terms must be far more complex than outlined by the authors, because the Ju|’hoan population is biologically not representative of all southern African click-speaking groups (Chen et al. 2000). Regardless of the validity of their genetic arguments, we hold that a linguistic hypothesis must be proven and tested with linguistic data and not with biological genetic data. Knight et al.’s paper contains a number of misinterpretations and misrepresentations of the available linguistic data. From a linguistic perspective, they claim that the only explanation for the presence of clicks in Hadza and Ju|’hoan is inheritance from an early common ancestor language, and they explicitly exclude other explanations for the presence of clicks in the two groups. A linguistically better-informed analysis yields several types of evidence contradicting their view. In particular, independent innovation and contact-induced transmission of clicks are more important than assumed. Evidence suggests there is in fact a real possibility that the emergence of phonemic clicks in Africa represents a far later episode in the diversification of human speech.

The genetic evidence put forth to support the hypothesis of ancient clicks has not been compelling (Traunmüller 2003; Güldemann 2003, 2007; Sands 2004). However, that is not to say that lack of proof implies that clicks *cannot* be archaic consonants. Some linguistic arguments have been put forth to suggest a great time depth of clicks in human language.

Phonetic arguments for the evolutionary advantages of clicks as early consonants have been presented by Miller-Ockhuizen (2004), but these are counterbalanced if not outweighed by the evolutionary disadvantages which the same author also presents. Clicks are found in paralinguistic forms worldwide, but with decreased functional load with increased distance from Africa (Gil 2005). This distribution might suggest an origin of clicks contemporaneous with human expansion out of Africa, but we agree with Gil (2005: 573) that this evidence does not constitute proof as to when clicks may have originated. As will be discussed, the paralinguistic use of clicks may instead be seen as evidence for recent, independent origins of clicks.

### 11.3 The “Cradle of Language” over time

Implicit in many arguments for an ancient origin of clicks is the supposition that languages spoken at or near the source of the origin of language will likely display more linguistic archaisms and greater diversity than languages spoken further away from that source location. Migration theory (cf. Dyen 1956), as this supposition may be called, has been used at time depths of a few hundred to a few thousand years. There is no evidence, however, that migration theory holds at great time depths. In fact, there is some evidence that it does not, as there seems to be an inverse correlation between linguistic diversity on a given continent and the length of time that a continent has been settled by modern humans (Nettle 1999a: 127, 1999c). Thus, we have no principled reason to assume that a language spoken a great distance away from Africa will be more conservative than one spoken in Africa. Given the observable pace of linguistic change in recent times, we agree with Vennemann (1992) that “Any property of a primordial parent language, if it ever existed, idiosyncratic or ordinary, would have been wiped out tens of thousands of years ago in the constant re-creation and re-shaping of all descended languages by countless generations.”

There is no reason to think that the linguistic ecology of Africa today strongly resembles that of 40,000 or more years ago. Many language families from that time have likely gone extinct, or are represented by any of the dozen or so isolates on the continent. The rareness of clicks worldwide may be partly due to historical accident. We have sampled only

a small fraction of all languages which have existed, and lineages may have gone extinct at a rate as high as 80–99% (Pagel 2000; Robb 1991).

We suspect the fascination in the popular media with the idea of clicks as ancient relics has less to do with the distribution of clicks in Africa and worldwide than with the fact that many of Africa's click languages are spoken by foragers. An implicit iconic link is made between a hunting-gathering economy which looks "ancient" and slow to change and a speech sound which is then believed to be "ancient" and slow to change. This link rests on several faulty assumptions. First, it fails to recognize that "present-day foragers are the end result of historical processes rather than their starting point" (Kusimba 2003). It treats foragers as if they have lived lives less touched by time than have other peoples. This is a general problem in studies of foragers in the western intellectual tradition (cf. Klieman 2003). Second, it assumes that linguistic change correlates with economic change; that foragers would preserve more archaisms in their speech than would pastoralists or farmers. There is no evidence that hunter-gatherers or Africans are any more linguistically conservative than other peoples (Catford 1997; Traunmüller 2003). Though speaking about foragers in central Africa, Klieman raises a caution that is relevant for this discussion involving the foragers of southern and eastern Africa:

Scholars begin to "mistake the model for the thing modeled" and African hunter-gatherers are deemed primordial rather than recognized for the role that they played in producing Western definitions of primordiality itself. (Klieman 2003: 17)

Knight et al. suggest there may be a functional reason why clicks were retained by hunter-gatherers. When speech is whispered, they argue, "click density of Ju'hoan allows devoiced communication," and so "click systems may impact hunting success" (Knight et al. 2003: 464). The phenomenon they refer to with "hushed whisper-like communication" is called in Ju'hoan *gòngòma* or *gùmágùmá* (cf. Dickens 1994: 179). It is not only used in hunting, but represents a fairly ordinary aspect of human languages in general. The view that whispered clicks are somehow special ignores the fact that some Ju'hoan clicks are voiced and are thus not faithfully transmitted in a whisper. It also ignores the fact that clicks are no different from other consonants in their ability to have burst and frequency information imparted during a whisper. Ju'hoan click bursts themselves tend to be somewhat muted during whispering (Amanda Miller, personal communication), which makes sense, since, in our

experience, clicks are readily perceptible to animals as being human-made noises. In contrast with the suggestion by Knight et al. (2003) that clicks conferred an advantage in hunting, we note that clicks are often employed in paralinguistic forms used for animal communication (see section 11.5). If anything, the high-amplitude bursts of clicks make them more perceptible to mammals than other consonants.

That there is no connection between clicks and hunter success is corroborated by published information on the context and manner of use of *gòngòma* ~ *gùmágùmá* in Ju|'hoan and whispering in other click languages during hunting:

When a few hunters work together, they communicate with hand signals. When they cannot see one another, they may use bird calls and whistling. Once the animal has been sighted, they may come together and discuss their strategy in soft whispers. (Liebenberg 1990: 108, see also p. 55)

Available evidence suggests the Ju|'hoan speakers are typical in their avoidance of speaking while hunting. N|uu hunters avoided talking while hunting, using hand signals for communication when necessary (Andries Olyn, personal communication), and Hadza hunters whistle signals to each other when speaking at a distance (Gudo Mahiya, personal communication). Furthermore, none of the Hadza hunting names have clicks in them, where they might be expected to be found (Kirk Miller, personal communication). The prioritization of the role of hunting in language evolution seems to recall a “Man the Hunter” paradigm of human evolution, in which the role of women forager-scavengers is considered secondary.

#### 11.4 Origins of clicks

How does a language or population come to possess a certain linguistic feature? There are three basic types of explanation: (a) retention from an ancestor language, (b) independent innovation, and (c) contact. Knight et al.'s (2003) general linguistic hypotheses rest on the assumption that clicks in modern languages are by default inherited, *inter alia* in Hadza and Ju|'hoan, only in a few cases being due to language contact with “genuine” click languages. Hence they can conclude that the origin of clicks goes back to a single historical process; that is, their monogenesis in

“proto-world” or a similarly ancient language. The downplaying of explanations for clicks other than inheritance is not at all supported by the entire range of cross-linguistic data on clicks in human languages.

11.4.1 *Retention of clicks from an ancestor language*

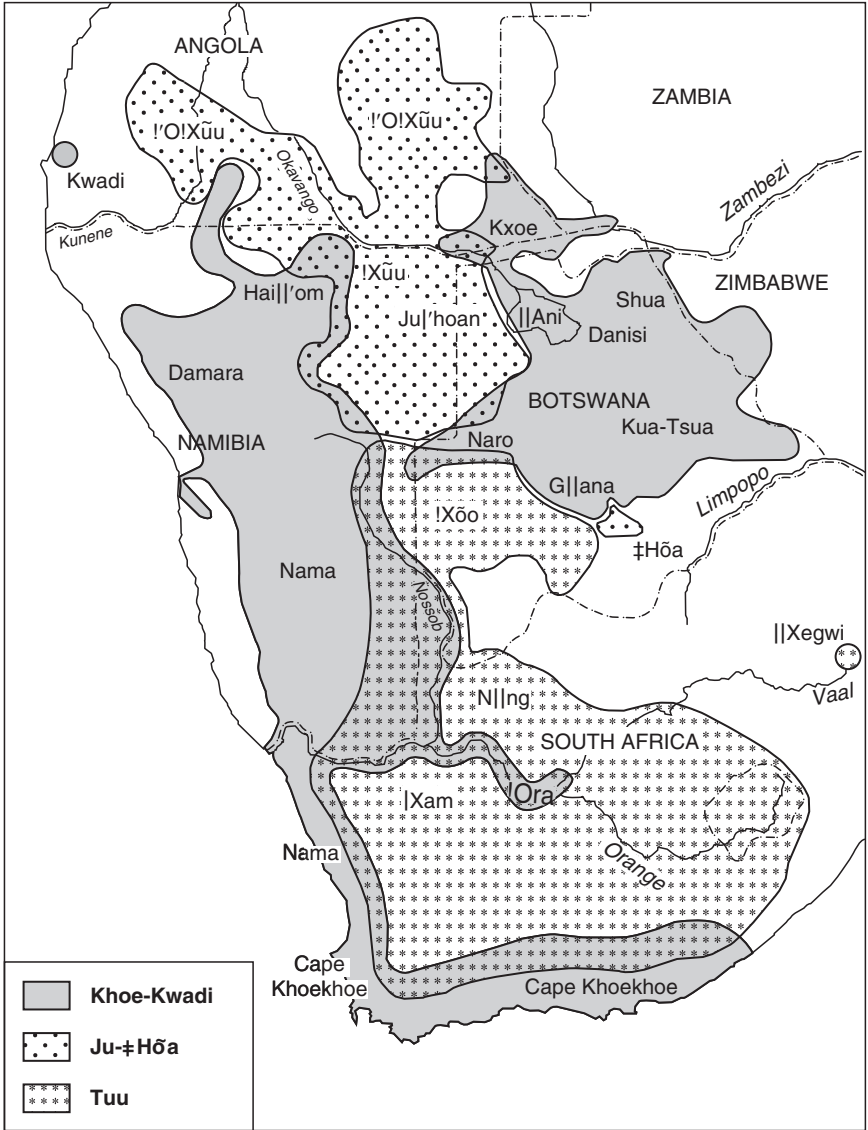
In Table 11.1, we list the eight distinct language families (some represented by just a single language) which are known to have click phonemes (for further details, cf. Güldemann 2007). As discussed below, three of these groups (Dahalo, Bantu, Damin) are thought to have received clicks through some means other than direct inheritance or transmission. The remaining groups were all considered by Greenberg (1963) to be “Khoisan” languages.

“Khoisan” languages, shown in Map 11.1, are characterized by their use of phonemic clicks. Greenberg distinguished “Southern African Khoisan” and “Eastern African Khoisan” languages. The former group was comprised of Ju (“Northern Khoisan”), Tuu (“Southern Khoisan”), Khoe (“Central Khoisan”). The latter group was comprised of two individual

TABLE 11.1 *Attested languages/language groups with click phonemes.*

Language or LANGUAGE FAMILY	Area	Highest linguistic affiliation
1 all JU-+HOAN (includes “Northern Khoisan”)	southern Africa	isolate family
2 all TUU (= “Southern Khoisan”)	southern Africa	isolate family
3 all KHOE-KWADI (includes “Central Khoisan”)	southern Africa	isolate family
4 <i>Sandawe</i>	eastern Africa	isolate language (possibly related to 3)
5 <i>Hadza</i>	eastern Africa	isolate language
6 <i>Dahalo</i> (CUSHITIC)	eastern Africa	Afroasiatic
7 some BANTU (groups K30–50, R40, S30, S40)*	southern Africa	Niger-Congo
8 <i>Damin</i> (speech register of <i>Lardil</i> , TANGKIC)	northern Australia	Australian

\* in K30: Kavango group; K40 = *Mbukushu*; K50 = *Mbalan’we, Fwe*; R40 *Yei*; in S30: *Southern Sotho*; S40 *Nguni* group (Letters and numbers refer to zones and groups in Guthrie’s (1967 1971) referential classification).



MAP 11.1 Khoisan languages.

languages: Sandawe and Hadza. Greenberg's groupings rest on evidence that does not conform to the standards of historical comparison and diachronic typology; they have thus not been accepted by the majority of Khoisan linguists (cf. Westphal 1962; Güldemann and Vossen 2000; Güldemann forthcoming a). Knight et al. (2003) assume Greenberg's classification, while admitting that it is controversial.

Clicks pattern differently in different languages with phonemic clicks. Those called "Southern African Khoisan" (which we call "core click languages") share certain typological phonological characteristics. They tend to have intermediate (10–20) or large (30+) numbers of contrastive click consonants, and high (40–50%) to extremely high (60%+) proportions of roots which contain clicks. Languages not sharing these characteristics, or which allow clicks in root-medial position, may differ because they have borrowed clicks. "Core click languages" may share many similar characteristics as a result of extensive contact and convergence in a Kalahari Basin linguistic area (cf. Güldemann 1998).

#### 11.4.2 *Independent innovation of clicks*

A first defect of Knight et al.'s (2003) approach is to ignore entirely the possibility of independent innovation of click phonemes—a possibility clearly attested in one case, namely in Damin. This is a fully functional speech form used in an Australian Aboriginal group by second-degree male initiates to ritually related community members (McKnight 1999). The normal linguistic register is Lardil—a language of the Tangkic family (Non-Pama-Nyungan, Australian). Lardil and the initiate register Damin are mutually unintelligible, so that the latter can be considered to a certain extent to be a separate language. While Damin is a parasitic speech form on Lardil in terms of grammatical structure, it differs radically in its lexicon and phonology (Hale 1973: 443).

According to N. Evans (personal communication), there is linguistic evidence for the innovative status of Damin-typical sounds, including clicks: Some of its words can be shown to have been created by replacing normal consonants of inherited words by new marked speech sounds, for example, Damin *mlii* (*m!* stands for a nasal labial click [m⊙]) <proto-Tangkic \**mi(y)i* "vegetable food" and Damin *k'uu* <proto-Tangkic \**kuu* "eye". There is no evidence to suggest that the clicks in Damin are related to those in Africa.



### 11.4.3 *Contact proliferation of clicks*

Some of the Bantu languages of southern Africa and one of the Cushitic languages of eastern Africa have click phonemes even though the majority of languages related to them do not. Click sounds cannot be reconstructed to either proto-Bantu or proto-Cushitic and so inheritance cannot be the source of the sounds.

In the case of the Bantu languages with clicks, many words with clicks are identifiable loans from languages from the Khoe and Tuu families (Louw 1977). Caprivian/Kavango languages with clicks such as Yeyi, Mbukushu, Gciriku, Mbalan'we and Kwangali seem to have borrowed words with clicks as a consequence of close economic interaction with speakers of languages of the West Kalahari branch of the Khoe family such as Kxoe and ||Ani (cf. Fisch 1984: 167; Legère 1998; Baumbach 1997). The borrowing of clicks in Southern Sotho and Nguni (Zulu, Xhosa, Swati, Ndebele) appears to have been facilitated by the existence of respect registers in those languages (Herbert 1990a, b), which themselves may have been borrowed from Khoe languages. *Isihlonipho sabafazi* (= "wives' avoidance language"), as this register is called in Nguni, primarily concerns the behavior of married women towards their male in-laws (see, inter alia, Kunene 1958; Finlayson 1982; Herbert 1990a). Its linguistic reflex in its most extreme form is a taboo on uttering the names of senior male in-laws (focusing on the father-in-law) and any of the syllables of which these names are composed. To achieve this goal, an original consonant may be substituted by another consonant, for example, a click, or a separate word (perhaps a loan word) may be used. Herbert (1990b), following Faye (1923–5), argues that the contact of Bantu speakers with click languages provided them with a welcome addition to the available inventory of segments for the Hlonipha register, segments which also entered the normal language.

In Xhosa, clicks (spelled with letters c, q, and x) seem to have been borrowed in a wide range of vocabulary items, e.g. *umqala* "throat," *igxalaba* "shoulderblade," *ingxabela* "axe," *ingxelo* "statement," *ukuqesha* "hire," *ukucanda* "cleave, split," *gqabhu* "bursting" (Louw 1977). Caprivian/Kavango languages have clicks in a smaller percentage of the overall vocabulary than Xhosa, but clicks occur in a wide range of lexical items, e.g. Kwangali: [n|ugu] "ankle," [ε-|i] "otter, big," [n|wanta] "to pinch," [l|apama] "sleep with the whole body," [mu-n|a] "tall" (Legère 1998).

In the case of Dahalo, the Cushitic language with phonemic clicks, a different type of contact situation may have happened. There is some slight evidence that Dahalo speakers may once have spoken a language similar to the east African click language Sandawe (Ten Raa 1969) and then shifted to a Cushitic language. This could help explain why clicks occur even in such basic vocabulary items in Dahalo as “breast,” “saliva,” and “forest” despite occurring in only about 40 known lexical items (Maddieson et al. 1993).

There are many Bantu languages in contact with click languages which have not borrowed clicks as regular consonants, e.g. Tswana, Kgalagadi. Depending on the dynamics of the contact situation, clicks may even be lost in a potential donor language if they are emblemized to carry negative meanings (cf. Wilmsen and Vossen 1990). It is also possible that some languages which once borrowed clicks have since lost them (Nurse 1968). To sum up, language contact may have occurred with or without clicks being borrowed and language shift may have occurred with or without clicks being retained.

#### 11.4.4 *(In)Stability of clicks*

Knight et al. (2003) also fail to consider that clicks are an areal feature. This is not just to say that clicks can be borrowed across languages in contact. It has the further implication that clicks tend to be maintained because they remain readily available in neighboring phonologies for reborrowing. They also may be less prone to loss because they are less stigmatized than they would be in an otherwise click-free area.

It is well known among Khoisan linguists that Khoisan languages can lose some of their clicks (Traill and Vossen 1997). The acoustically abrupt clicks (!, †) seem especially prone to partial or total loss in the phoneme inventories of several Khoe languages of eastern Botswana which have been in intense contact with Tswana (Wilmsen and Vossen 1990). Recent data collected on N|uu (Sands et al. 2006), a Tuu language of the !Ui subgroup, has allowed for a more fleshed-out picture of click loss in this subgroup than was seen in Traill and Vossen's comparison. ||Xegwi is an extinct !Ui language with relatively sparse documentation (Lanham and Hallows 1956a, b). In identifiable cognates which contain one of the two abrupt clicks (!, †) in N|uu or |Xam, ||Xegwi has a non-click consonant. Examples of words in which ||Xegwi might be expected to have an inherited (post-)alveolar click [!] are shown in Table 11.2. The most likely explanation for this pattern is that ||Xegwi lost inherited abrupt clicks.

TABLE 11.2 *Loss of ancestral ! clicks in ||Xegwi.*

Xegwi	N uu
k'uu, c'uu “be two”	!'uu “two”
kaŋ “road”	!an (Western N uu), !aŋ (Eastern N uu) “road”
gaʔa, ʃaʔa “sky”	!aaʃ “sky, heavens”
cçeu “land” / k'eu <sup>n</sup> “stone, rock”	!'au <sup>n</sup> “ground, soil”
kwi, gwi “person”	ŋ  ŋ !ui “person”

TABLE 11.3 *Bantu sources of ! clicks in ||Xegwi.*

Xegwi	Similar Nguni/Sotho* Words (< Bourquin 1951)
!wàà “one, only, alone”	qha “only” (Zulu/Xhosa)
!a “bone”	le-qa “small piece of meat” (Zulu/Xhosa)
!kxwa “head of cattle”	umthuqwa “tawny ox” (Xhosa)
!kxama “cut!”	qhaqha “cut open” (Zulu/Xhosa/Sotho)
!ala “begin”	qala “begin” (Zulu/Xhosa/Sotho)

Note: In Nguni orthographies, q represents the post alveolar click [!]!

The borrowing of click phonemes has been documented among the core click languages (e.g. Traill and Nakagawa 2000). More surprising is the evidence in Table 11.3 that the (post-)alveolar click ! in ||Xegwi (a core click language) appears to have been borrowed from Nguni and/or Sotho Bantu languages which have a single abrupt click, [!]. The last generation of ||Xegwi speakers were in intense contact with sociolinguistically dominant Swati speakers.

Clicks appear to undergo sound change in the same way as do other sounds; there is no evidence to view them as being particularly prone either to loss or to fossilization.

### 11.5 Marginal sources of clicks

The cases of click innovation in Damini and the click proliferation in Nguni Bantu suggest that the innovative “promotion” of clicks from

non-phonemic to phonemic speech sounds can occur, especially given an association with a marked sociolinguistic register. Typologically unusual or highly marked sounds or combinations of sounds are especially good ways to mark expressions so they stand out in the speech stream (e.g. *schm-* words in English borrowed from Yiddish). The labial flap, which like clicks occurs primarily in African languages, occurs in some languages only in a small set of expressive vocabulary not identifiable as loans (Olson and Hajek 2004). As with clicks used phonemically, clicks used paralinguistically may be inherited, borrowed, or innovated. It seems reasonable to suppose that special speech registers (including slang) and ideophonic vocabulary would be relatively open to creative and innovative uses of language, including uses of underutilized vocal gestures, compared with other speech registers. Clicks may be found widely in paralinguistic uses because they are well suited to the job in the way that they stand out from other vowels and consonants in a stream of speech given their high amplitude.

Clicks are used paralinguistically in many languages in expressive and directive speech acts:

English and various	dental click   used to express dismay or disagreement (“tsk-tsk”, “tut-tut”); bilabial click ☉ to express or simulate affection
Masalit	dental click meaning “yes”; “no!” “sucking” (ideophonic of sucking) (Edgar 1991)
Digo	n akule “miniscule” (Walsh 2006)
Chinese	Nasalized click n! used by mothers while nursing their children (Nathan 2001)
various	tooth sucking (labiodental click) to express disgust (Rickford and Rickford 1999) (compare English use of linguolabial trill or “Bronx cheer”)
English	lateral click    used to urge horses
Digo	n a “buzz off!” (Walsh 2006)

Readers are directed to Gil (2005) for more discussion. The appearance of clicks in paralinguistic forms cannot be taken unequivocally as evidence of their archaism. Given that the ability to make expressive and directive speech acts may have evolved before the ability to make referential/representational ones (cf. D’Andrade 2002) it may be that a consonant that is

restricted to such speech acts would itself tend to be viewed as somewhat more basic as well. Speech acts such as these are often used with animals, and D'Andrade (2002) is of the opinion that animals would, "if taught human languages, still predominantly produce only directives and expressives".

Not infrequently, clicks appear as marginal elements in running speech that is not sociolinguistically marked:

- allophonic variants: French (Marchal 1987), Idoma (Ladefoged and Maddieson 1996: 340) Mangbetu (Demolin 1991), and other languages, cf. Ohala (1995), Simpson (2001), and Engstrand (1997).
- pathological variants: English oral stops (Howard 1993); sonorants (Heselwood 1997); sibilants (Bedore et al. 1994)
- non-normal language acquisition: twin languages (Bakker 1990)

Clicks can occur allophonically due to consonant overlap, though their amplitude in these cases is not typical of clicks used phonemically. In other cases, clicks may be used to replace other speech sounds that are difficult or impossible for a speaker to make due to a speech defect. These marginal sources of clicks have never been known to lead to a wider-scale integration of clicks into a language's phonology. Nevertheless, they remind us that the idea of using clicks linguistically need not be borrowed from a previously existing language.

## 11.6 Conclusion

The present discussion has not falsified the idea that the origin of click phonemes is of the same age as the origin of language, as proposed by Knight et al. (2003) on the basis of genetic data. However, against the claims of these authors, the available linguistic data do not single out this hypothesis in favor of other hypotheses. The idea that modern click phonemes have their ultimate origin in the linguistic feature of a very ancient human language remains just one among several speculative hypotheses; it still seems to be inspired by the outdated default assumption that linguistic, genetic, and cultural features correlate. The possibility is very real that the emergence of clicks as phonemes in Africa represents a far later episode in the diversification of human speech, or that clicks may have developed multiple times throughout human history.

# 12 Social origins: sharing, exchange, kinship

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ALAN BARNARD

## 12.1 Introduction

This chapter considers three social aspects of the emergence of language: sharing, exchange, and kinship. I shall argue that the earliest full language coincided with the development of other forms of communication, including human kinship—with sharing practices and rules, marital alliance, kin classification, and ultimately kin category extension to the limits of social systems. Virtually all human hunter-gatherer societies retain completely universal systems of kin category extension (where in relation to any given individual, everyone is classified as some kind of “kin”), and the classification of kin among contemporary hunter-gatherers can provide clues to early human social structures. In both language and society, dispositions became rules. Social systems, like languages, evolved into extremely complex yet enabling structures.

## 12.2 Bickerton’s model of language evolution

My main goal is to suggest ways in which ethnographic comparison and classic anthropological theory might feed into understanding the social basis of the emergence of language. In the first part of the paper, I shall draw on the work of Robin Dunbar and of Derek Bickerton and William Calvin. Dunbar is the most prominent proponent of a social origin of language, and his view is known as the “social gossip” or “social bonding” hypothesis (Dunbar 1993). Language evolved in order to enable speakers

Thanks are due to Nick Allen and Robin Dunbar for helpful comments on earlier drafts.

to maintain social relationships. Other primates may have calls or some other form of primitive protolanguage, but they cannot, of course, exchange information about their communities at a distance. The other two main social views are Terrence Deacon's (1997) "social contract" hypothesis, and Geoff Miller's (1999) "Scheherazade effect" hypothesis. The social contract hypothesis states simply that language emerged in order to allow contracts; these contracts were formed among men to prevent the theft of their partners. The Scheherazade effect hypothesis states that the original purpose of language was to attract mates or keep them entertained. As Dunbar (2003a: 221) has suggested, all three of these hypotheses can easily be unified, in that all three are associated with increasing group size and group solidarity. Social gossip, in his view, comes first, ultimately out of social grooming among primates. This seems plausible to me, but I would add that all three hypotheses imply the development of notions of (human) kinship, and that Miller's and Deacon's also suggest some sort of exchange notion and indeed alliance theory (Lévi-Strauss 1949, 1969) in the minds of early linguistic people.

In 1990 Bickerton proposed two phases for the emergence of language. The first was the development of protolanguage with *Homo habilis* (followed by a protolanguage-using *Homo erectus* Out of Africa migration). The second was the genesis of full language with *Homo sapiens sapiens* (and the *H. sapiens sapiens* Out of Africa migration). According to Bickerton, like child language and like ape language, a pidgin is a kind of protolanguage, loosely reminiscent of the linguistic system of *Homo habilis*. Creoles, he argues, can emerge from pidgins within a generation. Producing them for the first time as true languages creates rules of grammar which in every way are reminiscent of those of any other true language (see, e.g., Bickerton 1990: 164–197, 1995: 110–111). Many of Bickerton's ideas on pidgins and creoles are controversial, notably his rejection of the gradual development of creoles. His point here though is that the creation of creoles, perhaps within a generation, shows us that there is no in-between, no "interlanguage" (1990: 177–181, 1998). A creole, in short, is not half a language. Through the 1990s, Bickerton (e.g. 1998: 354–357) argued for a catastrophic rather than a gradual birth of language proper. Among his five arguments, one is that a gradual, say 2-million-year, evolution should have left some mark in the fossil record. Instead, he suggests, we have a "cognitive explosion" which occurred only

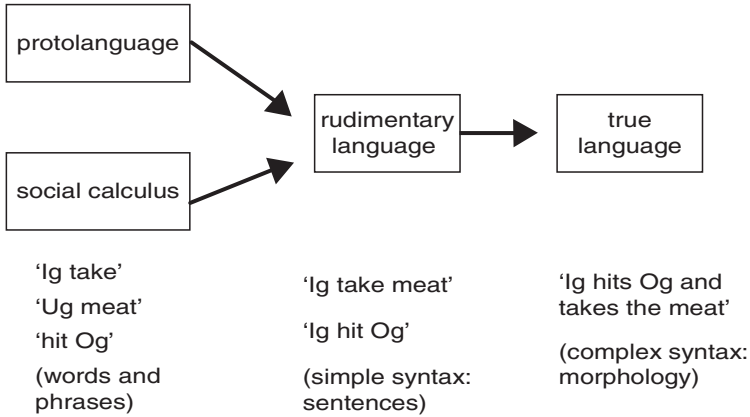


FIG. 12.1 Bickerton's three-stage theory (Calvin and Bickerton 2000, diagram adapted from Botha 2003: 78).

after the emergence of anatomically modern man in southern Africa about 120,000 years ago.

Bickerton has since modified his view, in his dialogical book written with William Calvin in 2000 (Figure 12.1). There is no interlanguage, but Bickerton does now suggest a modification of the two-phase model to allow for something called rudimentary language. This was the linguistic system of “archaic” *Homo sapiens*, before true language. Protolanguage might involve words and simple phrases such as “Ig take,” “Ug meat,” or “hit Ug.” According to Bickerton (Calvin and Bickerton 2000: 129, 136–137; see also Botha 2003: 76–81), when such phrases are coupled with a social calculus of reciprocal altruism, rudimentary language emerges. The social calculus entails knowledge of who grooms whom, who gives meat to whom, who supports whom in fights, and so on. In linguistic terms, it entails tags such as agent and theme. And so we have simple sentences, like “Ig take meat” or “Ig hit Og.” True language entails more fully parsable syntax, and in this later stage we have more complex sentences and rules such as agreement between subject and verb: “Ig hits Og and takes the meat.” What the in-between phase allows for is, in effect, an earlier existence of something we would recognize as language from a social as well as a neurological point of view, although it would lack the complexity of almost any true language we find today.

I am not concerned here with the technical rights and wrongs of their respective arguments on linguistic or neurological points. Rather, I want



to take this model as indicative of the process which surely led from the sort of societies possessed by *Homo habilis*, *Homo ergaster*, and *Homo erectus* to those of *Homo sapiens sapiens*. I propose that the phases suggested in Calvin and Bickerton (2000) correspond respectively to what we might call protokinship, rudimentary kinship, and true kinship.

Let me digress for a moment. The motto of the Republic of South Africa is *!Ke e: /xarra //ke*. It was created at the behest of President Mbeki in the year 2000. It is intended to mean “Diverse people unite,” or more literally “People who are different come together.” But it can also mean “People who are different are *talking* with each other.” *!Ke* is the unmarked form for “people,” not the form recorded as the “vocative.” There is no imperative particle in the motto, and *//ke*, in fact, means “talk together,” as well as “come together.” The motto is in the /Xam language, now extinct but spoken in the nineteenth century and well recorded. The president chose to have the motto put into what he calls “an ancient language of our people.” It rests beneath a coat of arms depicting two rock art figures greeting each other, because (in Mbeki’s words), this image “pays tribute to our land and our continent as the cradle of humanity, as the place where human life first began” (quoted in Barnard 2004: 6, see also 2003). Linguists have recently come to associate the beginning of language with the earliest material remains of human symbolic behavior—especially the two etched pieces of red ochre from Blombos Cave on the Indian Ocean coast. These were discovered in 1999 and 2000 and are dated to 77,000 years ago (Henshilwood et al. 2002). Of course there is no reason to suppose that the language spoken then at Blombos Cave was any closer to nineteenth-century /Xam than to modern French or German. But the image, and the /Xam words, are evocative.

Two questions about all this come to my mind. Why might “people who are different” come together, or talk together? And what would they talk about? Stone and bone tools, very probably, or indeed art or ritual (see, e.g., Watts 1999). But I would suggest something else as well. Lévi-Strauss, speaking at the Man the Hunter conference in 1966 (Lévi-Strauss 1968: 351), speculated that two or three hundred thousand years ago there were probably people with minds like those of Plato and Einstein. But in the absence of philosophical discourse or theoretical physics, what did they think about? What did they talk about? I quote Lévi-Strauss: “they were probably more interested in kinship!”

One area where language would have become essential is indeed in kinship designations. Therefore, an exploration of kinship terminology

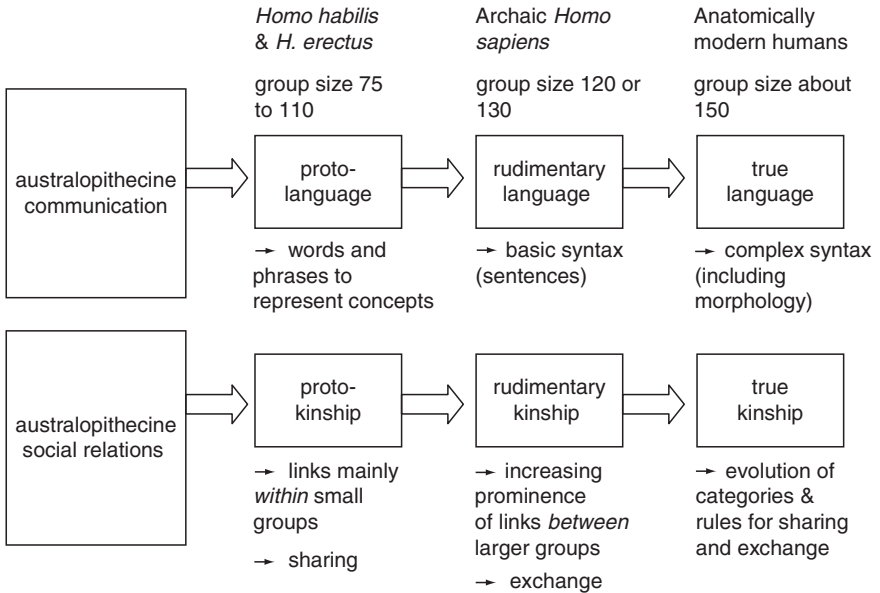


FIG. 12.2 Co-evolutionary relations between language and kinship.

structures, as well as of social institutions related to kinship such as sharing and exchange (including marital alliance), should help us to understand better the social origins of language. Figure 12.2 represents my hypothesis of the co-evolutionary relation between language and kinship. The language part, at the top, is based essentially on Bickerton's proposal, while the kinship part, below, is mine. The latter takes into account a now widely accepted notion of a further relation between neocortex size and group size among primates (Dunbar 1993; Aiello and Dunbar 1993). It has also been incorporated by Calvin and Bickerton (2000) in the suggestion that syntax is created through reciprocal altruism, in the social calculus that emerges with the first expansion of group size among the first representatives of *Homo sapiens*.

### 12.3 Neocortex size, group size, and the evolution of kinship

Chimps spend about 20% of their time grooming, and we humans spent about 20% of our time in social interaction, most of it in conversation (Dunbar 2001: 190–191). As group size increases, the requirement to

transform grooming relationships into linguistic ones also increases, as the effort required for grooming would otherwise be dramatically increased. According to Dunbar (2003b: 173–175), with a group size of 150 it would have to be 43%. His suggested threshold by which some form of language (or protolanguage) must have existed is 30%, in the *Homo erectus* period. Using predicted figures, we have australopithecine group sizes averaging around 65 or 70, *Homo habilis* at around 75 or 80, *Homo erectus* variable, but typically at around 110, “archaic” *Homo sapiens* 120 or 130, and Neanderthals at around 140 or slightly higher. The figure for anatomically modern humans is given in most sources at an average of 148, or 150.

At least half a million years ago our ancestors had developed the anatomical capability of speech (Mithen 1998: 175), and they had already evolved communication skills. Why had they not evolved language? Or had they? According to Dunbar, when groups became too large to make grooming *the basis of society*, rudimentary language took over. It became a selective advantage to develop language, because it allowed information to be shared with larger numbers. At first language was essentially social, and later it became generalized to communicate much more beyond the social. Art, symbolism, religion, mathematics, and so forth would follow.

Mutual grooming is a form of sharing, and it still exists in this context at least among hunter-gatherers—often among close kin and between spouses. Sharing of food also occurs within this sphere. In the Kalahari, hunted meat traditionally came in the form of large animals, and hunting success was (and to some extent still is) spread throughout kin groups through the sharing of meat, with parents-in-law entitled to the best meat a man “owns.” I say “owns” rather than kills, because ownership can be determined by ownership of the killing arrow, and arrows are loaned to others. Meat is shared very widely, while vegetable food is not. Vegetable food is shared normally only within a nuclear family. Of course, modern hunter-gatherers are fully modern, but their subsistence activities allow us a model through which to speculate about the distant past. A shift from mainly gathering to large-scale hunting, coupled with the technology to hunt larger animals, would involve a propensity for meat sharing. This, in turn, might be related to other aspects of communication, and therefore social interaction.

*Homo erectus* was the longest lasting of known hominid species and lived in Africa for at least 500,000 years, before venturing off to Asia around a million years ago (Gamble 1993: 96, 117). It was not only their

ability to produce tools which made this possible, but their abilities in communication, not least to teach tool-making skills to each successive generation. Clive Gamble (1993: 108–112) provides a speculative ethnographic diary of two weeks in the life of a band of undifferentiated ancient hominids, either australopithecine or early *Homo*. He supposes a group size of some 20 to 50, which is quite usual for African hunter-gatherer bands today, although the size of the social unit of identity, what I've called the band cluster (a group of several bands), will be much larger. In Gamble's imagined scene, the range of the group of 20 or 50 consists of seven habitats. In the imaginary habitat in which the group is camped, food is running out. The group moves around that habitat, while younger members, as individuals or small parties, scout each of the other habitats in the range. They find one habitat is best, and the group moves there. He calls this calculated migration.

In Gamble's scenario, what is important is to find the best habitat—not for the males, but for the females. Among more primitive primates, an alpha male might control a group of 15 to 20. Among australopithecines, *Homo habilis* and *Homo erectus*, females were much smaller than males, and according to Gamble they would have been controlled by males in the group. Alpha males would be restricted in their movements, because they needed to keep track of the females. The core group therefore relies on the subadults, teenagers, for information about foraging possibilities. He suggests that males might cooperate in order to defend territory and share access to the females. Hunting, in Gamble's view, though not so among all experts, is less significant than gathering. He argues that the impetus for exploration of foraging habitats is not adaptive curiosity, but “the cooperative alliances, negotiated and contested between core and peripheral members of the social group” (1993: 110). The subadults, presumably male, are peripheral here, and the alpha males and the females are the core. In all this, some kind of communication is essential. But in no sense, yet, is there anything we would recognize as a (human) “kinship system.”

To me, Gamble's description relates to a social situation at best reminiscent of that of proto-kinship. I have long felt that a merger of the ideas of the great rivals Morgan (1871, 1877) and McLennan (1970 [1865]), in the nineteenth century, represents the best pre-Lévi-Straussian model for the origin of human kinship. Gamble's description sounds a bit like a stage at the dawn of Morgan's phase of primitive promiscuity, or of the

cohabitation of brothers and sisters; and if alpha males were to share their females with the exploring and scouting, such stages are possible. The essence of Morgan's early phases is a lack of incest prohibition; and the essence of McLennan's is, in a later stage, exogamy—a term which McLennan invented.

#### 12.4 Society, symbolic culture, and language

There is a growing consensus that in order to understand language, it is necessary to understand the evolution of its social as well as its linguistic aspects (Studdert-Kennedy, Knight, and Hurford 1998). Bickerton's (2002) most recent work actually takes him in a different direction, away from that consensus, towards ecological origins for protolanguage, with the social emerging as significant only after early *Homo*'s acquisition of a few dozen or a few hundred either oral or manual symbols. I think that is a mistake on his part.

Meanwhile, one of the leaders in the social view, Chris Knight (2002: 155; this volume) sees speech, as well as language, emerging from a social revolution rather later—about 130,000 years ago. I part company with Bickerton in his ecological hypothesis, and tend to side with Dunbar in his suggestion that protolanguage was an evolutionary development from grooming. I part company with Knight too, in that I cannot see a single revolutionary origin for everything from language to egalitarian social organization, to art, to religious ritual, to religious belief. Knight's (1991) basic hypothesis is of a female sex strike: synchronous menstruation, demand of meat for sex, and so on, as leading instantaneously to such a revolution.

Knight's theory only makes sense to me as one of several possibilities leading ultimately to religious practice and belief. In a way, it is a feminist version of Freud's theory, the final essay in *Totem und Tabu* (1960 [1913]), though obviously with better evidence from archeology and ethnography (prehistoric art, the use of ochre, initiation ritual, mythology, and so on). What Knight's theory fails to explain is what happened before that revolution. Knight's implication is that there was just the one revolution, whereas Dunbar's hypothesis of neocortex size, group size, and language is commensurable with a step-by-step model. Not with gradual evolution, but, if you like, with a series of revolutionary advances which can be

correlated with any number of other events. These could include: evolutionary stages in cognitive development, changes in social group size and the *Homo erectus* and *Homo sapiens* Out of Africa migrations, stages of language development, and, of course, the beginnings of systems of kinship. It is the multi-revolution hypothesis that makes most sense to me.

Let me leave linguistics and archeology now for a brief excursion into classic kinship theory. Since the early twentieth century, we have become used to thinking in terms of the differentiation of various “types” of terminology structure: “bifurcate collateral,” “Sudanese,” and so on. Or in the later twentieth century, sometimes in terms of a differentiation of “Iroquois” from “Dravidian,” either with reference to second-cousin classification or in terms of the logical concomitants of something called “prescription.” These are all useful as comparative ethnographic descriptions. However, in evolutionary terms, some structural distinctions are more basic than others.

In essence, there are only two kinds of kinship terminology structure. All others can be collapsed into these. The prime distinction is inherent in the opposition between Morgan’s own English language and Iroquois, whose terminology structure he discovered in the 1840s. However, it is not quite as he thought: *descriptive* versus *classificatory*. In such a scheme, Hawaiians, for example, line up alongside Iroquois as classificatory, whereas I would place them alongside English in the other category. This is because Morgan’s distinction rests on the presence or absence of a lineal/collateral distinction (as in English). I would place the emphasis instead on the presence or absence of a parallel/cross distinction (as in Iroquois, and most South African languages). Why? Because the essential meaning of a system like English or Hawaiian, which makes neither distinction, is: kinship is based on relative genealogical proximity and distance (see Figure 12.3). The same is true, incidentally, of some systems that make both lineal/collateral and cross/parallel distinctions (so-called “Sudanese” systems). All others, including Iroquois and so-called “Dravidian” structures, and “Crow” and “Omaha,” are based not on genealogical proximity or distance, but instead on classification: with categories like joking/avoidance, marriageable/unmarriageable, and so on. There are no names for these two logical opposite forms of kinship mentality, but I might suggest something like *genealogical* and *classificational*. Same-sex sibling equivalence is more widespread and, I believe, more likely than the notion of genealogical distance for the earliest true kinship system. Let

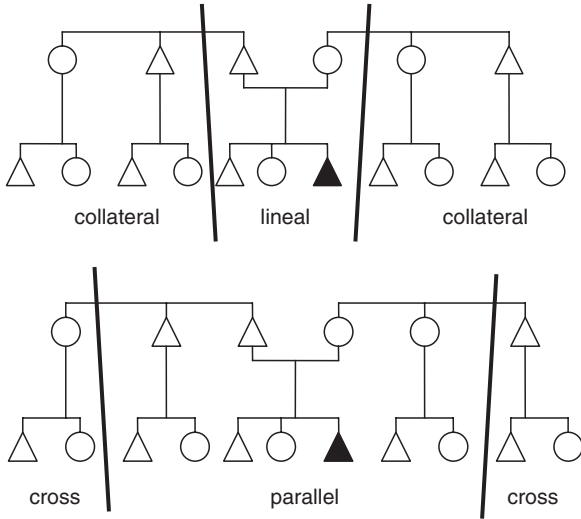


FIG. 12.3 Lineal/collateral and parallel/cross distinctions.

me suggest that at some point an incipient genealogical system became classificational, and that at that point categories, rules of behavior, and a full “grammar” of kinship, coinciding with the explosion of real (linguistic) grammar, came into being.

Beyond that, there are two radically different views concerning the possibilities of structures in the first true kinship system. Consider for a moment the global migration of *Homo sapiens*, in the third phase: after the establishment, in Africa, of true kinship. On the one hand there is the sociocentric view of early kinship, of Lévi-Strauss (1949) and of N. J. Allen (e.g. 1982, 1986), where moieties or sections define one’s position in a structure; and on the other, the egocentric possibility, based on relationship terminology alone. I favor the latter. The difference between the two views really depends on whether we see African models, or Australian or South American models, as the basis for the earliest system. Lévi-Strauss looks to South America, and Allen to Australia. The difference is whether we do or do not admit to the significance of alternating-generation equivalence as a second primal rule, after marriageable/non-marriageable. Allen, as a south Asianist, follows Dumont (e.g. 1975) in assuming such a rule. My notion of a primal egocentric structure, reminiscent of some San kinship systems, is quite consistent with Lévi-Strauss’s definition

of elementary structures, and is just as elementary, in his terms, as a moiety system.

Elementary structures of kinship are those in which the nomenclature permits the immediate determination of the circle of kin and that of affines, that is, those systems which prescribe marriage with a certain type of relative, or, alternatively, those which, while defining all members of the society as relatives, divide them into two categories, viz., possible spouses and prohibited spouses (Lévi-Strauss 1969 [1949]: xxiii).

Most British writers in the 1970s and 1980s were fixated by the concept of “prescribing marriage,” whereas my own emphasis was, and is, on the second part of Lévi-Strauss’s definition: “defining all members of the society as relatives.” I called this “universal kin categorization” (Barnard 1978), and virtually all hunter-gatherers and some small-scale cultivators, but not herders or anyone else, have such a system. It applies as much to Bushman systems, which are egocentric, working through individual kin terminologies, as it does to those of Australia or South America, which operate through sociocentric moieties, sections, or sub-sections.

Classic kinship studies are built on an evolutionary model, beginning with Maine’s *Ancient Law* in 1861. Maine (1913 [1861]) rejected social contract theory and substituted instead a model of progressive evolution of the family (from Roman times to modern). Morgan did much the same, though in a much larger timescale, from primitive promiscuity to civilization. And in 1975, Robin Fox did the same, from primate kin behavior to human kinship (Fox 1975). But we also have revolutionist theories that see kinship more in the mould of Hobbes, Locke, or Rousseau—precisely the vision that Maine objected to. The most important of these is Lévi-Strauss’s, but I would also put into this category Freud and Chris Knight.

Is there continuity or discontinuity between primate and human kinship? Slow evolution or revolution? This is analogous to the question of whether Kanzi’s preferred “word order” for the use of lexigrams on his computer keyboard constitutes a linguistic rule, or merely a personal preference (see Aitchison 1996: 114–116). I want to suggest that both the social contract and the family models are correct, but that they are correct for different purposes. There were two separate stages: the first was based on the social contract, and the second on the family. The



gradual evolutionist approach might be right for the development of protolanguage, and I would say also in a way for proto-kinship—although the advance of the *habilines* was of course also revolutionary. Only with the second and the third phases do universals appear. Just as there are language universals, there are also kinship universals, or at least near-universals. In his address on “The study of kinship systems,” Radcliffe-Brown (1952 [1941]: 49–89 *passim*) hints at two: the unity of the sibling group, and the principle of alternating generations. The former accounts not only for behavior, including the incest taboo, but also for the generation of the cross-parallel distinction still found in most of the languages of the world.

So which is the earlier, African kinship, or, say, Australian (see Barnard 1999)? And do we have direct exchange, bride capture, or husband capture? Husband capture would seem implausible, but the other two possibilities would work, though possibly with differential dispersal of mothers to teach the mother tongue to children who inherit language genes (cf. Rodseth et al. 1991: 236–237). Here an analogy with Pierre Bourdieu’s (1972) theory of social practice is relevant. I make no comment on whether or not Bourdieu was right about the nature of society. That does not matter here. Rather, I suggest that a Bourdieuan model may be applicable to an early phase of both kinship and language. In Bourdieu’s understanding, the Saussurianism of modern social science is wrong in its emphasis on distinctions between diachronic and synchronic, signifier and signified, and so on. And, in his view, social scientists should reject external or pre-existing rules and structures in favor of what he called “dispositions” which make up a “*habitus*” (or environment of dispositions). His model allows for individual choice within constraints.

At some level, no doubt his model is a truism. But I see its relevance to our problem in determining the origins of language and kinship not in opposition to structuralism, as he did, but rather as a possible model for pre-structured kinship, and by analogy, for pre-structured language. The Bourdieuan *habitus* (plural) of species-specific, and even “archaic” *Homo sapiens* kinship, lie at the root of kinship systems like those postulated by Lévi-Strauss. What is required to transform a Bourdieuan model into a Lévi-Straussian model is the reformulation of dispositions into rules. This requires language, not just in the sense of speakers being able to state rules, but also in the sense of people being able to name categories to which rules

refer. The conundrum is that while the categories may seem logically prior, they may in fact be secondary in logical and chronological senses.

One need not start with moiety organization, as in traditional readings of Lévi-Strauss's model, or with moieties plus intersecting generation categories, as in Nick Allen's. Rather, the propensity to exchange people in sexual unions or marriage necessarily leads to the creation of marriageable and non-marriageable categories. These can be defined either sociocentrically (as in the case of Australian Aboriginal systems) or egocentrically (as in the case of Kalahari San systems). All kinship systems have egocentric categories, but only some have sociocentric ones in addition. Rules of marriageability in purely egocentric systems can be generated with a principle of exchange, and the natural concomitant of any such rule is kin category extension such that members of subsequent generations know, for themselves as individuals, who is marriageable and who is not.

Exchange of siblings as spouses leads, in two generations, to a distinction between cross and parallel cousins, with siblings being equated with parallel cousins. One does not need a rule of descent. In N. J. Allen's model, a rule of generation (mine/the other one) will do. San or Bushmen do not have either of these sociocentric sets of categories (unilinearly defined moieties or sociocentric generation categories), but find their kinship categories through entirely egocentric means, such as name links (in the case of Ju/'hoan and Naro) or simply friendship links (in the case of G/wi and G//ana). Very unusually for hunter-gatherers, Ju/'hoansi do not make a cross/parallel distinction, but they do extend kin categories universally. That is, everyone in Ju/'hoan society is related to everyone else as if a close relative. My namesake's sister, I call "sister," and I am not allowed to marry her. The system works, in that where contradiction might occur with two people classifying each other differently, the older classifies the younger, who then reciprocates appropriately. If I call the young woman "sister," then to her I am a "brother." It is not just marriageability that is at stake, but how close two people may sit, whether they may joke or not, behave informally towards each other, and so on.

Naro kinship is in a sense analogous to the creation of a creole. I studied Naro kinship first in 1974-5 and commented some years later (Barnard 1988a) that, when one compares the terminology structure of Naro and other Khoe languages like G/wi, to Ju/'hoan, which is different, it is clear that Naro has elements of both structures. The Naro have been in contact with Ju/'hoansi for hundreds of years, and they have apparently borrowed

the Ju/'hoan naming system, although importantly, not their use of the lineal/collateral distinction. (Naro and G/wi, like Iroquois, distinguish parallel from cross-relatives, whereas Ju/'hoan, like English, distinguishes lineals from collaterals.) Among Ju/'hoansi, one is named after a grandparent, or if one's parents are out of grandparents to name their children, one is named after an uncle or aunt, which alters the structure. In the Naro case, one is always named after a grandrelative; for them, cross-uncles and aunts are grandrelatives too, and the Khoe parallel/cross distinction is not altered. Rather, at a lower level of structure the senior/junior distinction of other Khoe terminologies disappears; Naro has borrowed a Ju/'hoan word *tsx̄o* to use for grandrelative, and this term is applied both to seniors and juniors. This is coupled with the fact that a namesake is structurally equivalent to one's self, and for example, one's father's namesake is equivalent to one's father. My father's name was John. Anyone bearing the name John will be my "father," including, let us say, my own son.

In Bickerton's view, a creole language is a "nativized pidgin." The early mixture of Khoe and Ju/'hoan kinship was analogous to a pidgin in this sense; and the system which emerged, Naro, to a creole. The Naro case is unique in southern Africa, but we know of Australian examples of the creation of new kinships systems, and indeed ones with sections and subsections. Provided groups are allowed by their neighbors to exist without violent interference or subjugation, they can sometimes in a generation or two create a new kinship system. The point is that the system created really is a kinship system. It is not half a kinship system. And most certainly it is not a kinship system where different people play by different rules.

Like creoles, kinship systems are always fully formed, even if they are mixed. They are always "grammatical." That is, each follows its own internal logic, due in part to the law of uniform reciprocals. This is best illustrated by "Crow" or "Omaha" terminology structures. For example, in a "Crow" system, if I call my father's sister's son "father," I have to call my mother's brother's son "son," because I am his father's sister's son and he will call me "father."

## 12.5 Conclusion: a theory of three revolutions

There are still several, related, remaining questions. Which came first, kinship or language? Or did they evolve, or spring up suddenly, together?

Was one spontaneous and the other slowly developing after, or for that matter one more slowly evolving, and the other relatively spontaneous later? Were women first exchanged as wives, as in Lévi-Strauss’s model, or for food? Or were women doing the exchanging, sex for food, as in Knight’s model? I propose that mechanisms of universal kin category extension evolved simultaneously with the growth of population size, either through expansion of groups or through amalgamation. Amalgamation, not necessarily of physical bands coming to the same place, but of groups recognizing common sociality as a product of spouse exchange plus parenthood. Figure 12.4 shows my theory of the co-evolution of language and kinship.

Let me state my basic idea in simple terms. I propose that there were three biologically induced human social revolutions. Each has consequences for the evolution of language and for the evolution of kinship. Let us call the first the “signifying” or “sharing revolution” (in the original, spoken version of this paper I had called it the habiline

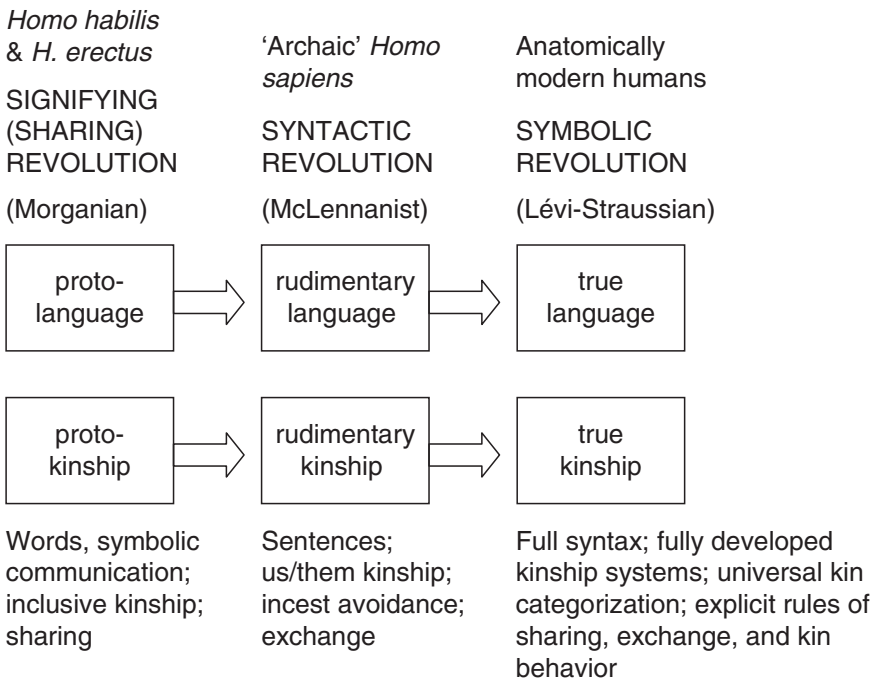


FIG. 12.4 A theory of the co-evolution of language and kinship.

revolution). It is associated with increased neocortex sizes over those of australopithecines, with consequent increases in group size from 65 or 70 to 75 or 80, which, others have argued (e.g. Dunbar 2001), triggers with the production of the first stone tools, protolanguage, and forms of sharing. Sharing (as with chimpanzees, possibly previously existent but not strong beyond the family) replaces grooming as a central form of social interaction, and individuals bear names, identities enabled by the increase in self-and-other-awareness (which is a product of neural evolution); and there is a form of proto-kinship. For me, this phase is analogous to Morgan's phase of group marriage.

Let us call the second phase the "syntactic revolution" (previously the archaic revolution), associated in fossil terms with "archaic" *Homo sapiens* or *Homo heidelbergensis* neocortex size increases, and with it group size to 120 or 130. Sharing is supplemented by exchange. People have greater need to communicate, and they do so in sentences. As in Sahlins' (1965) essay on "The sociology of primitive exchange," there is sharing or generalized reciprocity within the kin group and exchange or balanced reciprocity beyond it, but within society. Beyond one's own society, there is no doubt negative reciprocity: fighting, raiding, and possibly exchange of people as spouses. If the first phase is Morganian (or of Morgan), the second phase is McLennanist. There is rudimentary kinship, perhaps classification and incest avoidance. But there is as yet no fully structured kinship system, because kinship terminology is not yet extended throughout society. The second phase would, however, involve what Runciman (2005: 129) has recently called "cultural selection for strong reciprocity sustained by specific pressure from the palaeo-ecological environment."

These first two revolutions are possibly new, with this paper, but the third phase already has a name. It is commonly called the "symbolic revolution." There are different ideas about concerning its elements and its impetus, Knight's being the most prominent. For the moment, I prefer to think strictly in terms of the kinship and language model being proposed here, and I see this as a Lévi-Straussian revolution, the result of which is the diversity of kinship structures and of languages found in the world. And ultimately, the transition of elementary structures into complex ones and universal systems of kin classification into non-universal ones, as non-kinship mechanisms of social control take over. The presence of this last revolution is now widely accepted, although the relation of the earlier two to fossil finds and indeed the relation between

the linguistic and social revolutions is less secure. For example, whether the signifying, sharing, or habiline revolution is associated literally with *Homo habilis* or with *Homo erectus* might be a matter for further examination. At this stage, my bridge theory should be taken simply in broad terms, as indicative of the kind of co-evolution to be expected between language and sociality. It is perhaps less a complete explanation, and more a route to an explanation.

Earlier I made a distinction between genealogical and classificological terminology structures. I am certain that systems have changed through time and in either direction. However, if “archaic” *Homo sapiens* classified kin with kin terms at all, it seems reasonable to suppose that genealogical distance would have been important to them. With the increase in the significance of exchange, either gradual or revolutionary, the pressure to identify classes of people (marriage/unmarriageable, in particular) must have been great. However, with the symbolic revolution, and fully formed kinship structure, classificological kinship structures came to form the basis of human societies. Let me therefore end with the hypothesis that what Blombos Cave implies, in relation to kinship, is that, as language, art, and ritual emerged, genealogical reckoning became classificological and incest avoidance became incest taboo. Lévi-Straussian elementary structures appeared from the Bourdieuan habitus, and the earliest systems were egocentric. In the subsequent tens of thousands of years, the vagaries of history led to a multiplicity of kinship systems and of languages, the evolution of more elementary sociocentric systems (like those of Australia and South America) and of Lévi-Straussian “complex” ones, and with the latter a return in some systems, to the genealogical.

# 13 As well as words: Congo Pygmy hunting, mimicry, and play

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JEROME LEWIS

## 13.1 Forest hunter-gatherers

In the dense equatorial forest of the Congo Basin, seeing is useful for following events close by, but only hearing reveals what is further away. As Mbendjele hunter-gatherers in northern Congo-Brazzaville move about their forest they are hypersensitive to the sounds around them. All members of the party, children included, react instantly to a crack, a low rumble, or an animal call by stopping mid-step, balancing on one leg if necessary. Silent and motionless, they strain to hear any follow-up sounds that will tell them whether to run for their lives, chase after their supper, or just continue onwards.

The significance of sounds for Mbendjele Pygmies has consequences for communication that may be suggestive about conditions relevant to the emergence of language. In particular, the Mbendjele's rich communicative culture demonstrates the importance of interpreting language broadly, going beyond just speech with words to consider all the ways that sounds are strung together to convey meaning effectively.

Mbendjele have developed specific styles of communication for different audiences and situations. They mix words with sung sounds, ideophones, expletives, whistles, signs, hand signals, gestures, vocabulary from other people's languages, animal sounds, and other environmental

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sounds, sometimes in a single speech act. In the context of forest hunting and gathering, the role of different language styles and communicative strategies suggests that diverse styles of communicating could have been crucial to the survival of early humans, and imply that our focus on words may be partly due to a modern bias to lexical expression. Here it is argued that in terms of iconicity, words are at the extreme end of the symbolic communicative continuum.

Speech and speaking styles are gendered in many cultures, and this is the case here too. In the forest, for instance, whereas men tend to walk quietly in small groups or alone and use a variety of disguised and undisguised speech styles, women walk in large groups, rarely alone, and accompany each other's speech with conventional sung sounds that contribute to increasing the volume and distinctive melodiousness of their conversations. Women talk, sing, or yodel loudly in the forest to ensure that they do not surprise dangerous animals. Menstrual odor is said to anger large dangerous animals such as gorillas, elephants, buffalo, and leopards, causing them to charge or attack people who smell of it. This is culturally elaborated into a complex of practices and taboos referred to as *ekila* that define the sexual division of labor and proper sharing practices (Lewis 2002: 103–123, 2008).

Conservationists from the Wildlife Conservation Society at the nearby Nouabali-Ndoki National Park estimate that there are approximately 23,000 gorillas and 9,000 elephants in an area occupied by 7,500 Mbendjele. This fearfulness, therefore, is not unreasonable.

Women's fear of attack encourages them to do their daily activities in noisy groups. Infants and small children will be either left at camp or held close in a bark or cloth sling. They go gathering, fishing, digging yams, and collecting nuts, insects, and fruit in groups, often with boisterous older children in tow. Women spend much of the day together. The strong solidarity which this establishes between them has important consequences for their status and political power because they use it effectively to influence camp decisions. If women refuse a proposition made by men, men have great difficulty in persuading them to change their minds, and women quickly support each other in situations of conflict with men.

Men mostly talk in turn, and although tunefully responding to what is said, they do this in a subdued manner by comparison with women. Although men can speak loudly in camp, they do so less in the forest. When hunting they value quietness explicitly. Not simply in speech but also in step. Particularly accomplished hunters walk in smooth slow



motion, gently padding their feet down toe-first, gliding silently through the forest. If passing through dense, noisy undergrowth, they sit down in silence from time to time in order to scout with their ears, discussing in whispers and signs the sounds they can hear.

Mbendjele pay careful attention to the sounds of the forest and take pride in mimicking them precisely when recounting their day or chatting. When describing an encounter with a forest animal great attention is paid to the acoustic features of the event—lexical descriptions may be dropped for meticulous mimicry of the sounds of the encounter, from the thrashing of trees, to the calls or hoots of the animal that tell their forest-educated listeners all they need to know. There is a common vocabulary of characteristic sounds that are regularly incorporated into accounts and stories.

I call these characteristic sounds of encounters with animals “sound signatures”.<sup>1</sup> This is not onomatopoeia. The word for gorilla is onomatopoeic—“*ebobo*.” This sounds rather like the beautiful “*bobobobo*” call gorillas make to know where group members are. However, the sound signatures used most often to represent gorillas are their warning barks—meaning “I passed near to a male gorilla.” Or the characteristic furious retching roar followed by the sound of thrashing bushes that means “I was charged by a silverback.” These represent the typical sounds of an encounter with male gorillas. Juveniles and females are rarely encountered since they tend to flee from people.

Hearing these sound signatures while listening to people’s accounts of their experiences both reminds and educates listeners. Younger listeners’ attention is drawn to key warning sounds, and all are reminded of the actions behind the sounds, and what to do or not to do in response. This style of story-telling is especially cultivated by men on the secret *njanga* paths associated with their secret societies. In such exclusively male places men re-enact great hunting moments by making all the key sounds as exactly as possible, but also by performing the classic postures and moves of themselves and the animal as the tale unfolds. This is called *moadjo ya batopai*.

It is in these moments that the key tactics for approaching different prey animals are discussed and debated, subtle techniques for finding honey and other prized foods are shared, and the youth learn about the key

<sup>1</sup> Sound signatures here refer to species specific sounds, not an individual’s specific sound.

postures and sounds animals and hunters make to guide them during forest encounters. It is in these ways that young men begin their apprenticeship in advanced hunting techniques.

By cultivating their listening skills Mbendjele have also developed their skill in mimicry. This skill is used to great effect to call animals to them. Men commonly fake animal calls to lure prey within range and view so they can shoot or spear them. I have seen men successfully call three species of duiker, numerous monkey species, and crocodiles. Mbendjele men apply similar “faking” principles to their strategies for obtaining goods from non-Mbendjele. So, when visiting neighboring villagers they endeavor to get what they want at minimum cost and as safely as possible by playing up to the villagers’ arrogant pretensions, claiming pity, and speaking in the villagers’ tongue.

### 13.2 Learning Mbendjee

Such attentiveness to the sound dimension of experience has consequences for language that became evident early in my PhD fieldwork when I began to learn Mbendjee. Different people would give me different words for the same object, different families would pronounce the same words very differently, men and women had different styles of talking, even children had certain words only they would use.

As my language ability improved so I could follow people’s conversations, I noticed that their use of alternative words was often bound to context. Thus when recounting a visit to a Bongili farmers’ village, the speaker would switch into Bongili-ized Mbendjee when talking about the village. When recounting a conversation with a Bongili the entire exchange would be recounted as much as possible in Bongili. If a speaker reached the limits of their knowledge of the language they simply mimicked the accent and typical expressions, and listeners would guess at the meaning from the words they did enunciate. In these accounts it was striking that Mbendjele seemed more concerned with mimicking the acoustics of the encounter than with conveying meaning lexically.

Although I had expected Mbendjee to fit my European concept of a language as something bounded, composed of a recognized set of words, belonging to a distinct population or speech community, I no longer do. It makes more sense to consider Mbendjee as something broad and

all-embracing—a tool for establishing and maintaining social relationships with anything and anyone that might respond.

Cross's suggestion that "music and language constitute complementary components of the human communicative toolkit" sits comfortably with the Mbendjele ethnography (Cross 2005, 2006). Following Mithen (2005), Cross argued that any evolutionary precursor would have expressed functions of music and language, rather than one or the other. Mithen's characterization of compositional language as including arbitrary signs in addition to "protolanguage" holistic, manipulative, multi-modal, musical, and mimetic aspects, describes Mbendjee well. The range of techniques—from hand signs, plant signals, whistles, and other animal sounds, to human words and grammar, singing and ritualized dancing—that Mbendjele incorporate into their communicative toolkit also suggest that it may be useful to consider human communication broadly when hypothesizing the emergence of language.

Mbendjee is an open, expansive communicative tool that imitates any other languages or meaningful sounds and actions that enable Mbendjele to interact with agents with whom they wish to maintain social relations. These agents can be other Mbendjele, villager neighbors, crocodiles, duikers, monkeys, and other animals, and as I shall shortly describe, also the forest. Mbendjee could be described as "pre-Babel" because it seeks to communicate as widely as is possible, potentially incorporating everything that conveys meaning regardless of origin. Mbendjee seeks to communicate, at least in theory, with the whole world. In spite of this predatory approach to vocabulary and general openness to the new, linguists agree on classifying Mbendjee as a unique Bantu C10 group language<sup>2</sup> (Klieman 1997, 1999: 90–91; Rossel 1999: 109; Thomas 1979).

This has interesting parallels with observations of cultural openness among other African hunter-gatherers. Woodburn (1982: 448) characterized Hadza society as an open society, with no basis for excluding anyone. Lee commented that "the !Kung *consciously* strive to maintain a boundary-less universe because this is the best way to operate as hunter-gatherers" (1979: 335). Although he was referring to territory, it also applies rather

<sup>2</sup> Hombert described how Pygmies mostly speak the languages of their neighbors (2006). Although Mbendjele maintain relations with over 13 different agriculturalist groups, Mbendjee is not spoken by non Pygmies (Lewis 2002: 52).



events; *bhuuuu* refers to arriving somewhere, *teeeeeeeeeee* to duration (length of utterance representing duration), *dtooo* to shooting, and so forth.

### 13.4 Women's talk

Although children, men, and women all speak like this, the importance of laughter and humour, of timing and overlapping interjections, of tune-fulness, rhythm, pitch, and appropriate body language is most developed in women's speech styles. During the day, when sitting in a group in camp and chatting to pass the time, women excel in animating the camp with this style of melodious and periodically raucous banter. The sound sometimes reminded me of a beautiful anthropomorphized birdsong. In the forest women regularly break into song, mostly yodeling, and substantially increase the volume when talking, ensuring they can be heard far away.

When women are resting together they often begin *besime ya baito*—women's chat. Though any situation of note can be discussed, women often focus on men's behavior, particularly that of their husbands. In exaggerated tones with enthusiastic miming, accompanied by laughter, a wife can recount a husband's misdemeanors to shame him. His fury will only encourage her, and other women will gang up against him. Most men leave for a secluded place. Some good-humoredly join in and laugh. Occasionally it ends in violence. Generally, Mbendjele men tolerate such explicit criticism if women do it, despite their embarrassment. When men do it, it causes serious fights.

The shaming element of such speech is central to women's power in society. In its most elaborate form women perform theatrical sketches that re-enact people's bad behavior or a situation. This is *moadjo ya baito*. Recently, I saw some women use this to mock a grasping villager. The actors focused on key moments in their early morning encounter with him and repeated this many times. With repetition, his key expressions were mimicked increasingly well—with masterful attention to the eyes and stare, the mouth and accent, the gait and other traits. The results provoked hilarity.

In the early stages watchers reacted with the normal sung expletives that women use to accompany each other's speech. As the laughing and enactment proceeded they increasingly made comments such as "Liar!"

or “What hard hands!” (how tight-fisted or stingy) to underline the moral context of the humor. Younger girls were restrained in their comments, but laughed loudly. Older women quickly became boisterous, supporting the actors with jokes and offering humorous condemnation of mimicked behavior. By the end of the show the women had mapped out the moral high ground: exposing the villagers’ attempts to impose a moral universe of debt and labor as unjustified, unfair, and contemptible. Such *moadjo* informally educates those present about Mbendjele moral values.

Widows have a special place in this type of humorous but directed criticism and are expected to do this in serious moments when tensions are high. A good performance will succeed in calming the atmosphere by allowing everyone to laugh and forget their anger. Indeed if the person being criticized is present, the *moadjo* will only end when they have laughed aloud. However, most often trouble-causers flee when they see themselves becoming the center of the camp’s mirth, to hide in the forest until things calm down. This is suggestive of Eibl-Eibesfeldt’s (1989: 138) hypothesis that laughter is derived from “an old pattern of mobbing behaviour.”

In the Mbendjele’s case, women demonstrate solidarity by colluding with the performers to mock skillfully and shame others by mobbing them with laughter. *Moadjo* enables women to make mimicry a powerful form of social control that imposes their moral authority over the rest of society.

### 13.5 Forest talk

In the forest men communicate with each other and the forest’s occupants in ways suggestive of communicative skills that could have given our hunting ancestors evolutionary advantages. Some techniques are meant to be understood, some to go unnoticed, and others are used to deceive.

As any tracker knows, plants speak. The way a plant is bent, broken, or flattened, or the handling or feeding marks left on or around it, reveal much to an educated observer. Mbendjele imitate these plant signs and turn them into symbols they use to leave messages for each other. Camp movements are traced by leaves placed strategically on paths. Hunters pursuing prey snap over young saplings to mark their route so others may find them. Lovers bite distinctive patterns into certain leaves and put them at junctions so their lover may find them. Walkers are warned of trap-lines by bark slats jammed horizontally into split saplings, and so on.

Men out hunting use disguised modes of communication to maintain contact with each other without the animals knowing. For example, I recorded a variety of hand signals referring to animals, movement, and activities, some of which are illustrated in Plate 5. These include signs for aardvark, bay duiker, bees, black-fronted duiker, bongo antelope, buffalo, chimpanzee, crocodile, elephant, fish, gorilla, leopard, several monkey species, monkey eagle, porcupine, red marsh duiker, snake, tortoise, and wild boar. There are also signs for movement—"go around it," "over here," "right here," "stop!," "marshy"—and others such as "I'm thirsty," "I'm hungry," "lay your head on my arm" (i.e. let's have sex), "honey," "drink," "marijuana," and "tobacco." These and other signals are used outside hunting contexts, either alone or to add another, often secret, message over the one being spoken. Men especially use signs in farmers' villages to inform each other of the location of desirable goods such as tobacco, alcohol, and marijuana, or to warn each other of problems. But most signing occurs during hunting.

When tracking or sensing the presence of game nearby men stop talking and simply sign to one another. Group spear-hunting techniques, such as the popular *bopfienga* style, depend on these disguised modes of communication. The *bopfienga* encirclement technique is used on herd animals such as pigs or large game such as buffalo and elephants. A group of men armed with spears wander in likely habitats until they notice the sounds or fresh traces of game. It is rarely productive to chase game directly since animals move faster than men through the dense undergrowth. Rather, hunters predict where they are likely to go next using their knowledge of the animals and resources around.

Silently and rapidly they move towards this area to prepare an ambush. If they were correct and the prey arrives, they fan out in carefully orchestrated movements silently encircling the prey. Sign language and fake bird-calls are crucial at this stage since all must know where the others are in order to prevent accidents when the action starts. During the wait and subsequent encirclement men only communicate in these ways. Each local group has its own characteristic bird or animal call that men habitually fake. In this way they coordinate their movements without the prey being aware. If hunting pigs, when everyone is in position an experienced man signals before lunging forward to strike a boar and so scatter the other pigs towards the hidden hunters. They in turn strike out. Often several pigs are

killed. This hunting style was a mainstay of the past, but is now increasingly replaced with shotgun pursuit.

Another context in which men use sound-making deceptively is in ritual languages. In addition to employing their own unique vocabulary, ritual languages disguise words from normal language by giving them new meanings only interpretable from the context of their use. Although men appear to have more ritual languages, this may reflect my lack of access, as a man, to the women's secrets. Men have distinctive vocabularies associated with particular secret societies, but these are too secret to divulge to the uninitiated.

Mbendjele love playing with signs, symbols, and meanings. They excel in making one thing stand for another, even making the same thing stand for different things to different people or change meaning according to context. They also like to fake. They manipulate acoustic and physical symbols intelligently to communicate selectively, deceptively, and generally, not according to a rigid grammar familiar to written languages, but with one flexible enough to structure and encompass a wide ranging vocabulary of words, utterances, and actions from many sources, even from animals.

Men fake animal calls to lure animals to them. Most men competently fake many key animal sounds. I remember watching a group of men passing time deceiving a mother hen by so perfectly mimicking the chirping of her chicks that she would constantly attack them—to laughter and a gentle shove. Most young men are capable mimics of bay duikers and blue duikers, both very abundant game and popular food. Faking the duikers' call "come frolic with me" brings them to within a few meters of the hunter. The confused animal can return again and again, unable to understand why he is not meeting another duiker.

Monkeys are drawn out of the canopy into range of crossbows or shotguns by faking the sound of a fallen infant or the call of a monkey eagle. Faking a crocodile's mating call while standing waist deep in sludgy dark water and as return calls gradually get closer requires courage. The crocodile is lured onto a small island in the marsh where prepared liana ropes are used to trap its jaws shut, before binding its limbs. Calling pigs is done when pigs are already close by and involves mimicking eating sounds so as to attract greedy animals close enough to be speared.

Mbendjele men have made mimicry an art form. From plant marks to animal calls Mbendjele mimic diverse signs and sounds. They practice



these in their spare time, pepper their speech with them, encourage their sons to learn, celebrate their successes, and appreciate excellence. From leaving marks for other hunters, to using bird calls to encircle pigs unnoticed, or by calling duikers to frolic, mimicry facilitates hunting success.

There are some rare cases where non-human predators use sound to lure prey, but the sounds in question are normal sounds for that species to make. Their ability to lure prey is accidentally discovered. Thus cheetahs call their young using chirruping sounds that also happen to call certain birds towards them. Some bird-eating cheetahs are said to employ this as a hunting technique. Northern shrikes (*Lanius excubitor*) have been observed “acoustically luring” prey birds such as small passerines (perching birds). The research provides no evidence for the intentionality of the practice, simply that it seems to work (Atkinson 1997).

Mbendjele hunters make excellent practical use of a key distinction (Tomasello 1999) between human and animal cognition: Humans recognize the intentions behind an action or sound easily, but animals do not. As predators, this gives us certain advantages over prey that could have been critical in enabling modern humans to survive difficult or extreme periods in the past. Animals such as duikers have extreme difficulty recognizing that the hunter is lying. The techniques Mbendjele use are successful time and again because the animals concerned cannot fake these sounds. Whereas animals make vocalizations according to context, humans can vocalize out of context, to trick, to lure, or to tell a tale.

Mbendjele apply mimicry and deceit subtly and creatively in their predatory activities with humans too, at least where it works to increase success and reduce effort and danger. In relation to villagers, Mbendjele are willing to humiliate themselves from villagers’ perspectives, to get what they want. Esakola, an elder Mbendjele man from Mobangui, explained: “When you want to kill an elephant you must follow it. It is hard work and dangerous. You must smear its fresh excrement on yourself. It is the same for us with villagers!” Esakola expresses the perception that despite the initial discomfort of mimicking prey, it will result in the unspoken reward of elephant meat or farmers’ goods. Smearing shit on one’s self is equivalent to playing up to the expectations and conceits of the villagers. By theatrically mimicking villagers’ stereotypes of themselves, Mbendjele get things as safely as possible. This includes aping village customs and

language while appearing fearful, even stupid, mumbling shyly when spoken to, never looking in the eyes, behaving childishly, laughing inanely, praising and flattering, agreeing to whatever is suggested without any intention of doing more than is absolutely necessary, buffoonery and clowning. The key is to make villagers feel superior, to make them feel like a patron, obliged to satisfy the pitiful Mbendjele's request. Köhler and Lewis (2002) discuss these relations in comparative perspective.

Amongst themselves Mbendjele frequently refer to the villagers as "gorillas" because of their tendency to shout angrily at Mbendjele without good reason. As one man explained, just as gorillas charge up making terrifying noises when you accidentally tread on a twig while walking in forest they occupy, villagers do the same to Mbendjele, shouting a lot and becoming violent and dangerous because you walked across their farm. Mbendjele insist that villagers are reborn as gorillas in their next lives. Using predatory techniques based on mimicry and deception to get things from villagers is acceptable because they are really gorillas. Once Mbendjele have got what they want they tend to drop the pretensions and will be quite normal with villagers, both polite and friendly. But if they don't get what they think they should have they can be rude and insulting (Lewis 2002: 231–232).

Using animal labels for groups of people and applying hunting techniques based on mimicry and deception to get desirable things is indicative of the way that hunter-gatherers perceive themselves as agents interacting with other natural agents (such as non-humans, plants, and other humans) in nature, rather than subjects in a society somehow outside of nature. This has been usefully described as perceiving the environment as a "single social field" (cf. Bird-David 1990, 1992; Ingold 1994, 1996). From this point of view Mbendjele faking animal vocalizations is equivalent to speaking with people.

As sophisticated mimics, Mbendjele communicate with a range of agents who share their forest, using plant signs, fake animal calls, villagers' languages and customs, and aping the ideas villagers hold about them. Between these "spheres of involvement there is no *absolute* separation, they are but contextually delimited segments of a single field" (Ingold 2000: 47). For the Mbendjele the forest is their "single field." Their relationship with it is total, expressed in the proverb, "An Mbendjele loves the forest as she loves her own body."

### 13.6 Singing without words as the most expansive communication

Mbendjele are very concerned to please the forest. Although rarely expressed in ordinary talk, a common explanation given to me for many practices—such as why people tell stories in the evenings, or sing a lot—is that the forest likes it. When the forest is pleased it “opens” the camp and all the things people need for a good life are easily available. Mbendjele, like other forest hunter-gatherers (Bird-David 1990, 1992), see the forest as abundant, ready to dispense resources as they are needed. The “people who came before” (*bisi boso*) showed the “children coming afterwards” (*bana bambusa*) how to please the forest so that their camps remain open for food.

Most notably, they left behind *ekila* rules derived from symbolic elaborations around menstruation that serve to define gender roles, correct sharing, and proper conduct. Doing *massana* of both play and ritual varieties and maintaining amiable relations between camp members is also important. As with other forest agents, it is the sounds coming from people in camp that indicate if camp life is good (*enye*) or not (*mobe*). The good sounds are in the styles described above: the laughter and song of good-humored people telling stories, joking, and doing *massana*. This is contrasted to *motoko*-noise. The sounds of angry debate, of argument, of whingeing children, of people shouting aggressively, screaming, or fighting are all *motoko*. They are the product of discord, stress, pain, suffering, and an absence of cooperation or of sharing. Too much *motoko* leads to the camp “closing to food.” When this happens camps split up and people go to different places. In the same way that Mbendjele listen to the forest in order to know about it, the forest listens to them to know about them. Just as animals respond to Mbendjele sound-making by fleeing, coming to them to facilitate capture, or attacking them, so the forest provides abundantly or withholds food according to the sounds it hears.

Likewise, Mbendjele listen constantly to the forest. Hearing sound signatures tells them who is there. The steady drone of bees returning to their hives at around 4p.m. indicates that it is time to stop tracking and return to camp. If frogs are heard it means there is water to drink nearby, and so on. Recognizing a monkey’s “saw a leopard” shriek from “saw a pig” yell or “found good food” scream provides important information.

To listen so intently, and understand so much of what the forest tells you, turns being in the forest into a sort of communion, a constant conversation. The importance of this conversation has led to the elaboration of some sophisticated ritualized communication techniques intended to influence the forest positively.

Mbendjele systematically attribute intentions to animals, quite commonly anthropomorphizing them in fables and story-telling. Intentions are attributed to their sound-making. So Kata would systematically break into howls of laughter whenever he heard the tree-hyrax's sexually desperate-sounding mating call. Or one night Emeka swore furiously, threatening a silverback gorilla who repeatedly barked at us for camping too close. From an Mbendjele point of view, their acoustical interaction with the forest is a conversation with a complex multi-agent organism we call a forest.

This forest soundscape is ever-present. Every creature has its contribution to make, and many coordinate with each other, as cicadas seem to on a warm afternoon. When creatures contribute they do so with their whole bodies, with all their might; they seem to live the sounds they make. A small bird in the undergrowth puffs his chest and chirrups his tune, then pauses before repeating the performance. Cicadas and grass hoppers rub their hard washboard legs against their abdomen, and other insects with similar sound-making abilities repeat their different sounds over their own unique period. Other creatures join this overlapping and intertwining soundscape at their own pace and rhythm; monkeys here, birds there, tree hyrax elsewhere, and so on.

When people really want to charm the forest they turn their part of this conversation into a song, a song which involves their whole bodies, and mimics the forest back to itself. This is done using percussion, polyphonic singing, and dancing. The people who came before established particular ways of doing this called *mokondi massana*. There are many styles of *mokondi massana*, each with its characteristic repertoire of melodies, songs, clap and drum styles, and different ways of dressing (or not) the mystical forest creatures called *mokondi* so that they come safely into camp. I shall shortly describe this in more detail, but should first introduce *massana* activities.

Both children and adults should play regularly at *massana*. This is crucial for keeping the camp open to food. It is also the major avenue for Mbendjele to learn the key skills that they require to live well in the forest (Lewis 2002: 124–195). *Massana* activities range from any children's

play that involves groups of children cooperating together—from games like “tag” or pretending to dig for yams, to making “play” huts, or spearing slices of a banana plant rolling along the ground. In these contexts younger children learn to observe and mimic older or more skilled children with the intention of becoming skillful themselves. In this light-hearted and supportive atmosphere they cultivate expertise by becoming increasingly aware of the intentions behind the other’s actions. Learning is implicit and self-motivated. The egalitarian ethos of Mbendjele society makes overt instruction potentially offensive since it implies power differences.

Older children’s *massana* activities include more sophisticated and structured role-play games that require high levels of coordination and cooperation. Hunting games now involve groups of boys mimicking animals while others mimic hunters laying an ambush; “play” camps now also have hearths to roast food; tree-climbing games use the participants’ combined weight at the top of a supple young tree to bend it over, at which point all but one let go and the one left is taken on a high-speed, high-thrill spring back to upright (see Lewis 2002: 124–136 for more examples).

These styles of playful cooperative merriment in complex coordinated activities are at their most sophisticated in *mokondi massana* rituals involving complex polyphonic singing. When the group achieves the synergistic harmony familiar to good choirs or orchestras, the forest shows its pleasure by allowing the mysterious forest spirit-creatures called *mokondi*, sometimes embodied as leafy dancers, sometimes simply experienced as an ambience, to enchant the participants and further deepen the profound *communitas* they experience.

While forming a continuum with children’s play for Mbendjele, *mokondi massana* have been the subject of considerable outside interest, both locally and from abroad,<sup>4</sup> due to the spectacular beauty of such performances. Locally, Pygmies are so renowned for their ritual skill that they conduct all the major community rites for their villager neighbors. They are considered the most expert singers, dancers, and musicians of the region. Despite Pygmy groups speaking different languages, they sing in a

<sup>4</sup> Olivier and Furniss (1999: 117 fn. 1) list ethnomusicological accounts of Aka (Mbendjele) music, and Feld (1996) identifies the many exports of it into contemporary western music.

similar polyphonic style and organize their rituals in terms of similar principles and institutions—mostly gendered secret societies (Lewis 2002: 29–31).

During the musical performances of *mokondi massana* the whole camp assembles in the central space. People sit in gender groups, close together with shoulders touching and limbs resting on each other. As their bodies intertwine, so too do their voices—singing out different melodic lines that overlay each other to constitute the polyphonic song. Like each creature of the forest, each melodic line is different, has its own period, and combines itself with other melodic lines, some with different periods. Typically each gender has its characteristic melodic lines, though they may also sing each other's lines from time to time. Singers switch melodies when they hear too many singers singing the same one, and people seem to improvise freely.

However, there are rules that govern how the polyphony progresses, what innovations are possible and what is appropriate participation, as Arom (1985), Demolin (2006), and Fűrniiss (1993) have shown. There is also much to be made of the continuities between how such singing and associated rituals are organized and the organization of social life in general (Arom 1978; Lewis 2006). But here I wish to emphasize how by singing in this way Mbendjele are playfully mimicking the forest's sound,<sup>5</sup> while contributing a uniquely human aesthetic to it that pleases the forest. Just as the forest's song is composed of the forest creatures each calling out in their own time with their unique sound, so too is Mbendjele polyphonic singing.

Many polyphonic songs belong to distinctive repertoires associated with particular forest spirits (*mokondi*). When the assembled Mbendjele doing *mokondi massana* get the song just right the forest sometimes shows its joy by allowing leafy dancers called *mokondi* to approach the singers and dance for them. When this is done well *massana* may go on through the night with participants entering euphoric trance states. In these performances, as participants melt into their neighbors sitting overlapping with them, so they also melt into the mass of sound they create. It is easy to lose oneself in this physical and acoustic mass and experience

<sup>5</sup> Nigerian superstar Fela Kuti used instruments such as the saxophone to mimic typical Lagos traffic sounds: horns tooting, tyres skidding, engines revving. Like Pygmy polyphonic singing, both employ environmental mimicry as the basis for music making.

profound *communitas*. When done with excellence, as demonstrated when *mokondi* appear, the group experiences *communitas* with the forest.

Arom (1978: 24) characterized Pygmy polyphonic singing as “pure” music because the melody is not subjugated to words. Actually, Mbendjele songs are not to be understood because of words they use from human language but through the acoustic form they have adopted based on the forest’s “language.” In a sense, their melodies are the forest’s “words.” This may explain why polyphonic singing is characteristic of Pygmies throughout the Congo Basin.<sup>6</sup> It enables them to commune and communicate with the entire forest, themselves included, by mimicking its sound back to itself. If one wanted to speak to the forest, what other language could be used?

Just as dancing is the most sophisticated form of walking, singing is the most sophisticated form of talking. So singing and dancing are the most inclusive and expansive forms of communicating,<sup>7</sup> encompassing all the forest’s inhabitants. The notion that song is an aspect of communication is not new, but is particularly relevant for appreciating African religions. Senghor eloquently expressed this: “En Afrique noire, c’est la musique qui *accomplit* la parole et la transforme en *verbe*, cette invention supérieure de l’homme qui fait de lui un *démiurge*”<sup>8</sup> (1964: 238, quoted in Arom 1985: 49).

Arom emphasized the role of music in central African religions as one of communicating with the spirits and enlisting their help (1985: 40–48). This, he argued, explains the strictness of rules about who can sing or dance in particular contexts, and suggests that this music is better understood as a rhythmic utterance. The intended recipient of the Mbendjele’s rhythmic utterances is the forest as an organic whole, of which people, spirits, and animals as well as plants are all part. Thus, in *mokondi massana* the Mbendjele are the forest singing to itself. When communication is

<sup>6</sup> This ubiquity also extend to other cultural traits beyond singing and musical styles: see for instance Arom 1978; Bahuchet 1992; Hewlett and Cavalli Sforza 1986; Kazadi 1981: 836; Lewis 2002: 53–70; Thomas 1979: 145; Turnbull 1966: 23.

<sup>7</sup> This resonates with Demolin’s comparison of language skills with those required for vocal music, which shows that more are required for music than speech (2006), as does the data from brain imaging suggesting that music generation and perception tasks require larger networks (both hemispheres) than similar language tasks (left hemisphere) Parsons (2006).

<sup>8</sup> Author’s translation: *In black Africa it is music that completes the speech act by transforming it into a verb, this superior invention of man makes him a divinely creative force.*

effective people know it because they experience the *mokondi* spirits, whether they are embodied or not.

### 13.6 Some considerations for theories of the origins of language

The Mbendjele are particularly interesting because they use so many different techniques to communicate; some are intended to be understood, others to deceive or avoid being understood, and some to be noticed only by the intended recipients. The range of symbols they exchange in these endeavors is also illuminating. From bent-over saplings, whistle, and hand signals to animal sounds, singing, and lexical exchange, Mbendjele use whatever communicative mode is most effective for achieving their aims. In lexical exchanges meanings can be layered by the combination of words, sounds, and signs, words may be intended to mean one thing to some listeners and something else to others, and comprehensibility is controlled by the degree to which consonants are dropped. Mbendjee speakers are able to communicate with each other, their neighbors, forest animals, and the whole forest.

Mbendjele communicative practices are “good to think with” in imagining scenarios for the origins of language not because they are primitive—they are clearly modern—but because of the extent to which human communicative potential is explored in these diverse practices and what this implies. Their diversity seems to be largely accounted for by the Mbendjele’s inventiveness, their mimetic interaction with their dense forest environment, and their efficacy as hunters of a wide range of animals, including large, dangerous mammals. The Mbendjele’s unconventional predation of vocabulary and communicative techniques from all around them challenges our Saussurian bias of presuming that the links between sounds and meanings in any language should be arbitrary. The evidence presented here suggests that the human ability to mimic these various phenomena “out of context” may be more revealing in relation to the question of the origins of language than an exclusive focus on the relations between arbitrary signifiers.

Brent Berlin’s work on the role of onomatopoeia and sound synesthesia (phonoesthesia) in determining suitable names for things (especially 2005, 2006) is relevant here. Berlin suggested that “non-arbitrary



sound-symbolic, phono-mimetic reference must have had enormous adaptive significance for our hominid ancestors... [and] that the intuitively plausible and metaphorically motivated principles of phonoaesthesia served to drive lexicon in general” (2006: 49). This work suggests that animal names, for instance, often reflect inherent properties of the animals concerned—whether these are sounds they make, their size, or how they move—and that the arbitrary link between an entity and its name so central to much linguistic theory is not a universal feature of language. Additionally, Ramachandran and Hubbard show that the activity of phonoesthesia so central to intuitive lexicon appears to be based on cross-sensual mimicry—from the senses to movements of the tongue on the palate (2001: 19).

The Mbendjele ethnography demonstrates a broad range of mimetic practices. *Massana* highlights the key role of playful, intentional mimicry and cooperative imitation in human learning, creativity, development, and religious experience. The linguistic manifestation of mimicry’s creativity is exemplified in the range of the Mbendjele’s communicative utterances and actions. In the context of conflict, Mbendjele women use mimicry very effectively to humble antagonists and reinstate social harmony. This range of mimetic practice is suggestive of ways that mimicry can drive lexicon, and how it might have contributed to the early establishment of what Knight (2006, this volume) has termed “the rule of law.” For arbitrary signs to communicate meaning, people must agree to adopt linguistic conventions and categorizations, and play the language game honestly—to share sociality.<sup>9</sup> Mimicry out of context may have had a key role in both lexicon development and enforcing the “rules of the social game.”

Mbendjele language freely interchanges vocal and visual signs and symbols ranging from full iconicity to total arbitrariness. These signs and symbols are copied or mimicked from fellow Mbendjele, plants, animals, other people’s languages, or the forest soundscape, and are recombined regardless of context according to what will most effectively achieve particular goals. As Knight (2000: 100) argued, citing Givón (1985: 214), we should treat iconicity as “the truly general case in the coding, representation and communication of experience,” arbitrarily constructed words being an “extreme case on the iconic scale.” Mbendjee provides a good example of this.

<sup>9</sup> See Luc Steels, “Is sociality a prerequisite for the emergence of language?,” ch. 3 in the companion volume to this book, *The Prehistory of Language*.

Knight emphasizes life-long play as key to the evolution of language because it encourages symbolic behavior. Play depends on participants doing things “out of context,” agreeing to “pretend” that something is not what it seems—“that nip is not a bite” or “this stick is a horse.” Amongst primates, only humans maintain the ability to play throughout adulthood. This lifelong “pretending” has established a propensity to engage in symbolic communication and language-like activities that has led to, among other things, the arbitrary Saussurian sign (Knight 2000).

“Faking” (intentionally deceptive mimicry) and “pretending” (playful mimicry) are related. This is suggestive of the possibility that early humans’ language-like behavior began with intentionally deceptive mimicry to facilitate hunting success. Thus early language-like behavior might have initially evolved not for in-group communication but for deceiving other species. A secondary in-group use for fake vocalizations could have then emerged in the context of play and other interactions, possibly in early story-telling using sound signatures, for instance.

Humans are unusual primates for being such expert predators, yet our physique is poorly equipped for such activities. No large canines or sharp claws, for example. Of interest when imagining the situation for early human hunters, the Mbendjele demonstrate the advantage of diverse modes of communication to facilitate spear-hunting herd animals and big game—from faking animal vocalizations to signing and whistling, and using speech to plan and organize hunters working in a group. The widely acknowledged status of Pygmies as the master elephant hunters of the Congo Basin suggests that the advantages these techniques give them go beyond the technology at their disposal, which is also available to other local groups. These other groups only exceptionally spear-hunt big game. Instead they depend on Mbendjele and other Pygmies to do it for them.

This suggests that a significant contributor to selection for language-like behavior was that it gave survival advantages to hominids that depended on hunting and specialized in big-game hunting. Understanding the intentions of both fellow hunters and prey animals, particularly when they are large, intelligent, and dangerous animals such as elephants, offers a clear evolutionary advantage if one’s survival depends on killing them.

As the ability to anticipate and to judge intentions develops through natural selection for big-game hunting success, so the use of mimicry transforms from one of incidental imitation to one of intentional manipulation as hunters increasingly appreciate the intentions behind the

sounds and actions of animals and use these to hunt them more efficiently. In particular they begin to fake animal vocalizations to call prey to them, use animal sounds or plant signs to communicate secretly with one another and more easily outwit prey when group-hunting, or use gesture and other non-verbal abbreviations to communicate silently in the presence of prey. It is a small step to use these same vocalizations and gestures to communicate within the group—for instance in describing a significant event or explaining an accident when back at camp.

If this were so, it might be plausible to suppose that language evolution accelerated at times when hunting was particularly difficult and survival precarious. The Penultimate Glacial between 190,000 and 130,000 years ago, when emerging modern humans were competing intensively for resources in the few habitable refuges left in Africa, would certainly have been such a time. Similar pressures would have existed during the Last Glacial (74,000 to 12,000 years ago), most particularly when it began and modern humans still inhabited Africa. The onset of ice ages must have been times of particularly severe selective pressures on hunters. Hunting difficulty may have accelerated selection for the key skills (reading intentions, environmental mimicry, and faking animal vocalizations) that promoted the use of increasingly arbitrarily coded signs in communication between people.

Based on McBrearty and Brooks' (2000) examination of the archaeological evidence from Africa for the emergence of what he calls "human cognitive fluidity," Mithen (2007: 117) postulates the slow emergence of language from 200,000 years ago to the dominance of compositional language in human communication around 70,000 years ago in Africa. This accords with what would be expected if hunting, mimicry, faking, and play had a crucial role in the evolution of language.

# 14 Sexual selection models for the emergence of symbolic communication: why they should be reversed

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CAMILLA POWER

## 14.1 Introduction

This chapter presents a model for the emergence of symbolic culture, focusing on costly performance in symbolism—ritual, body art, ornament, dance—rather than less costly aspects such as language. Following Knight (1998, 1999) and concurring with Watts (this volume), I argue that ritual and language necessarily co-evolved. Evidence for the presence of one can be regarded as evidence for the presence of the other. The argument in a nutshell is that language cannot evolve without cooperation between strangers; the only medium for securing such cooperation within and between groups is costly ritual (cf. Durkheim 1947 [1915]; Knight et al. 1995; Irons 2001; Sosis 2003).

A scientific model should be framed within a coherent and well-trying body of theory; it should enable us to relate fields of data previously unrelated; above all, it must generate predictions which are testable in the light of appropriate empirical data (Tooby and DeVore 1987). The theory used here is signal evolution theory, the branch of behavioral ecology applied to animal communication. The model investigates individual fitness-maximizing strategies giving rise to symbolic culture and communication. It yields counter-intuitive and testable predictions in a wide range of fields—archeology, paleontology, and hunter-gatherer ethnography. It is the only Darwinian explanation of why red ochre became the cultural species marker of *Homo sapiens* as we emerged in Africa and moved out to the Middle East, Eurasia, and Australia.

## 14.2 Ritual and language

To evolve at all, language requires widespread human prosocial behavior across communities. The puzzle of language is to a great degree the puzzle of human sociality—why are unrelated members of human social groups who only infrequently interact so ready to cooperate with each other? Nowak and Sigmund (1998) model indirect reciprocity, with one stranger helping out another who may never return the benefit, on a basis of securing a good reputation. People who are known to be helpful are more likely to get help. Alexander (1987: 93–106) previously argued that indirect reciprocity rewarded by status and reputation could give rise to moral systems, but he discussed the real difficulty of establishing reputation reliably within Machiavellian groups where interests of individuals and alliances might conflict. Reputation relies on linguistic communication with third parties; we are building a house of cards if we found our models of cooperation on language itself—cheaply produced and volitionally controlled signals that are so easily manipulated.

By contrast, Gintis et al. (2001) follow Zahavi's (1995) idea of altruism as a handicap, modeling cooperative behavior as a costly signal of underlying quality. Such behavior will evolve provided that there is an indexical link between group-beneficial signaling and underlying qualities of the signaler that would benefit potential mates or allies. Prosocial signaling relies on public or audience effects—not reputation—to spread. For example, sharing of difficult-to-acquire food (Smith and Bliege Bird 2000; Sosis 2000) will attract an audience and be broadcast more efficiently than, say, willingness to punish non-cooperators. The fundamental mechanism for such public display is ritual; Irons (2001) and Sosis (2003) propose ritual as in itself costly display of commitment to one's group, its costliness acting as deterrent to freeriders. Initiation ritual among hunter-gatherers may be viewed as a "tax," a requirement of publicly displayed commitment to a group before any social or reproductive benefits accrue to an individual. Knight et al. (1995; Knight 1998, 1999) argue for an intimate co-evolution of ritual and language, as costly ritual displays seal the boundaries of the community within which low-cost linguistic signals can be trusted. Although he did not offer an adequate evolutionary account, social anthropologist Roy Rappaport (1999) similarly argued that the establishment of convention is intrinsic to ritual. Without ritual, no social contract, moral order, or language could exist.

The need for ritual can be highlighted by considering the flaw in one of the more popular recent hypotheses for language evolution—Dunbar’s vocal grooming and gossip model (Dunbar 1996, and see *The Prehistory of Language*). In this model, language in the form of “gossip” emerged as a vital mechanism for bonding early human groups. Larger brains reflect the increasing size and complexity of these groups (Aiello and Dunbar 1993). Vocal grooming (without meaningful content) in the first place and, ultimately, gossip (exchange of meaningful social information) offered alternative means of servicing such extensive social networks, because they saved valuable time compared with the traditional primate means of manual grooming.

This “time-saving” argument leads to a contradiction for the gossip hypothesis of language origins, however. As our ancestors maximized brain size in response to the pressure for larger groups, they maximized their machiavellian intelligence. Byrne and Corp (2004) demonstrate that neocortex size in primates predicts the rate of deception. Humans appear to be selected for a capacity involving both social cooperation and alliance formation, but also manipulation and exploitation of their relationships. We cannot consider gossip as a mechanism of social bonding without factoring in this machiavellian aspect of manipulating information for selfish purposes (Kemmerer 1997; Gluckman 1963: 310). In the case of primate grooming, time becomes a currency (Byrne 1995: 200–202). Time spent grooming an ally reliably quantifies an individual’s commitment to that ally. Correspondingly, if vocal grooming and gossip mechanisms led to a reduction in time spent grooming per individual groomed, this implies a reduction in the commitment signaled to each individual (not necessarily equally distributed). Hence, while *H. heidelbergensis* had larger numbers of allies than any previous hominin, those more numerous allies would have been less intrinsically trustworthy.

This undermining of trustworthiness in relationships throws into doubt all claims that social information can be simply and reliably exchanged; that reputations will be discussed in objective and politically neutral ways; that freeriders can be effectively suppressed. Gossip need not reinforce bonds within groups but is just as likely to exacerbate dissent and conflict between cliques and alliances.

This anomaly led me (1998, 2000) to argue that for gossip to function as a means of social bonding, it necessarily co-evolved with another independent mechanism for establishing commitment to alliances. Raising the

threshold of costs to those who aim to join coalitions safeguards against exploitation by freeriders—those who accept benefits of social cooperation without paying the costs (Enquist and Leimar 1993). Costly ritual performance is the way to secure emotional commitment to long-term alliances (Maynard Smith and Szathmáry 1995: 272–273; Alcorta and Sosis 2005).

### 14.3 Costly signaling and symbolism

The perspective of behavioral ecology and signal evolution is especially appropriate for application to symbolism because of the focus on costs. From a Darwinian standpoint, the most salient and puzzling feature of ritual, religion, or art is its costliness. Chase (1999: 36) describes symbolic culture as constructing “an amazing repertoire of ‘things’ that have no existence outside a symbolic context, things that depend on symbolism for their very existence.” The costs of engagement with the symbolic domain can be counted firstly as material costs of time and energy expenditure; and secondly as cognitive costs: “things that don’t exist” are liable to distract or mislead individuals in real-world competition for mates or resources. It is unclear why there should be advantages to individuals who do engage with tricksters, spirits of the dead, and similar intangibles. In a case of typical hunter-gatherer ritual activity such as Ju’hoan trance dance (Katz et al. 1997), healers who have undergone years of training endure great stress and pain on entering trance; the people of the camp invest heavily along with them in energetic clapping and singing through the night. Around the entire experience are woven ideologies elaborated in narratives or rock art in various Bushman groups (Lewis-Williams 1981).

A model for the evolutionary emergence of such costly engagement with the symbolic realm must answer the following: What selection pressures promoted an interest in sharing and propagating conspecifics’ illusions? Why would it benefit individuals to share in the unverifiable fantasies of others, rather than develop resistance to what Dawkins (1993) would describe as “parasitic” memes? What drove hominins to expend increasing time and energy on “things that don’t exist”—fictions entertained by whole groups of individuals?

Recently, evolutionary anthropologists have started to apply costly signaling theory to human symbolic behavior (Bliege Bird and Smith

2005). Zahavi and Zahavi (1997: 40) contrast the increased efficiency and cost-cutting tendency of “utilitarian” selection with the extravagant and apparently wasteful trajectory characteristic of “signal” selection. The handicap principle argues that signals evolve because a receiver has an interest in assessing the quality of a signaler, and the signaler benefits from being assessed by the receiver. Despite conflicting interests, if the assessment is accurate both parties may avoid unnecessary energy expenditure. Even between predator and prey species, it pays a wolf to gauge accurately which is the easiest gazelle to chase; while it pays a gazelle which is not the slowest to signal its quality to the wolf. Yet this communication can only work if signals are honest. Signals will evolve to ensure reliability through costliness. Where signals are “hard to fake” indices of quality, an individual of lesser quality will not be able to invest sufficiently in the “handicap.” With inbuilt honesty through cost, signals reliably demonstrate the quality of the signaler.

#### 14.4 Sexual selection models for elaborate signaling

Zahavi incorporated interactions between prey and predator or parents and offspring within signal selection in addition to sexually selected display. However, when considering the evolution of elaborate signaling systems among hominins, the most promising arena is communicative interaction between the sexes. With both conflict and cooperation endemic in complex multi-male, multi-female social groups, costly signaling between potential mates may especially arise where there is strategic conflict over levels of investment in offspring. Males and females experience different costs and benefits in allocating energy to parental effort (investing in current offspring) as against mating effort (investment for producing future offspring) (Trivers 1972). In the case of human evolution these trade-offs were critically affected by the extraordinary reproductive costs imposed on hominin mothers by encephalization (Foley and Lee 1991; Martin 1996, 1998). In circumstances where both sexes participate in extensive parental care and either sex stands to lose badly from defection by the other, Møller suggests that sexual evolutionary conflict will generate elaborate signal evolution and adapted psychologies—a “breeding ground for extreme abilities of mind-reading” (1997: 44–45).



Discussing the earliest contexts for symbolic communication, Deacon (1997: 379–380) asks what selection pressures would favor such a “radical shift in communicative strategy” among hominins, and considers the general circumstances which “produce significant evolutionary changes in communication in other species . . . the context of intense sexual selection” (ibid.). Huxley (1914) coined the term “ritualization” for the process by which movements that serve as signals are derived from movements that originally served some other function. For example, the preening activity of grebes, through formalization and stereotypical exaggeration, develops into elaborate courtship sequences. Once displaced from its original function, the ritualized behavior is only useful for communicative display. In his account of the emergence of symbolic communication, Deacon (1997: 379–384) invokes a sexual selection process of ritualization arising from the problem of maintaining pair-bonds in complex evolving human groups. This he associates with the emergence of genus *Homo* upwards of 2 million years ago.

I agree fully with Deacon that we need a process of ritualization involving signaling between the sexes to account for the onset of symbolic communication. But the time frame argued here is different, coinciding with the later stage of encephalization in the past half million years culminating in the emergence of *H. sapiens*. How can a ritualization process motored by sexual selection pressures be related to cultural experiences such as, for example, Khoisan trance incorporating symbolic worlds?

Ritualization is fundamentally driven by the observer—an individual interested in evaluating the behavior of some other individual, whether that be prey, offspring, a mate, or a rival. The observer tries to detect some cue anticipating the likely actions of the animal observed, perhaps some movement that conveys information without being performed for that reason. This Krebs and Dawkins (1984) term “mind reading.” Once observers have evolved to pick up certain cues, then it may pay observed individuals to exaggerate those movements to make sure they are seen. At this point, as exaggeration evolves in ways designed to convey information to receptive observers, the movement becomes a signal. There is a cost to the signaler, in that the movement may lose its original intrinsic usefulness, but the benefit comes through energy saved by accurately conveyed information regarding the quality of the signaler. This implies the signal must provide some accurate measure of the quality the observer

is interested in. So, it remains indexical. But, with ritualized movements in this way displaced from original functions, in a sense their “meaning” has changed. What once signified “preening of feathers” in the grebe now signifies courtship. Even though real information is being conveyed about intentions, at a certain level the “meaning” of the movement is deceptive or illusory. Receiver and signaler share mutual benefit in engaging with and probing behind that “illusion,” for instance that apparent feather-preening is really a courtship display.

In processes of ritualization, therefore, we find some collusion between a signaler and receiver that one thing X (directly perceptible) stands for something else Y (intangible or inferred)—a process that mirrors the key aspect of symbolism, displaced reference. Ritualization in non-humans involves signals between individual signalers and receivers; rarely, if ever, is such collusion in the significance of display shared among a collective as in human ritual. Furthermore, among non-humans we find no references to another world; all signals are necessarily indexical, intrinsically linked to the real world in ways that indicate the quality of the signaler. But something resembling human ritual could emerge through ritualization involving coalitions of signalers in costly display, who engage collectively in some illusion—“things that don’t exist” outside a symbolic context. This would yield an evolutionary model for the emergence of the symbolic domain through a process driven by observers probing the display for signals of quality.

### 14.5 Ritualization models for language and culture

Alexander (1989) proposes the major pressure to increasing group size—driving encephalization—is the threat posed by other human groups. Group on group competition should intensify cooperation within groups and ritualized communication between them. Boehm (2001) argues that where behavioral competition within groups is effectively suppressed, then selective forces operating between groups may come into play enabling multi-level selection. But this depends on overcoming disruption at a lower level. For hominin evolution, it implies a tendency to reproductive egalitarianism. For symbolic communication to be spread through cultural group selection, disruptive forces of sexual competition have to be defused.

I set out from the premise that each sex pursues differential strategies of investment in offspring, giving rise to conflict both within and between the sexes. This gives two basic possible scenarios for a ritualization process that could yield symbolic culture. The first is of male coalitions performing ritualized displays of quality to probing females, in competition with rival males. This follows the standard animal model of male–male competition and female choice in sexually selected display. The reverse possibility invokes female–female competition and male choice. In this, female coalitions display their quality to attract probing males in competition with rival female coalitions. Perhaps some combination of both models could be considered, but here I will explore each in turn, starting with the standard model.

A recent crop of sexual selection models of cultural and linguistic evolution have focused on males as costly signalers displaying to attract choosy females. The evolutionary psychologist Geoff Miller (1999, 2000) proposes that culture, language, and art evolved as an array of sexually selected displays. In this model, the expansion of the brain was motored by selection for these costly demonstrations of creative ability, which were wasteful because they had little to do with survival. All cultural forms could be seen as reliable fitness indicators of quality, where the quality being sought by mates was (and is) creative intelligence. Miller does concede that, with humans of both sexes being high parental investors, they appear to have mutual mate choice (2000: 94–98). Males have been just as choosy as females when it comes to long-term partnerships, implying that males have chosen females for cultural display just as much, producing equality in general intelligence rather than dimorphism. Nevertheless, when Miller tries to test his theory, he reverts to standard sexual selection formulations. Because males have greater reproductive variance, he argues, they will produce the majority of cultural displays. He demonstrates this by quantifying age/sex differences in production of jazz albums, paintings, or literature (1999: 81–87).

Generally Miller's model can account for protean novelty, but is less able to deal with conventionalization. How could purely individualistic sexually competitive display produce the arbitrary and collective rules characteristic of all human cultures? When Miller's idea is applied to the evolution of language, courtship display of verbal ability offers a powerful way of exercising playful linguistic inventiveness. Whereas gossip needs to be believed to have force, jokes, stories, and rhymes do not need to be

“true” to serve the purpose of showing off the inventiveness of their creator. The problem for Miller is that, courtship apart, language is used for so much else where truth and deception may have serious consequences for fitness (Knight 1998; Power 2000). Courtship could pre-adapt for language, selecting protean ability, and it might exapt syntactical language once that has emerged, but alone, it will not generate language as an arbitrary, conventionally agreed code for conveying propositional information.

If Miller’s individualistic mate-choice model could be reconciled with a machiavellian intelligence model of individual members of ritual coalitions showing off their alliances, it might gain in explanatory power. If we as a species have been selected above all for machiavellian intelligence, that is, abilities of negotiating the social world, then it certainly makes sense to advertise those qualities to potential mates. In that case, highly standardized initiation rituals which cement alliances of kin groups or age sets, even where conducted in secrecy and seclusion, could function as sexually selected cultural displays giving reliable information to prospective mates.

Others who have extended Miller’s model in arguing specifically about evolution of language are Burling (2005), and Locke and Bogin (2006). The latter have the interesting idea of viewing the problem of language evolution through the framework of evolution of human life history, especially the emergence of adolescence. Again, their sexual selection model is standard, with language evolution motored by adolescent males displaying quality in the manner of rap and hip hop. Meanwhile, young females are reduced to, if not silence, short, sharp comments judging male displays. Locke and Bogin do consider some element of ritual collective, citing contexts of male initiation, although the ethnographic record reveals at least as much costly female initiation ritual as male (Power 2001). Like Miller, they do not address how a model based in individualistic sexual competition can promote the conventional and institutional aspects of language.

In the above models, even where some interactive process of mate choice is discussed, selection on males is viewed as the driver. None of the models offers specific predictions about the costly signaling systems that arise with symbolism. Here, I will run a thought experiment: Could a process of ritualization involving hominin male coalitions driven by observing females—and male rivals—produce symbolism? Can we identify specific signals that should characterize such early ritualized coalitions?

Bliege Bird and Smith (2005) argue that to apply costly signal models to ethnographic cases it is necessary to show: 1) the underlying quality being signaled; 2) the benefit to an observer of accurate assessment of the quality; 3) the benefit to the signaler; and 4) quality-dependence of signals such that higher-quality individuals pay lower marginal costs.

What qualities would individual members of a male ritual coalition advertise of interest to females and rival males? Presumably, they would display reliable indicators of size, strength, numbers of the coalition, quality and quantity of weaponry or tools, and some signal of "morale" through uniform movements like dancing. Females would want to know that potential mates have plenty of allies, skills for producing food, and ability to protect them and their offspring. Rival males would want to know about strength of alliances and fighting abilities. The problem is that these qualities will have to be displayed in "hard to fake" terms, that is in ways corresponding to perceptible reality guaranteeing quality-dependence. Suppose some males tried to display pebbles as tokens which the coalition had agreed "stood for" handaxes. Or suppose one male set a stick into the ground as a sign for his brother who was coming over the hill in half an hour. Even if observing females were interested in such creative tricks, rival males would not be impressed by these tokens. Rather they would scent weakness and force a fight. Very probably singing and dancing would be media for male coalitionary signaling but this really amounts to energetic activity indexical of real fitness. Nothing promises to drive male coalitions to share in illusions about a world counter to perceptible reality. Under pressure of competition with other males, they simply cannot afford to depart from the real world.

Given this constraint, what types of signals might males use, in coalitions, to show quality, i.e. that they were healthy, fertile, and available as mates? It is possible they might use blood from wounds, lacerations, or scars as a handicap to demonstrate "hardness." But, to demonstrate that quality, real blood must flow. While females might be capriciously attracted to cosmetic signals, again the male competition would only respect the real thing; "warpaint" would not do (but see the finding of Hill and Barton (2005) that red coloring may offer a psychological edge in sporting competition).

In terms of sexual maturity and availability, male coalitions are predicted to display readiness for sex rather than any form of taboo or restriction on sex. Displays should highlight male sexual characteristics

rather than mask or invert these. It is easier to predict what male coalitions should not do: They should not use cosmetics as a substitute for real wounds; they should not signal restriction on sexual activity; they should not signal themselves as the opposite or “wrong” sex.

It seems entirely plausible that male *H. erectus* would have displayed alliances in ritualized coalitions on occasions threatening group-on-group conflict. But in those contexts, signals would have been limited to real-world, indexical rather than symbolic communication. Instead, I will explore the alternative possibility of sexually selected female coalitionary display.

#### 14.6 Female cosmetic coalitions: reverse sexual selection

There is a strong theoretical justification for this reversal which lies in the reproductive requirements of evolving human females. As females of *Homo heidelbergensis*, *H. neanderthalensis* and the direct ancestors of modern *H. sapiens* came under increasing selection pressure for encephalization within the past 500,000 years (Kappelman 1996; Ruff et al. 1997; De Miguel and Henneberg 2001; Rightmire 2004), they needed more energy to support their larger-brained offspring. During this period of accelerated encephalization, somehow more energy was found by mothers. Part of this extra energy may have come from improved dietary range with use of cooking fire, but a large part surely derived from increased male productivity (Kaplan et al. 2000). Females who recruited increased investment from males would have had greater fitness (Marlowe 2001). This implies that females selected males for provisioning abilities. Because females continued to invest heavily in their offspring, standard sexual selection factors of male–male competition and female choice would operate. But as males had to work harder to gain reproductive access, according to theory of parental investment and sexual selection (Trivers 1972), they should become more choosy about which particular females they invest in. As male investment in female partners increased, so too females would compete for access to investing males. Therefore atypical factors of male choice and female–female competition became increasingly prominent in determining variance in female reproductive success (Gowaty 1997; Harcourt 1996: 122; Andersson 1994: 161, 177).

Before considering male criteria for choosing females, it is worth noting that female requirements for productive males suggest males would have competed to display costly signals of quality via “show off” hunting (Hawkes and Bliege Bird 2002). From the perspective of mothers of encephalized offspring, time and energy spent by males on ritualized coalitionary display would be wasted.

Which females did males choose and why? What was the signaling system that arose as females competed to gain investment from choosy males? Why would this be a coalitionary activity rather than individualistic? What qualities were they advertising with costly signal displays?

We can start from exactly the same situation of group-on-group conflict with males competing for access to females. But instead of males prioritizing an offensive strategy to grab females from other male groups, suppose males pursue a defensive strategy, protecting reproductively valuable female kin. Their sisters can join with mothers and aunts in core female kin coalitions. Males as defenders do not permit access to their sisters except at a price: exploitation of muscle power and labour of outsider males. In that case, what signals will evolve?

Here, the observers are outsider males interested in fertile, reproductively valuable females. This suggests that among a group including pregnant and lactating females, males will primarily target cycling ones. Menstruation is a reliable indicator that a female is not now pregnant but is likely to be fertile in the near future. Menstruation did not evolve as a signal, being a by-product of endometrial function (Strassmann 1996). But it is a trait that can be exapted as a signal in the definition of Zahavi and Zahavi (1997: 58): Its value to the signaler is that it conveys information to signal receivers, in this case information about fertility. Pleistocene males who were attentive to recently menstruating females in an effort to improve mating prospects should have enhanced fitness. No male could afford to ignore that information. Individual hominin females would respond to such interest by advertising their imminent fertility to local males in order to promote mating effort. Once females themselves draw attention to it, menstruation does indeed become a signal (cf. Zahavi and Zahavi 1997: 67). Menstrual blood as a reliable index of fertility has a material value translatable into energy in the form of male provisioning.

For her pregnant and lactating neighbors, an individual female who uses menstrual blood to gain economic advantage from males is a threat capable of diverting male investment away from them. In the period of

accelerated encephalization, as female *H. heidelbergensis* began to experience increased reproductive stress, non-cycling females would take strategic action, but what? In a natural fertility population, non-cycling females outnumber cycling ones. Non-cycling (pregnant, lactating, or menopausal) females with a cycling female in the vicinity should immediately act to prevent any males from sequestering the imminently fertile female(s) with a protective cordon. One possibility is they might try to hide the menstruant's condition so that males do not know. But given the economic value of the signal, rather than hide it, I predict females would do the opposite: flaunt it. Whenever a coalition member menstruates, the whole coalition joins in advertising and amplifying this valuable signal to attract outsider males and their labor to the coalition (Plates 6–8).

I call this strategy “sham menstruation” or the Female Cosmetic Coalitions (FCC) model. The strategy succeeds as long as it generates extra male attention and mating effort, which is distributed among the whole coalition, benefiting both non-cycling and cycling females. The strategy is well designed for demonstration of commitment to the alliance. Within any coalition, there is little chance to cheat since each female must clearly show that she foregoes any benefits of trading with philanderer males when cycling before she stands to gain benefits when she is pregnant and lactating. All fertile females alternate between states of cycling and non-cycling. By cooperating in the strategy, a cycling female shows costly commitment to other females in the alliance, securing long-term trust for “gossiping” relationships (cf. Dunbar 1996; Power 1998).

Between female coalitions, a competitive dynamic is expected as they strive to attract available male muscle power. This should drive an evolutionary “arms race” of increasingly elaborate cosmetic advertising, resulting in ritualistic amplification of displays. These could involve use of red pigment to amplify and broadcast the cosmetic menstrual signal, with multimedia effects of movement, song, and dance.

But why would male observers sexually select cosmetically decorated females? Why should males be interested in fake menstruation? Few are likely to be deceived about which females are actually pregnant/lactating and which really menstrual. In the first place, the strategy works to deter would-be philanderer males from targeting menstrual females, and demonstrates the solidarity of the female coalition (with male kin backup). Where then is the honest signal and what is the quality males are interested in assessing? Suppose a young female starts to menstruate, prompting her



kin to stage a ritual. The performance advertises a female of maximum reproductive value and also demonstrates in ways that are “hard to fake” and “easy to judge” the extent of the female’s kin support network. The pubertal female whose kin coalition stages body-paint display is signaling to discriminating males: “Invest in me because I have extensive kin support and my babies will also have it!”

I envisage two stages of this ritualization process, determined fundamentally by degree of reproductive stress on females. The initial situation, during the early phase of brain expansion in *H. heidelbergensis*, would produce context-dependent, ad hoc ritualized displays triggered by incidence of menstruation in the local population. Female coalitions would have used the strategy opportunistically to attract and retain male support, deterring any male philanderers’ attempts to monopolize menstruating females. This implies less planning depth in obtaining cosmetic materials, with greater reliance on biodegradable matter. I predict only sporadic traces of utilized ochre during this phase. As cranial capacity regularly reached modern levels (De Miguel and Henneberg 2001) in the period between 200 and 100 ky, I expect female coalitions with male kin support to have used the cosmetic strategy habitually to motivate outsider male labor and investment whether or not any females were actually menstrual. Greater regularity, planning, and organization of performances would produce abundant and regular use of pigment such as ochre where available. This earliest ritual tradition would institutionalize an economic division of labor and forms of social cooperation both between the sexes and between kin groups. It should permit relaxation of selection pressures for robusticity, especially in females, with reduction of stress experienced by juveniles. It should promote investment in campsites with females and offspring able to stay “home” while male hunters depart on logistic hunts. This second stage of the strategy corresponds to Knight’s original “sex strike” hypothesis (1991). As soon as male mates respond to ritualized signaling by going hunting to get access to female coalition members—effectively performing brideservice—they are actively investing in that female coalition (and their own offspring). Male choice of ritually, cosmetically decorated females should motor the explosive spread of these ritual traditions. These forces of sexual selection would be implicated in the speciation of modern humans from c. 200 ky in Africa. Cultural, artificial secondary sexual signals would mark divergence between modern and archaic forms (cf. Andersson 1994: 46–47, 223, 226).

Whereas in the case of male ritualized coalitions it is unclear how symbolism would arise, it is quite clear in the case of female coalitions. The FCC strategy involves deceptive signals where some females who are not imminently fertile pretend to be. In this case, unlike cases of non-human primate tactical deception, the deception is sociocentric, maintained by a collective, and takes a vital step towards sustaining an imaginary construct and sharing that construct with others—the essence of symbolism.

Where deceptive displays occur only because a local female actually menstruates, these signals, however elaborated, are indexical and embedded in perceptible reality. But it is easy to see how a female coalition would be pushed into signaling shared fictions. Males attracted by cosmetic displays may be reluctant to leave the vicinity; they may instead be inclined to mate-guard imminently fertile females, or even attempt to abduct them. In those circumstances, female coalitions drawing on male kin support would have to respond with louder, clearer signals of resistance. Knight et al. (1995: 84) argue that the way female coalitions would construct their “No” signal is by reversing the settings of the species mate-recognition system. Where female animals in courtship normally display “right species/right sex/right time,” systematic reversal by a defiant female coalition yields “wrong species”—we are animals, not humans; “wrong sex”—we are males, not females; and “wrong time”—we are not fertile right now, but soon we will be. However absurd it seems to turn the world upside down in this way, this is the predicted signature of ritual power, establishing the inviolable, taboo, or sacred state of menstrual or body-painted females. Transmission of such signals counter to perceptible reality and counter to normal relations of dominance (Erdal and Whiten 1994; Boehm 2001) would require energetically expensive, repetitive, iconographic pantomime—high-cost ritual signals sustaining fictitious “gods” (Durkheim 1947 [1915]).

Whereas the need for quality dependence of signals would constrain male ritualized coalitions to refer only to perceptible reality, in the case of female coalitions, representations of counter-reality could be quality dependent. Only a highly committed coalition would be able to perform such an improbable pantomime convincingly. Further, only a high-quality female coalition confident of males returning could risk signaling “No” to outsider males.

Female coalitionary display readily produces a repertoire of shared fictions. The motive for coalitionary action is economic—the need to

mobilize male mating effort. The signaling system delineates a boundary of kinship between groups: Male kin act defensively in support of female relatives who display to outgroup males. This first symbolic strategy is also the first “moral” strategy, since non-cycling females exert collective pressure to obstruct sexual access to cycling females. All the dimensions of economics, kinship, and morality emerge together in the first symbolic ritual performance. Rights to food, rights in sex partners, and allocation to social group of kinship and gender are all now decided collectively, hence signaled digitally and categorically.

Once the rule of law has thus been established through ritual performance (cf. Rappaport 1999), all the barriers to language evolution have effectively been removed. While ritual does not presuppose language and shows continuity with animal display, language requires the trust that only ritual can establish. With ritual in place, the risk of losing one’s reputation established through costly engagement in ritual is likely to far outweigh any transient gain from linguistic deception within the ritual community. In these conditions, the many potential benefits of language use come into play, whether this be through gossip and exchange of social information (Dunbar, *Prehistory*), through competition for status by altruistic provision of relevant information (Dessalles 1998), through access to others’ valuable information (Pinker and Bloom 1990), or through courtship display (Miller 1999).

### 14.7 Predictions and tests

The two possible sexual selection models of male vs. female ritualized coalitions produce diametrically opposed expectations of the signaling system that emerges (Table 14.1). Even if red cosmetic display were favored psychologically in male contests (see Hill and Barton 2005), the prediction here is that such display is necessarily associated with exaggeratedly male characteristics, and certainly not with any reverse sexual signaling.

The FCC model generates a range of predictions across a wide field of disciplines and is potentially testable against evidence from the archaeological record, the fossil record, and, importantly, symbolic evidence drawn from the ethnographic record of hunter-gatherer myth and ritual (Table 14.2).

TABLE 14.1 *Predicted male versus female ritualized signaling.*

Male ritualized display of alliances	Female Cosmetic Coalitions model
Use of real blood but not cosmetics	Use of cosmetics to mimic cycling females
No taboos on sex	Forced to establish taboos on cycling females
Alliance members do not signal as if female	Coalition members (including male kin) signal as “wrong sex” during ritual

TABLE 14.2 *Predictions of the Female Cosmetic Coalitions model.*

## Archaeology:

- a) Earliest evidence of symbolic behavior will be found in a cosmetics industry focused on blood-red pigment.
- b) A two-tier process is predicted. Initially, the strategy will be ad hoc and context-dependent, later becoming habitual and routinized as part of a symbolically structured sexual division of labor characterized by preparation and processing of ritual cosmetics.
- c) The two stages correspond to early campsites vs. later structured hearths, homebases.
- d) In the second stage, coalitions incur high costs to get red pigments where none are locally available.

## Paleontology:

- e) Window of emergence correlates with encephalization rates as index of female reproductive stress. Female cosmetics strategy should not predate *c.* 500,000 BP; it should predate modern levels of cranial capacity *c.* 150,000 BP.
- f) Reduced robusticity is expected, especially in females, with reduced stress in juveniles.
- g) The cosmetics strategy is driven by sexual selection as part of the speciation process, associated with modern morphology in Africa.

## Kinship and male investment:

- h) Matrilineal residence with brideservice is expected as the initial situation.

- i) Principles of classificatory kinship emerge with ingroup/outgroup boundaries; marriage prohibited within clan and between generations.
- j) Hunters observe taboos on consuming their own kills. In Darwinian terms, this equates to “show-off” hunting, whereby men compete to show quality by generous display in “costly” big-game hunting.
- k) Male investment arises via “partible paternity” or mating effort strategies rather than presupposing paternity certainty.

Ethnography of magico-religious symbolism:

- l) Counterdominance generates collective counterreality. We expect the first gods to be represented as WRONG + RED (e.g. wrong species/sex/time).
- m) These collective representations will be transmitted with high fidelity, yielding a time-resistant syntax of ritual power conserved in subsequent traditions including rock art, myth, and hunter-gatherer ethnography.
- n) The WRONG + RED signature will be associated with widespread menstrual taboos.
- o) Hunters’ prohibitions on sex and menstruation will operate within a lunar/menstrual cosmology.

Underlying premises for a behavioral ecology of cosmetics usage:

- p) In natural fertility pre-state populations, men will be interested in tracking menstruation and women will respond by advertising it.
  - q) Costly ornamentation by women will increase with levels of male investment, whether via resources or labor; conversely, where women do the work in both production and reproduction, men will be the most decorated sex.
  - r) Cosmetics function as sexually selected signals through which individuals display their quality as members of coalitionary alliances.
- 

The main archeological prediction—a)—states that the first symbolic evidence will be found in a cosmetics industry focused on red pigments. The evidence for this is discussed in chapters in this volume by Watts and Henshilwood and Dubreuil. While there is no clear prediction about types of ornament that will develop among cosmetically decorated coalitions, jewelry traditions should reveal traces of use with red body



FIG. 14.1 Kua women dance as wrong species (eland), wrong sex (males) at a girl's first menstruation ceremony (Valiente-Noailles 1993).

paint (cf. d'Errico et al. 2005: 16; Bouzouggar et al. 2007; and see d'Errico and Vanhaeren, this volume).

The main paleontological prediction—e)—states that the strategy must emerge in a window corresponding to encephalization rates, between c. 500 and 150 ky, as females respond to levels of reproductive stress. This suggests similar strategies among both sister species descendants of *H. heidelbergensis*—Neanderthals and moderns. In the next section, I consider the implications for Neanderthal culture.

Predictions l) to o) concern the first “gods”—the earliest collective representations—and the “syntax” of ritual power which should be highly conserved in subsequent traditions of rock art, myth, and ritual. Knight and colleagues have developed a body of work testing the ritual syntax in a wide range of cultural contexts. These include Aboriginal Australian ritual and mythology (Knight 1983, 1987, 1991), Amerindian myth (Knight 1997), Khoisan ideology of initiation, hunting and fertility (Power and Watts 1997, 1999; Watts 2005), Sub-Saharan African initiation (Power

2001), European folklore (Cardigos 1996), European and African rock art (Power 2004), and Neolithic monumental architecture (Sims 2006). Although cognitive models of religious constructs (e.g. Boyer 2000; Atran 2002) define some characteristics of the “gods,” no other Darwinian models of magico-religious symbolism have offered such fine-grained descriptions of the original signature of ritual power (see Figure 14.1).

#### 14.8 What about the Neanderthals?

The FCC model leads us to expect a multi-species emergence of symbolic culture (d’Errico 2003; Zilhão 2006), and therefore that Neanderthals, alongside modern humans, should have evolved language. Yet there appear to be significant differences between the Eurasian and African records of pigment use over the past 300,000 years. The major difficulty for the FCC model is to account for this difference.

Watts (this volume) notes only five occurrences of ochre—two questionable—for Middle Pleistocene Europe, pre-dating c. 220,000. There is then a gap in the record until c. 100 ky with two occurrences in the early Late Pleistocene. Most of the pigment finds from Mousterian sites in Europe post-date 60,000 years ago, and red ochre use only becomes habitual in Châtelperronian sites c. 40,000 years ago. Klein (1995) has claimed comparability of the pigment records in Eurasia and Africa. Watts (this volume) disputes this. Archeological research on European Middle Paleolithic sites has been far more intensive than on the African Middle Stone Age, yet the African record of pigment appears much more extensive and continuous over the past 300,000 years.

A possible interpretation of the record is that Neanderthals did develop the first, context-dependent phase of the strategy but did not break through to habitual cosmetic ritual instituting symbolic gender and kinship. Alternatively, they may have achieved the fully symbolic stage relatively late compared to moderns. But why? Arguments about Neanderthal cognitive deficiency are unsatisfying and untestable. More promising are comparative behavioral ecological approaches (cf. Finlayson 2004), focusing on difference in foraging, mobility, seasonal flexibility, and energetics (e.g. Sorenson and Leonard 2001; Weaver and Steudel-Numbers 2005). Owing to their shorter limbs and greater body mass, Neanderthals are estimated to have costs of foraging around 200 kcal/day greater than Early

Upper Paleolithic, Late Upper Paleolithic, or Mesolithic people (Weaver and Steudel-Numbers 2005, table 3). Raised basal metabolic rate and high activity levels as cold adaptations (Stegmann et al. 2002) would have added to Neanderthals' increased daily energy expenditure. Roebroeks and Verpoorte (this volume) argue that the higher energy requirements of Neanderthals relative to moderns can help us explain differences in the archeological record, particularly in use of space, investment in campsites, length of stays, and frequency of moves.

These high energy costs would have impacted most seriously on strategies of Neanderthal females with large-brained offspring. Neanderthal morphology was shaped by the climate during Oxygen Isotope Stage (OIS) 6 (186–128 ky, with glacial maximum 160–130 ky) (Stringer and Gamble 1993: 45–46). They appear adapted to shifting cool temperate to periglacial conditions, showing stocky body form and foreshortened limbs; large noses, large brains, and the robusticity indicating high activity levels may all be cold adaptations (Churchill 1998). But we should remember that Neanderthals survived over a vast climatically differentiated area through climatically unstable times (Schwartz et al. 1999; Finlayson 2004). When operating in the same environments as early moderns, such as the Mousterian Middle East, their use of resources and technologies are barely distinguishable (Lieberman and Shea 1994; Shea 2003) with the probable exception of the early moderns' symbolic repertoire (Hovers et al. 2003) and some evidence for difference in seasonal mobility. Middle Paleolithic faunal assemblages show similar single-species specialization patterns to Middle Stone Age assemblages (Marean and Assefa 1999), though the high degree of Neanderthal skeletal trauma may be attributed to “non-modern” close-encounter hunting (Berger and Trinkaus 1995). Neanderthals appear successful hunters, top carnivores eating protein from large herbivores (Bocherens et al. 2005).

Commenting on Kuhn and Stiner's (2005) discussion of evidence for a gendered division of labour among Neanderthals, MacDonald and Roebroeks (2005: 966–967) suggest that differences of energy requirements, based in differences of body size, must be important for understanding differences in social organization between Middle and Upper Paleolithic humans. In particular, these energy-cost differences, between species and between the sexes, may alter costs and benefits of cooperation between males and females (e.g. Key and Aiello 1999, 2000).



But the raised energy costs of female Neanderthals seem to imply they should have got to symbolism first. Another factor may have altered costs and benefits of male and female reproductive strategies among Neanderthals in ways that differed from African moderns. This is the impact of seasonality of food availability on seasonality of reproduction. Energetic challenge to ovarian function is the best predictor of patterns of birth seasonality in contemporary humans (Ellison 1994: 265–269; Bronson 1995: 147–151). The evolution of genus *Homo* from the Plio-Pleistocene coincides with increasingly seasonal conditions, particularly marked wet/dry season variation in tropical Africa. Periods of dry-season resource stress could be implicated in some seasonality of reproduction. In the Middle Pleistocene, *H. heidelbergensis* expanded into northerly latitudes with variation in day-length and temperature, and oscillation between glacial and interglacial cycles, at the same time that females incurred the significant costs of encephalization. The energetic challenge model suggests this phase of rapid increase in brain size could have featured increasingly seasonal patterns of hominin conception and birth rates, with fine-tuning of ovarian function to metabolic cues. The coldest stages of OIS 6 (160–130 ky) produced the classic Neanderthal morphology. If any female hominins of genus *Homo* ever had markedly seasonal reproduction, it would have been Neanderthals of OIS 6 and OIS 4, the penultimate and last glacial cycles (and see Mussi (2007) for a similar argument).

But why does this affect male strategies? Reproductive synchrony in general among primates, and seasonal synchrony in particular, tends to undermine any single male's ability to monopolize fertile matings (Dunbar 1988: 140–147). By contrast with non-seasonal primates, seasonal species tend to be multi-male, since no one male can mate-guard all females who are fertile during the same period. Power (2001) modeled the payoffs to philanderer and faithful male strategies where hominin females reproduced seasonally and where they reproduced at random. The model showed that strong birth seasonality could promote pair-bond stability since it curtails the benefits to philandering males from extra mating opportunities relative to males who invest to improve infant survivorship.

My argument is that Neanderthals did not need regular ritual traditions because they had more reliable pair-bond stability, especially during cold periods. By contrast, African modern ancestors developed those traditions because their pair-bonding was inherently less stable. They

needed symbolic counter-strategies to resist male philanderers in tropical environments with less marked seasonality of reproduction. This led to an explosion of cosmetics use motored by sexual selection. A “seasonality thermostat” effect could drive changes in male and female strategies, for both Neanderthal and modern ancestors. Where strong birth seasonality was maintained by cold/dry climate and northerly latitude, males with restricted philandering opportunities would be more cooperative in supporting females. A shift in climate towards warm or temperate that allowed females to relax birth seasonality also allowed males to do more philandering. This predicts that the periods when females would be driven to ritual means of countering male philandering would occur in terminal phases of glacial cycles and the relatively warmer periods following. Can this model account for the visible record of ochre use by both species and their forebears in Eurasia and Africa?

The first sporadic use of ochre in Europe clusters between 250 and 220 ky, early in OIS 7, a period of variable climate, but relatively warmer than the preceding glacial; it may be triggered by the termination of OIS 8 at c. 245 ky. In Eurasia the record does not continue after 200 ky, but limited evidence suggests continuity in Africa. Eurasian females are expected to have very marked birth seasonality during OIS 6. In central Africa, despite drier conditions, birth seasonality should not be so extreme, therefore patchy continuity of ritual traditions can be expected. In southern Africa, regularity of ochre use is witnessed with the spread of modern humans (Watts, this volume), coinciding with the Penultimate Glacial. By this time in Africa, ochre has taken off culturally, as part of speciation, and its use is no longer driven by climate shifts. In Europe, a couple of ochre finds c. 100 ky suggest return to the sporadic usage seen over 100,000 years previously in OIS 7. As the climate cools again in Eurasia with OIS 4, Neanderthals still have no regular tradition. With the Châtelperronian, the French Neanderthals did reach for the jewelry and red cosmetics. Whether or not the Châtelperronian is viewed as an independent Neanderthal tradition (d’Errico et al. 1998) or ascribed to cultural contact with moderns (Mellars 2005), the cosmetic signals predicted by the FCC model appear to have worked for the Neanderthals and cultural communication was possible. What remains problematic for the FCC model is the apparent significant use of black manganese by Neanderthals (d’Errico and Soressi 2002).

A “seasonality thermostat” model, combined with effects of cultural transmission, may account for the contrasting Eurasian and African records. Neanderthal mothers pursued the stable pair-bond route, underpinned by their marked reproductive seasonality, occasionally resorting to deceptive sexual signals when seasonality broke down in warmer phases. Modern African foremothers used ritual means to counter our philanderer forefathers and mobilize men’s hunting as mating effort. They did not rely on parental effort but promoted “show-off” strategies. Ultimately, in terms of demographic factors of birth rate, and the social exploitation of the landscape, this proved the most successful strategy.

In comparing the explanatory power of models of cooperative parenting and reproductive conflict in accounts of the sexual division of labor, Bliege Bird remarks: “The puzzling pattern of the human sexual division of labour seems to make more sense as an outcome of conflicts rather than similarities in reproductive goals” (1999: 72). Behavioral ecology suggests that greater or lesser mating opportunities for men may be a key factor underlying variability in the pattern within our species. The same principles can account for the difference between ourselves and the Neanderthals.

## 14.9 Conclusion

This chapter has presented a sexual selection model, the FCC model, for the emergence of symbolic culture, including ritual, body art, and ornament. Language necessarily co-evolved within this religious and moral framework. The model generates predictions testable across several disciplines. It is the only current Darwinian account of why red ochre became the cultural species marker of *Homo sapiens*. Although the strategy is compatible with a multispecies origin of symbolism and language, differences of ecology and energetics between Neanderthals and African moderns may explain why the symbolic cultural records diverged so markedly on each continent.

# 15 Language, ochre, and the rule of law

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CHRIS KNIGHT

The passage from the state of nature to the civil state produces a very remarkable change in man, by substituting justice for instinct in his conduct, and giving his actions the morality they had formerly lacked. Then only, when the voice of duty takes the place of physical impulses and right of appetite, does man, who so far had considered only himself, find that he is forced to act on different principles, and to consult his reason before listening to his inclinations.

Jean-Jacques Rousseau, *The Social Contract* (1973 [1762]: 195)

## 15.1 Digital minds in an analog world?

Language has been described as a “mirror of mind.” Noam Chomsky (1996: 1) attributes this exciting idea to “the first cognitive revolution” inspired by Descartes among others in the seventeenth century. “Cartesian linguistics” (Chomsky 1966) can be dated to 1660, when the Port-Royal grammarians (Arnauld and Lancelot 1972 [1660]: 27) celebrated

this marvelous invention of composing out of twenty-five or thirty sounds that infinite variety of expressions which, whilst having in themselves no likeness to what is in our mind, allow us to disclose to others its whole secret, and to make known to those who cannot penetrate it all that we imagine, and all the various stirrings of our soul.

For Descartes (1991 [1640]: 143), however, language was no mere human contrivance: the “seat of the soul”—the site of its “various stirrings”—was the divinely conferred pineal gland. In Chomsky’s twentieth-century reformulation, the relevant organ becomes “that little part of the left hemisphere that is responsible for the very specific structures of human

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language” (Chomsky in Piatelli-Palmarini 1980: 182). Or as Pinker (1999: 287) puts it: “We have digital minds in an analog world. More accurately, a part of our minds is digital.”

But if “a part of the mind is digital,” how did it ever get to be that way? Under what Darwinian selection pressures and by what conceivable mechanisms might a digital computational module become installed in an otherwise analog brain? Can natural selection acting on an analog computational mechanism transform it incrementally into a digital one? Is such an idea even logically coherent?

If these were easy questions, the origins of language—recently dubbed the “hardest problem in science” (Christiansen and Kirby 2003)—might long ago have been solved. Chomsky (1988: 183) has condemned previous attempts to apply Darwinism as “a complete waste of time, because language is based on an entirely different principle than any animal communication system.” The origin of language, insists Chomsky (2005: 12), was “effectively instantaneous, in a single individual, who was instantly endowed with intellectual capacities far superior to those of others, transmitted to offspring and coming to predominate. . . .” The new faculty was so “perfect” as to suggest the work of a “divine architect” (Chomsky 1996: 30). Needless to say, Chomsky is no creationist. But otherwise supportive colleagues have been puzzled by his suggestion of an apparent miracle—forgetting, perhaps, that his guiding principle has always been internal consistency, not conformity with the rest of science. “In fact,” Chomsky (2005: 12) writes in defence of his suggested scenario, “it is hard to see what account of human evolution would not assume at least this much, in one or another form.” Here as elsewhere (e.g. Hauser et al. 2002; Fitch et al. 2005), Chomsky is informing us that language *as he defines it* cannot gradually have evolved.

Chomsky (2005: 11–12) explains:

An elementary fact about the language faculty is that it is a system of discrete infinity. Any such system is based on a primitive operation that takes  $n$  objects already constructed, and constructs from them a new object: in the simplest case, the set of these  $n$  objects. Call that operation Merge. Either Merge or some equivalent is a minimal requirement. With Merge available, we instantly have an unbounded system of hierarchically structured expressions. The simplest account of the “Great Leap Forward” in the evolution of humans would be that the brain was rewired, perhaps by some slight mutation, to provide the operation Merge, at once laying a core part of the basis for what is found at that dramatic “moment” of human evolution.

As to how a single mutation could possibly produce such effects, Chomsky (1988: 170) invokes “reasons to do with the biology of cells, to be explained in terms of properties of physical mechanisms, now unknown.”

## 15.2 Analog minds in a digital world

One way to escape the conundrums inseparable from Chomsky’s position—conundrums foundational to all our debates and very well documented by Botha (2003)—might be to keep the essential idea but reverse the underlying philosophy. Humans have analog minds in a digital world. More accurately, just a certain part of our world is digital. We resemble our primate cousins in being immersed in material and biological reality—Pinker’s “analog world.” But unlike them, we have woven for ourselves an additional environment that is digital through and through. This second environment that we all inhabit is sometimes called the “cognitive niche in nature.” But the evolutionary psychologists who coined this expression (Tooby and DeVore 1987) did so in pursuit of their own particular agenda. Champions of the 1960s “cognitive revolution” but attempting to marry a reluctant Chomsky to their own mentalist version of Darwin, they were committed to minimizing the intrinsically social, cultural, and institutional nature of the digital semantic representations made available to our brains. The expression “cognitive niche” may have explanatory value, but not if the purpose is to deny the existence of what anthropologists and archaeologists term “symbolic culture” (Chase 1999; Rappaport 1999). Contrary to those who coined the expression, no cognitive niche has ever been found “in nature.” Distinctively human cognition presupposes culture (Deacon 1997; Donald 1991; Tomasello 1999). The “cognitive niche,” to be precise, exists *only as an internal feature* of human symbolic culture.

So what exactly is this thing called “symbolic culture”? Following Searle (1996), let’s begin by distinguishing “brute” facts from “institutional” ones. Birth, sex, and death are facts anyway, irrespective of people’s beliefs about such things. These, then, are brute facts. Legitimacy, marriage, and inheritance are facts only if people believe in them: Suspend the belief and the facts correspondingly dissolve. But although institutional facts rest on human belief, that doesn’t make them mere distortions or hallucinations. Take two five-pound banknotes. Their monetary equivalence to ten

pounds is not merely a subjective belief: It's an objective, indisputable fact. But now imagine a collapse of confidence in the currency. Suddenly, the bits of paper are worthless—the former facts have dissolved.

Where communication is concerned, the choice between brute facts and institutional ones translates into a choice between analog (more-or-less) and digital (either/or) principles of encoding and transmission. This is because unlike brute facts, institutional ones presuppose prior agreement between minds. When confidence in paper money dissolves, the residual brute facts—such as the perceptible color, condition, thickness, and durability of the paper—must by the very nature of things be evaluated on an analog scale. Money is digital—but only when it's money. The phonetic and semantic distinctions of language are likewise characteristically digital (Burling 2005: 23–47), but only when seen “from the inside”—only to those sufficiently acquainted with the language or languages concerned. “All speech is an illusion,” as Pinker (1994: 159) observes.

Institutional facts are not necessarily dependent on verbal language: One can play chess, use an abacus, or change money without words. The relevant digits are then the chess pieces, beads, or coins that function as markers in place of linguistic ones. Digital abstractions of this kind—the intricacies of the global currency system, for example—are patently non-physical and non-biological. They are products of what Searle (1996: 43–48) terms “constitutive rules”—rules that don't simply govern but internally *constitute* the “facts” to which they apply.

Can an objective fact be changed simply by perceiving it in a different way? This apparent magic is possible on one condition: The new perspective must be jointly agreed. Chomsky (2000: 136) comes close to acknowledging this possibility:

I take the thing in front of me to be a desk, but could be convinced that it is a hard bed for a dwarf that I am misusing as a desk; that's a matter of designer's intent and regular use. From one point of view, I take it to be the same thing whatever the answer; from another point of view, a different thing.

Chomsky might at this point have acknowledged that while some facts are “brute,” others are “institutional.” But that line of reasoning would have risked engagement with Rousseau, Marx, Durkheim, and the shifting obscurities of social science, violating Chomsky's Cartesian first principles. For him, it is an article of faith that language is a natural object, its scientific study therefore a branch of natural science. Whether a given

object is really his own desk is an external matter, of no special interest to the linguist. Meanwhile, the available concepts—"desk", "bed", "dwarf", and so forth—are of deep scientific interest, being for Chomsky innate items internal to the "semantic component" of the language faculty as a modular organ (cf. Fodor 1975).

Does this mean that *all* lexical concepts are innate? What about, say, "carburettor" or "bureaucrat"? "For these to be innate," as Putnam (1988: 15) famously objected, "evolution would have had to be able to anticipate all the contingencies of future physical and cultural environments. Obviously it didn't and couldn't do this." To the astonishment of his colleagues, Chomsky replied that "carburettor" and "bureaucrat" were indeed installed in the human mind/brain at the point of speciation. "However surprising the conclusion may be that nature has provided us with an innate stock of concepts, and that the child's task is to discover their labels, the empirical facts appear to leave open few other possibilities" (Chomsky 2000: 65–66).

A less implausible idea is that lexical concepts are historically determined conceptualizations of institutional facts. When people coin a new word—"spam" to mean "bulk e-mail" is a recent example—it becomes established as just such a fact. Whether linguistic or non-linguistic, facts of this kind develop ontogenetically out of the distinctively human capacity for two-way mindreading, joint attention and "let's pretend" (Leslie 1987; Tomasello 1996, 1999, 2003; Tomasello et al. 2005). The underlying formula may be expressed as "*Let X count for us as Y*" (Searle 1996). For "our" joint purposes, is this object a "desk"—or is it a "hard bed for a dwarf"? Chomsky's (2000: 136) humorous question makes sense in these terms—as a choice not between innate lexical concepts but between alternative roles in a game of "let's pretend." Children playing "horse" with a broomstick evidently do have this ability, as do adults imaginatively coining the word "spam" to signify "bulk e-mail."

Of course, it's always possible to term this critical ability "language." This might seem helpful if you view language as an innate mechanism operating independently of the rest of cognition or of any institutional setting. Chomsky does hold this view, as does Pinker. Both view language on the model of echolocation in bats or stereoscopic vision in primates. Distancing himself from Chomsky, however, Pinker concedes that language is designed for communicating thought. He explores how "words and rules" (Pinker 1999) are continuously invented and reinvented for



this purpose. In Searle's terms, the results of this process are "institutional facts." Pinker calls them "inventions." But if they are indeed inventions, Chomsky's foundational assumption must be wrong. Language cannot be understood simply as a biological object. It operates on an entirely different level of organizational complexity from walking or stereoscopic vision—mechanisms which, after all, don't require institutional arrangements in order to work.

What would language amount to in the absence of institutional facts? What meaning would language have to an isolated child deprived of "words and rules"? Chomsky (2002: 148) sees no difficulties here:

Actually you can use language even if you are the only person in the universe with language, and in fact it would even have adaptive advantage. If one person suddenly got the language faculty, that person would have great advantages; the person could think, could articulate to itself its thoughts, could plan, could sharpen, and develop thinking as we do in inner speech, which has a big effect on our lives. Inner speech is most of speech. Almost all the use of language is to oneself, and it can be useful for all kinds of purposes. . . . So if one organism just happens to gain a language capacity, it might have reproductive advantages, enormous ones. And if it happened to proliferate in a further generation, they all would have it.

But in that case, why would people have needed to make audible sounds? Why would language have included what Chomsky (2005) calls "Phonetic Form"? But without that, how could syntax have interfaced between Phonetic Form and Logical Form? After all, there would have been no Phonetic Form. Can language be said to exist when stripped of this interface—when stripped of syntax as Chomsky (2005) himself defines it? Why not just call it "mentalese" or "thought"?

### 15.3 Why symbolic communication cannot naturally evolve

Symbolic culture, in Donald's (1991) evolutionary scenario, emerges in the first instance as "mimesis." In essence, this means "faking it." Irrepressible cries, sobs, smiles, and so forth are not mimesis; their intentionally "acted out" imitations are. To the extent that evolving humans conveyed their thoughts by selecting between intentions in a realm of patent fakes, according to this scenario, their communicative strategies were becoming

symbolic. “Thus semiotics,” as Eco (1976: 7) has explained, “is in principle the discipline studying everything that can be used in order to lie.”

Monkeys and apes reject this kind of thing. It’s not that they can’t transmit cultural traditions—they do that as a matter of course (Whiten and van Schaik 2007). With their “Machiavellian” intelligence, they can also deploy “tactical deceptions” (Byrne and Whiten 1985). These, however, are frequency-dependent: For the occasional trick to work, communication must be generally honest. No monkey or ape positively welcomes being deceived. Symbolism is problematic because it means trading in patent fictions as a matter of course.

Let’s imagine that a mutant ape or hominin did suddenly acquire speech (see Diller and Cann, this volume). Under Darwinian conditions, its combinatorial output would be ignored by conspecifics as completely unreliable. From the perspective of a wild-living primate, each cognitively controlled, intentionally manipulated, and therefore potentially misleading speech-sound would appear from the outset as an obvious fake. Extending up to the highest combinatorial levels, the corresponding messages would seem equally unreliable. Falsehood is in fact central to the very principle of human symbolic communication. During his anthropological fieldwork, Sperber (1975: 3) found himself reasoning: “That’s symbolic. Why? Because it is false.” His Ethiopian informants had recently explained to him, for example, that the local leopards were all Christians. Since this seemed unlikely, Sperber concluded that it must be “symbolism” of some kind. A “symbol” is on this definition a socially accepted fiction—a patent artifice, on one level “false,” yet deployed in such a way or in such a context that others can accept it and share in it. When a fiction is replicated and shared, it loses its fictional status and becomes instead a kind of fact—not a “brute” fact, of course, but a “social” or “institutional” one.

#### 15.4 Why don’t apes talk?

Human minds have been described as “deeply social” in that they mutually “interpenetrate” (Whiten 1999: 177). Intentional states become nested inside one another—“I *think* that you *believe* that I *suppose* that we *understand* that Jane *wants*,” to use Dunbar’s (*Prehistory*) illustration. This kind of mind, then, is intrinsically recursive: mind as represented in other minds, and as it represents to itself such representations. Only

pure intentions—constituents without material substance—could in principle be embedded inside one another in this way. Combining a sob with a cry would not illustrate the principle. Neither would we call it Merge if a chimpanzee happened to combine, say, a bark with a scream (cf. Crockford and Boesch 2005). Merge means combining things, combining the combinations and combining these in turn—in principle, to infinity (Chomsky 2005). Clearly, this can be done only with digital abstractions, not concrete sounds or gestures. A willingness to entertain fictions is, then, a precondition for the emergence of “deep social mind.”

There is a subtle difference between this idea and the theory that distinctively human thought is dependent on fully fledged verbal language. “No support can be found for the view that words are necessary for thought,” writes Bloom (2000) in his exhaustive study of how children learn the meanings of words. But if words are not necessary, in what sense can “language” be said to be necessary for thought?

To appreciate the problem, take the case of pointing. Pointing with the aim of sharing a focus of attention begins spontaneously in children at about 14 months. Chimpanzees never reach this stage. They may beg, reach out, direct behavior, or express themselves in other ways superficially reminiscent of pointing (Pika and Mitani, *Prehistory*; Leavens and Hopkins 1998; Savage-Rumbaugh 1990). Yet as Tomasello (2006: 507) remarks, “there is not a single reliable observation, by any scientist anywhere, of one ape pointing for another.”

Pointing would seem to be a relatively simple activity, not requiring much in terms of computational hardware. But if so, why don't apes point? Is it that Universal Grammar is required—and chimps don't have Universal Grammar? But that idea would surely be absurd: Pointing doesn't depend on any kind of grammar. Tomasello (2006: 520) writes:

To explain human cognitive uniqueness, many theorists invoke language. This contains an element of truth, because only humans use language and it is clearly important to, indeed constitutive of, uniquely human cognition in many ways. However, . . . asking why only humans use language is like asking why only humans build skyscrapers, when the fact is that only humans, among primates, build freestanding shelters at all. And so for my money, at our current level of understanding, asking why apes do not have language may not be our most productive question. A much more productive question, and one that can currently lead us to much more interesting lines of empirical research, is asking the question why apes do not even point.

So why don't apes get that far? Tomasello offers a social explanation. Left to themselves, these animals lack any *motive* to correlate perspectives or share future goals. Apes are incorrigibly competitive Machiavellian strategists. They don't point for the same reason that they don't collaborate on joint projects or commit to sharing out future gains. As Tomasello (2006) puts it, only quite peculiarly cooperative primates motivated to share *goals* and *intentions* could have any reason to point—or any reason to go yet further and invent “words and rules.”

### 15.5 Why language is digital

When fictional representations get stored, abbreviated, and accumulated in publicly retrievable form—as in a repeated and familiar game of “let's pretend”—language has started to evolve. Scaled up from the level of children's games and extended across society as a whole, “let's pretend” may culminate in a community-wide system of ritual, formal kinship, and religion (Durkheim 1947 [1915]; Huizinga 1970 [1949]; Rappaport 1999). The morally authoritative intangibles internal to any such domain are always on some level digital. This has nothing to do with the supposedly digital genetic architecture of the human brain. The explanation is less mystical. It's simply that institutional facts depend on social agreement—and this is not possible on a slippery slope. What would it mean if the Queen in her official capacity were to “open Parliament”—but only “slightly”? Or if a couple who had just made their wedding vows were pronounced man and wife—but only “more or less”?

What applies to royal and religious edicts applies to the production of speech acts in general. Take the classic case of basic color terms. All humans, in all cultures, discriminate perceptually between an immense variety of different hues. But while actual colors can be directly perceived—and while innate biases play a role in determining which regions of the spectrum are picked out—it need hardly be stressed that digital color categories operate on a quite different level. Knowing that the spectrum is segmented into two, three, or some other limited set of “colors”—“the seven colors of the rainbow,” for example—requires access to the relevant institutional conventions. Basic color terms—English “red” and “green,” for example—map directly to these simplified abstractions; they do not and could not possibly map to the vastly more complex

features of the human visual system as such (Davidoff et al. 1999; Steels and Belpaeme 2005; Watts, this volume).

The sound system of a language—its phonology—is prototypically digital. It is no more possible to compromise between the *t* and the *d* of *tin* versus *din* than to compromise between 11:59 and 12.00 on the face of a digital clock (Burling 2005: 23–47). Of course, categorical perception is common enough in nature (Harnad 1987). But the meaningless contrastive phonemes of human language comprise only one digital level out of the two that are essential if meanings are to be conveyed at all.

Digital song format is well documented among songbirds and cetaceans. But combining and recombining phonemes—“phonological syntax,” as it is technically called (Marler 1998)—will remain informationally empty unless it can interface with a second digital level of structure capable of specifying semantic meanings. No non-human species has access to the second level of digital structure. It would therefore be inconceivable and in principle useless anyway for any animal to make use of syntactical operations—whether Merge or anything else—in order to interface between the two digital levels. The explanation is that animals inhabit only their own world and don’t have access to the second level of structure. It is the nature and evolution of the entire second level—that of symbolic culture and its internally authoritative “institutional facts”—that has proved so difficult to explain.

To summarize my argument so far: By definition, anything perceptible can be evaluated and identified through direct sensory input—in other words, on the basis of innate perceptual mechanisms. Being invisible, intangible, and in a fundamental sense unreal, institutional facts can be narrowed down and agreed upon only through a process in which abstract possibilities are successively eliminated. “Discrete infinity” captures the recursive principle involved.

## 15.6 Why animal signaling is analog

Continuity theorists Cheney and Seyfarth (2005) claim that baboons routinely extract “referential, highly structured, propositional, and rule-governed information” from one another’s vocalizations. In baboons as in other primates, however, signal production falls far short of the evident complexities of signal comprehension. These animals monitor vocal and

other evidence of one another's dominance, relatedness, reproductive status, and so forth on an analog scale, much as humans do when we respond intuitively to one another's sobs, sighs, smiles, and so forth (cf. Ekman 1972). Primates in response may arrive at either/or choices—"fight" or "flight," for example—but this doesn't mean that vocalizers are encoding messages in correspondingly digital terms. Because the cues afforded by body language are so rich in finely graded social information, digital processing is precluded—it would miss out precisely the features of interest (Zahavi and Zahavi 1997: 221–223). Baboons don't narrow down interpretative possibilities for the benefit of their listeners. Undoubtedly their vocalizations are richly meaningful—but extraordinarily little of what happens inside any one animal's head is capable of being communicated to the outside world.

When human cooperative norms are being enforced, apes have amply shown that they have the intelligence to make good use of symbols (Savage-Rumbaugh et al. 1998; Sagir and Savage-Rumbaugh, this volume). Intelligence in itself, however, does nothing to enhance the likelihood that symbols will be honestly used. Primate neocortex size predicts rates of tactical deception (Byrne and Corp 2004). In the wild, correspondingly, chimpanzees tend to suspect anything which might be a trick. Despite their intelligence, they "vocalize at low rates, tend not to respond to calls that they hear, and, when they do respond, tend to give calls that are similar to the ones they have heard" (Arcadi 2000).

From a human linguistic standpoint, all this might seem very boring (Dessalles 1998). But then no wild-living ape would enhance its fitness by relying on second-hand information intentionally provided by others as Machiavellian as itself. In such a world, the only safe strategy is to rely on the evidence of one's senses. When a chimpanzee vocalizes, consequently, it must deploy not just its voice but its whole body—whose perceptible size, condition, posture, and so forth comprise variable aspects of one and the same signal (Goodall 1986: 114–145, 313–586). Pant hoots and waa barks are "body language" in this fundamental sense. To produce an impact in a Darwinian world, the signaler cannot deploy cost-free subtleties but must incur a material cost in establishing each and every point. This is the essence of "the handicap principle" governing signal evolution in the animal world (Zahavi and Zahavi 1997). Peacocks illustrate the principle with their extravagant tails, as do modern commercial banks erecting skyscrapers to show off the resources they can afford to waste.

Since 1990, this principle has been generally accepted as a valid way of explaining how honest signaling can evolve in a selfish-gene world (Grafen 1990a, 1990b). Even so, some scholars remain unconvinced. Linguistic signs, such skeptics argue, can easily evolve between sufficiently close kin (Fitch 2004). Some theorists assert that under conditions found frequently in the wild, “the signal is free to take whatever form the signaller wishes,” a situation which “paves the way for an explosion of symbol use” (Scott-Phillips 2008: 280). But if this were true, symbol-use including language would surely be more common than it is. The reason it never evolves at all is that it’s theoretically impossible. In a Darwinian world, no individual can be sure at any given moment whether or not there is some conflict of interest between itself and any particular collaborator, whether related or not. All signals, therefore, must demonstrate intrinsic reliability—a constraint obstructing language-like communication of any kind (Zahavi 1993, 2003).

For signals to be effective, they must advertise some bodily investment (Zahavi and Zahavi 1997). It is true that *relatively* cheap signals may evolve where levels of cooperation are sufficiently high (Krebs and Dawkins 1984; Maynard Smith and Harper 2003). Shared genes, however, are insufficient to ensure language-like evolution, while reciprocal altruism is much rarer in nature than was previously supposed (Hammerstein 2003). When a message is urgent and immune from being immediately checked, the animal’s voice is favored because sounds carry further than visual signals and because receivers need not be within close range. But a price has to be paid: Much as a banknote requires its watermark, a primate vocalization must convince its audience that it cannot be a fake. It is this constraint that prevents vocal communication in particular from coming under cognitive control (Knight 1998, 2000). None of this applies to the chimpanzee manual gesture known as the “directed scratch” (Pika and Mitani, *Prehistory*). If this gesture is cognitively expressive, it is because it lies at the opposite extreme—reference could hardly be less displaced or deceptive signaling more improbable given the context.

Clearly, humans have exploded this entire system of constraints. Words are not just cheap: They sink beneath the floor on any costly signaling scale. Linguistic signs are “honest fakes”—literal irrelevancies and falsehoods, significant only as cues to the communicative intentions underlying them. Regardless of cognitive architecture, no wild-living animal could conceivably give credence to digital abstractions of this insubstantial and unreliable kind.

### 15.7 Only in virtual reality can zero-cost communication evolve

Speech is digital, cheap, and efficient. It costs nothing to switch, say, voicing from the “on” to the “off” position, thereby selecting the consonant “t” as distinct from “d.” This is the system’s immense power: Substituting one zero-cost consonant for another can alter the meaning of an entire sentence, potentially with life or death consequences. If signalers can do this at no cost to themselves, the arrangement would seem to presuppose infinite trust—an obvious impossibility in a Darwinian world. But if that is accepted, how can language in the human case ever have evolved?

The puzzle is difficult, but not quite insoluble. Briefly, institutional guarantees can permit patent fictions to be taken on trust (Knight 2008a). Chess would be unplayable without prior commitment to the rules and corresponding trust in the validity of each signaled move. Capitalism would grind to a halt if everyone had to spend time scratching each coin or holding up each banknote to the light. If short-cuts can be taken, it is because printing one’s own banknotes is a punishable offence. In chess, likewise, neither player can make one move on the board while making another in secret under the table—no concealed movement can even count as a “move.”

“The question ‘What is a word really?’ is analogous to ‘What is a piece in chess?’” (Wittgenstein 1958: 108). Within this philosophical paradigm, to produce a “speech act” (Searle 1969) is to make a move in a virtual world. Each player’s communicative intention is fulfilled not by producing any material impact but simply by securing recognition as the intention it is (cf. Grice 1989: 86–116). Like the “illocutionary force” of a chess move, that of a speech act (Austin 1978 [1955]) is independent of physical effort: A barely perceptible wink might suffice, allowing intuitive mind-reading to search for contextual relevance and fill in any remaining gaps (Sperber and Wilson 1986). Because speakers and listeners are trading in pure intentions and these cost nothing at all, this kind of communication falls outside the scope of Darwinian signal evolution theory of any kind. To adapt Chomsky’s (2002: 148) words, “language is off the chart.”



## 15.8 Human language and animal play

In play contexts, animals love to bite, scratch, and chase—but not “for real.” “Predator” and “prey” playfully exchange roles, signaling their intentions to avoid misunderstandings. Quite unlike primates’ vocal signals, such playful gestures are flexible, creative, and imaginative (Tomasello et al. 1994). A playful nip differs from its aggressive functional counterpart in being cognitive, categorical, and in an important sense “displaced.” Whereas chimpanzee barks and screams, when combined, convey only a compromise blend of meanings, a preliminary “play face” or other invitation to play resembles a linguistic marker in that it may wholly *reverse* the significance of subsequent “chases” or “bites” (Loizos 1967; Pellis and Pellis 1996; Palagi et al. 2004; Paquette 1994).

In these and other respects, what we might regard with hindsight as prefigurations of language are present already in primates. In children, continuity between play and language is even more striking. Language and “let’s pretend” have the same critical period, the same features of intersubjectivity and joint attention, the same triadic (“Do you see what I see?”) referential structure—and the same cognitive expressivity and independence of external stimuli. It is unlikely that these and other parallels (Tables 15.1 and 15.2) are purely coincidental (Bruner et al. 1976; Leslie 1987; McCune-Nicolich and Bruskin 1982). I conclude that the human “language instinct” (Pinker 1994) has its evolutionary roots not in animal vocal communication but in animal “fantasy” and “play” (Bateson 1973 [1955]).

Play prepares young children for an adult life of engagement with symbols. Young apes and monkeys share many of the same instincts (Ragir and Savage-Rumbaugh, *Prehistory*), but in their case no symbolic future can be anticipated; their skills in play-fighting prepare them instead for future challenges in fighting for real. But if play underpins symbolic culture in humans (Huizinga 1970 [1949]), why does it all turn out so differently among our closest primate relatives?

In a primate’s lifetime, play lasts only until sexual competition rears its ugly head. It’s not that sexually mature primates never surrender to their playful instincts: Sometimes they do, mostly with immature relatives but occasionally in relaxed situations among themselves (Pellis and Iwaniuk 2000). But with the onset of sexual maturity, what was formerly a playfight can suddenly turn nasty (Paquette 1994; Pellis and Iwaniuk 1999). The

TABLE 15.1 *Linguistic sign versus primate call.*

Language	Call
Discrete/combinational	Graded/blending
Bidirectional	Unidirectional
Head	Body
Symbolic	Indexical
“Faking it”/unreliable	“Hard-to-fake”/reliable
Digital	Analog
Displaced/imaginative	Here-now/stimulus-bound
Open/productive	Closed/unproductive
Learned signals	Innate signals
Culturally variable	Species-specific
Focus on intentions	Focus on performance

stakes in terms of reproductive success are now too high. Turn-taking stops because no one can afford to lose.

Why is this relevant to the evolution of language? It matters because play is the most fertile arena in which apes produce gestural conventions and associated “symbolic” shorthands (Tomasello et al. 1994). Sexual

TABLE 15.2 *Linguistic signing and social play.*

Language	Social play
“Faking it”	“Faking it”
Depends on trust	Depends on trust
Displaced reference	Displaced reference
Cortically controlled	Cortically controlled
Cognitive	Cognitive
Productive/creative	Productive/creative
Two-way interpersonal	Two-way interpersonal
Turn-taking	Turn-taking
Reducible to shorthands	Reducible to shorthands
Stimulus-free	Stimulus-free
Focus on intentions	Focus on intentions
Critical period	Critical period

conflict obstructs the accumulation of these because it ensures that no playful game ever endures long enough for its shared understandings to be transmitted from one generation to the next (Knight 2002). Somehow, in the course of human evolution, this logic must have been set in reverse. Instead of sex dictating an end to play, play must somehow have begun dictating its distinctive principles to sex (Knight 1999, 2000, 2008).

As the core principle of language, “discrete infinity” corresponds to what developmental psychologists might recognize as an imaginative game—in this case, “let’s play infinite trust.” Take any patent fiction and let’s run with it and see where it leads. Hunter-gatherer systems of “fictive” kinship illustrate this principle: Let my mother’s sister be “my mother,” my sister’s husband “my husband.” When “let’s pretend” is applied to sex and reproduction in this way, “fictive” or “institutional” kinship is the result (Barnard, this volume; Knight 2008b).

An equally valid adjective to describe distinctively human kinship would be “metaphorical.” A metaphor “is, literally, a false statement” (Davidson 1979). But by accepting it and sharing in it, we construct it as “truth” on a higher level—truth for “our own” joint purposes of conceptualization and communication. As gestural fictions become conventionalized and reduced to shorthands, one possible trajectory is that they crystallize out as linguistic signs (Corballis 2002a). Grammatical forms likewise emerge out of historical processes that are now well understood—creative processes whose secret is essentially metaphor (Meillet 1903; Heine et al. 1991; Gentner et al. 2001; Hoefler and Smith 2008; Deutscher 2005).

## 15.9 Why menstruation matters

What does all this tell us about the origins of language? It means that we need to redefine our problem. The task is not to explain how a digital computational module came to be installed in an otherwise analog brain. It is to explain how an evolving primate mind came to inhabit a digital world. How did immature playfulness survive the transition to sexual maturity? What outbreak of public trust allowed adults in their interactions to stop insisting on reliability and celebrate one another’s fictions instead?

The solution I favor is schematically outlined in Plate 6. It involves a topic not usually addressed in debates on the origins of language: primate

and human reproductive signaling, including the curiously neglected subject of menstruation.

What logic can possibly connect menstruation with the origins of language? Since I am often asked this question and since the answer is not immediately obvious, let me approach it via an easier question. What might connect violence with the origins of language? Here, a negative relationship can be intuitively grasped. Imagine a situation in which the route to male reproductive access involved direct physical violence or threat. Under such conditions—familiar enough to primatologists—no community-wide contract could hold. Society would fall apart, “selfish-gene” Darwinism would be the only law—and communicative subtleties such as “illocutionary force” (Austin 1978 [1955]) would be ruled out.

Primate sexual signals are not in themselves violent. However, they regularly trigger violence. Chimpanzee oestrus swellings, for example, typically provoke chaos as males in the vicinity compete for fertile sex. Ovulation in humans has become effectively concealed, leaving menstruation salient as the one remaining external evidence of a female’s (imminent) fertility (Power and Aiello 1997). In extant hunter-gatherer societies, men are not oblivious to the opportunities this presents. The Mbuti “consider that any couple that really wants children should ‘sleep with the moon’” (Turnbull 1993 [1961]: 169). Hadza informants generally view conception as occurring immediately after menstruation (Marlowe 2004). “A woman is considered most fertile during menstruation” writes Lewis (2002: 109) of the Mbendjele. “Sexual intercourse at this time is widely held to be a sure way of ensuring pregnancy begins. Menstrual blood is the symbol par excellence of human fertility.” Adequate statistics are lacking, but on theoretical grounds I would predict that hunter-gatherers everywhere will be found to hold similar views.

Imagine a world in which selfish-gene Darwinism was the only law. Menstruation would then act as a starting gun for sexual conflict. It is precisely in reaction against this danger that hunter-gatherers surround the blood with such elaborate taboos (Knight 1991, 1996). Across Africa, the senior female relatives of any young woman experiencing her first menstrual onset respond to her condition as if to an immediate threat. Mobilizing the entire community, they subject her to strict supervision and control, celebrating her fertility but at the same time bonding tightly

with her and above all *controlling male access to her*. The logic at work here is digital in that it excludes intermediate states—to acknowledge the presence of blood, no matter how small or even imperceptible the actual quantity, suffices to trigger the full collective response.

Contrary to many western misconceptions, menstrual taboos are not necessarily evidence of sexist oppression under patriarchal rule. African hunter-gatherers are not noticeably male dominated. In fact, women typically have much solidarity and power (Turnbull 1993 [1961]; Lewis 2002, this volume). Although cultural variability is great, the ubiquity and evident antiquity of menstrual taboos can be explained on a Darwinian basis—as the outcome of an evolutionarily stable strategy pursued for millennia by women in pursuit of their own reproductive interests. Child-burdened mothers require investment from males, not abandonment, harassment, or violence. Socially approved posture, clothing, scarification, and cosmetics are characteristically human ways of “covering up”—concealing, modifying, and scrambling those signals which, if left biologically naked and exposed, might preclude the possibility of rule-governed behavior of any kind.

#### 15.10 The southern African ochre record

The Female Cosmetic Coalitions (FCC) model (Knight, Power, and Watts 1995; Power 1999, 2001; Power and Aiello 1997; Power, this volume; Watts, this volume) is easily misunderstood. One frequent misunderstanding concerns the significance of red ochre in the archeological record. The model, according to Taylor (1996: 104), “assumes that a man would actually be fooled by red ochre into thinking that a woman had a period when she had not.” Hovers and colleagues (2003: 509) likewise misread the argument as a claim that women during the African Middle Stone Age “faked” menstruation in what amounted to an elaborate “fraud”.

Notions of men being “fooled” or subjected to “fraud” are, needless to say, misleading. FCC does not state that the pigments found at Blombos Cave (Henshilwood and Dubreuil, this volume; Watts, this volume) or Pinnacle Point (Marean et al. 2007) were produced for purposes of fraud. On the contrary, all the evidence suggests that they were used in the Middle Stone Age much as comparable pigments have continued to be

used across much of Africa up to the present day (Watts 1998, 1999). As in the Himba case illustrated here (Plates 7 and 8), red ochre doesn't simply mimic menstruation: It transcends and negates it, signaling group identity and corresponding inviolability by conveying concepts such as "order," "beauty," and "fertility" independently of anyone's actual biological condition. Similar messages are conveyed by the use of red body-paint across much of the world (Knight 1991: 417–448). Given that menstruation is for Darwinian reasons potentially a source of chaos, a sufficiently brilliant red can act as a cultural antidote to that potentially disruptive signal (Power and Aiello 1997). We may recall that the word "cosmetics" in English derives from the Greek *κοσμος*, the plural of which meant "adornment" (Power 2001: 189–190). It conveys the idea of the cosmos as embodying order in opposition to chaos. In the Classical Greek worldview, ordered arrangement underlies both morality and beauty. The cultural anthropologist's term "cosmology" likewise links the human moral and social order with the harmonious ordering of the cosmos (cf. Beidelman 1971: 30).

When female coalitions under the conditions envisaged assert their unity and inviolability, they go beyond amplifying and scrambling their sexual signals: They systematically reverse them (Power and Watts 1997, 1999). The logic here is not difficult to grasp. Male success in philandering entails picking and choosing between females on the basis of their hard-to-fake fertility cues, abandoning each current partner once pregnant in favor of a new mate signaling that she is of the right (same) species, of the right (opposite) sex, and in her right (fertile) period. The reverse on every count would read "*We are all of the wrong species, the wrong sex, and anyway it is the wrong time.*" This is therefore predicted on logical grounds as the signature of coalitionary resistance to male philandering, such collective action taking the form of "counterdominance" (Erdal and Whiten 1994, 1996) culminating in "reverse dominance" (Boehm 1993, 1997, 2001) or "sex strike" (Knight et al. 1995). For theory-internal reasons, each such performance should

- embrace and overturn the social order
- coincide with specific phases of the moon
- be enacted with special intensity whenever a young woman starts to menstruate.

Since these and other predictions are unexpected, detailed, and specific, the FCC model should in principle be easy to disprove.

### 15.11 The Kalahari “Eland Bull Dance”

Extant hunter-gatherer traditions can offer test-data, but not because they represent fossilized survivals from a distant prehistoric past. If cross-cultural uniformities are discernible—if the syntax of hunter-gatherer ritual and myth largely resists historical change (Knight et al. 1995)—it is because the solutions men and women still arrive at today are evolutionarily stable strategies, optimal responses to intellectual and social problems likely to vary only within a limited range.

Ethnographic details are important because they allow fine-grained testing of the FCC model (see also Knight 1997, 2008b; Power 2004; Watts 2005; Power and Watts 1997, 1999). Briefly, marital sex is predicted when ritual power is switched “off.” When it’s switched “on,” we predict sex in reverse. In rock-art, myth, and religious iconography, consequently, we expect ritually potent sex in gender-reversed, species-reversed form. Any past or present hunter-gatherer cosmological system celebrating heterosexual pair-bonding as its core “sacred” representation—as would be predicted, for example, by Deacon’s (1997) contractual monogamy theory of the origins of symbolic culture—would falsify the model.

The “Eland Bull Dance”—climax of a young woman’s first menstruation ceremony—has been described as the most joyful, prominent, and socially unifying event in Kalahari Bushman life (Lewis-Williams 1981). As young people approach maturity, the playful boys-versus-girls games characteristic of earlier years don’t come to a halt but are extended and elaborated, albeit now in adult form. Menstrual onset marks the occasion when, from a Darwinian standpoint, a young woman might seem most at risk of being singled out and fought over by rival males. But in the Kalahari as elsewhere among hunter-gatherers across Africa, the female relatives of the “New Maiden” defend her against any such damaging possibility. An elaborate game of “let’s pretend!”—a performance centered on the young woman herself—announces that her blood is supernaturally potent and that a moral order is now in place. Who needs a man? Singing, clapping, and laughing at the humor of it all, the dancers thrust out their buttocks in imitation of eland cows consorting with their Eland

Bull—a systematically reversed role played by the “New Maiden” herself (Lewis-Williams 1981; Power and Watts 1997; cf. Power this volume, fig. 14.1).

The men during such performances typically keep their distance. As they collude in the elaborate make-believe, it is not that they are being “fooled” or subjected to “fraud”. By concealing ovulation and extending receptivity across the cycle, the evolving human female long ago “fooled” her mate into enjoying sex regardless of whether impregnation was likely to be achieved. Against that background, hunter-gatherer traditions of cosmetic adornment and associated ritual performance can be seen as so many cultural continuations and extensions of this time-honoured female strategy of sexual time-wasting aimed at preventing philandering. Any time-saving information of potential use to a philanderer represents a threat to female interests. Women should therefore withhold such information by all available means (Power and Aiello 1997; Knight 2008b).

Why don't philanderers mount physical resistance to such female strategies? Human males as selfish-gene strategists will weigh up the costs and benefits. Females can favor dominant male philanderers for their “good genes”. Alternatively, they can insist on an investment of time and energy in the form of mating effort. We expect mothers of increasingly encephalized, slow-maturing and correspondingly burdensome babies to increasingly demand time and energy as the price of fertile sex (Plate 6). Female resistance drives up the costs philanderers may expect to incur. While a dominant male might well consider violence, less dominant investor males have every interest in supporting collective action against them, driving up the costs incurred by violators and entering into the spirit of the game (Knight 1999; Power 2001, this volume).

“Wrong sex/wrong species/wrong time” may be a let's pretend game, but it is not “play” in the sense of idle entertainment over and above the more serious demands of life. In the African hunter-gatherer record, gender solidarity as expressed in various forms of sexual teasing and play is fundamentally *constitutive* of life. Lewis (2002: 125) describes how Mbendjele practices that westerners might call “ritual” or “religion” are “woven into daily life so subtly that they are almost invisible.” Life as a whole is relaxed, playful, and in a certain sense “other-worldly.” The word *ekila* links menstruation with the moon, the supernaturally potent blood of game animals, mystical dangers to human reproduction—and ritual taboos governing hunting, conservation, and the maintenance of



egalitarian relationships (Lewis 2008). The word *massana*—“play”—embraces “children playing casually and children’s role-playing games, as well as adult group rituals” (Lewis 2002: 125). Childhood play, then, continues seamlessly into adult life—whereupon it becomes what an outsider might term “ritual” or “religion.” Common to *ekila*, *massana*, and emotionally expressive dance, song, and oratory is community-wide, socially empowering make-believe. Laughter—and especially women’s laughter—is a fundamental leveling mechanism, puncturing the pretensions of those unable to enter into the spirit of the game. It is in a liberated world of this kind that language is at last free to evolve (Lewis, this volume).

### 15.12 A theory of everything?

“Animals,” observed Durkheim (1947 [1915]: 421), “know only one world, the one which they perceive by experience, internal as well as external. Men alone have the faculty of conceiving the ideal, of adding something to the real. Now where does this singular privilege come from?” Maynard Smith and Szathmáry (1995) offered a bold answer to Durkheim’s question, citing Rousseau (1973 [1762]) and viewing the puzzle of language origins as inseparable from the wider problem of explaining the emergence of community-wide social contracts. Their “major transitions” paradigm is ambitious and conceptually unifying, assuming no unbridgeable chasm between natural and social science. The same can be said of the paradigm being developed by Steels and his colleagues (Steels 2006, *Prehistory*; Steels et al. 2002), who use robots to show how lexicons and grammars—patterns far too complex to be installed in advance in each individual brain—self-organize through processes of learning, recruitment, co-ordination, and cumulative grammaticalization. By maintaining continuity with primate cognitive evolution while introducing novel social factors, we can continue to apply basic principles of Darwinian behavioral ecology to account for the emergence of distinctively human cognition and communication.

“It is surely no coincidence,” writes Pinker (1999: 287), “that the species that invented numbers, ranks, kinship terms, life stages, legal and illegal acts, and scientific theories also invented grammatical sentences and regular past tense forms.” Confusing correlation with causation, Pinker attributes the digital distinctions of language, law, and scientific logic to a

digital innate component of the human mind. Note, however, that the items he actually lists—whether linguistic or non-linguistic—are in all cases products of collective agreement. As Pinker himself concedes, they are “inventions.” Is there any evidence that a language faculty could operate at all outside such institutional settings? No evidence has been offered to date. Reversing Chomsky—and correspondingly reversing the whole idea of “digital minds in an analog world”—we may conclude that “doing things with words” (cf. Austin 1978 [1955]) involves more than activating a biological organ. To produce speech acts is to make moves in a non-biological realm—a realm of facts whose existence depends entirely on collective belief.

“An analog mind in a digital world” is fully compatible with Darwinian evolutionary theory. “A digital mind in an analog world” is not compatible at all. Installation of an innate digital mind—whether instantaneous or gradual—is a *deus ex machina* with nothing Darwinian about it. A model of language evolution, to qualify as scientific, cannot invent fundamental axioms as it goes along. It cannot invoke currently unknown physical or other natural laws. It should be framed within a coherent, well-tried body of theory; it should generate predictions that are testable in the light of appropriate empirical data; and it should enable us to relate hitherto unrelated disciplinary fields. Whereas the *deus ex machina* approach rigidly rejects reference to any part of social science, the major transitions/reverse dominance/deep social mind paradigm has the potential to link the natural and social sciences in a theory of everything.

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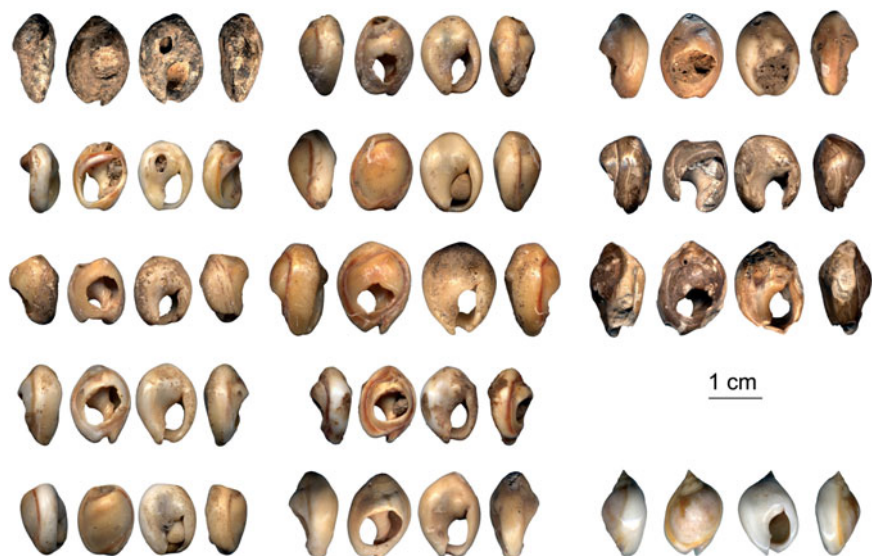


PLATE 1 *Nassarius gibbosulus* shell beads from the Mousterian levels of the Grotte des Pigeons, Taforalt, Morocco, and a present day shore (bottom right). (Modified after Bouzouggar et al. 2007, photo by the authors.)

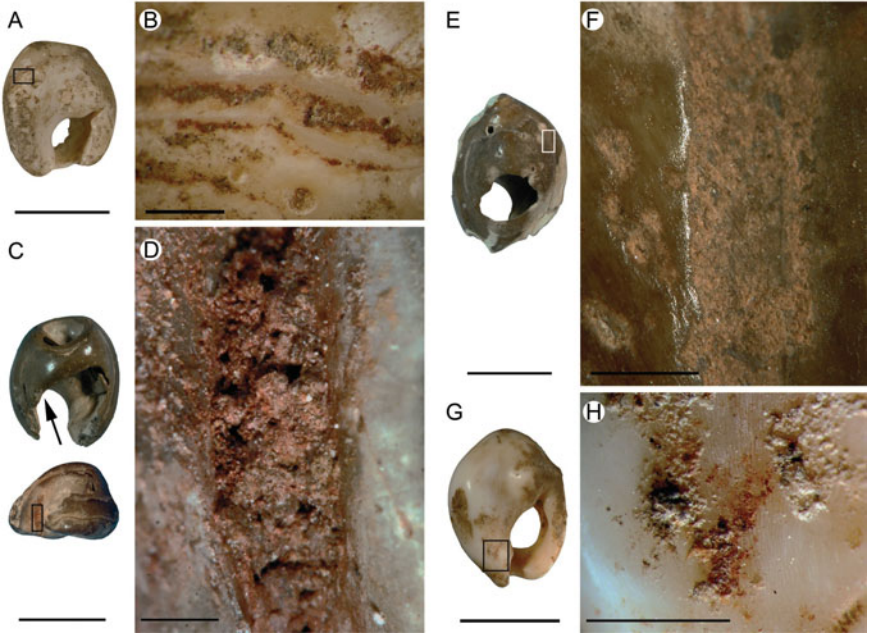
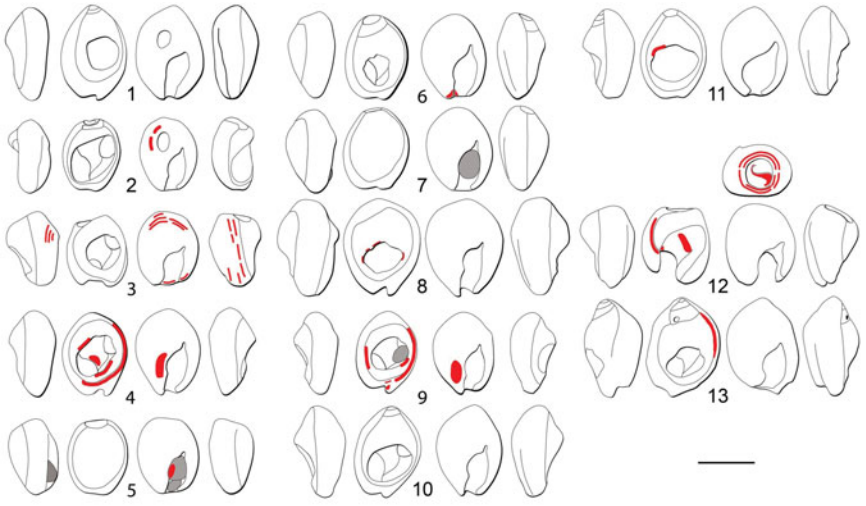


PLATE 2 (Upper) Areas in red identify the location of red pigment residues on *N. gibbosulus* shells from Grotte des Pigeons. (Lower) pigment residues on the ventral side (A and B, no. 3), inside the body whorl (C and D, no. 12), on the dorsal side (E and F, no. 13), and close to the syphonal canal (G and H, no. 9). Black squares in A, C, E, and G and arrow in C identify the area enlarged in the adjacent micrograph. (Scale bar: 1-13, A, C, E, and G 1 cm; B, D, F, and H 500  $\mu$ m). (Modified after Bouzouggar et al. 2007, photo by the authors.)



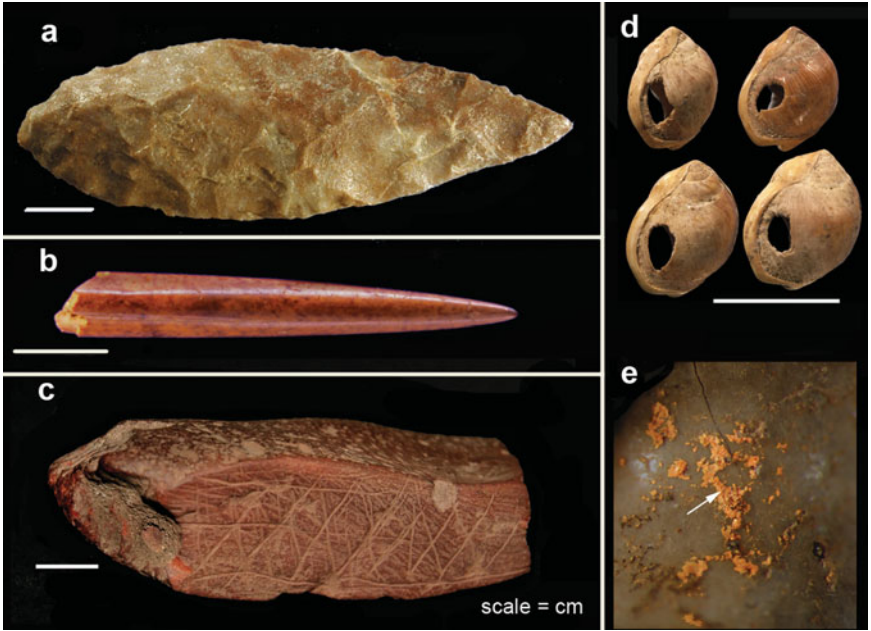


PLATE 3 Artifacts from the Still Bay levels at Blombos Cave: a) silcrete bifacial point b) formal bone tool c) engraved ochre SAM-AA 8938 d) *Nassarius kraussianus* shell beads e) ochre deposit on a shell bead (Images by C. Henshilwood and F. d'Errico)

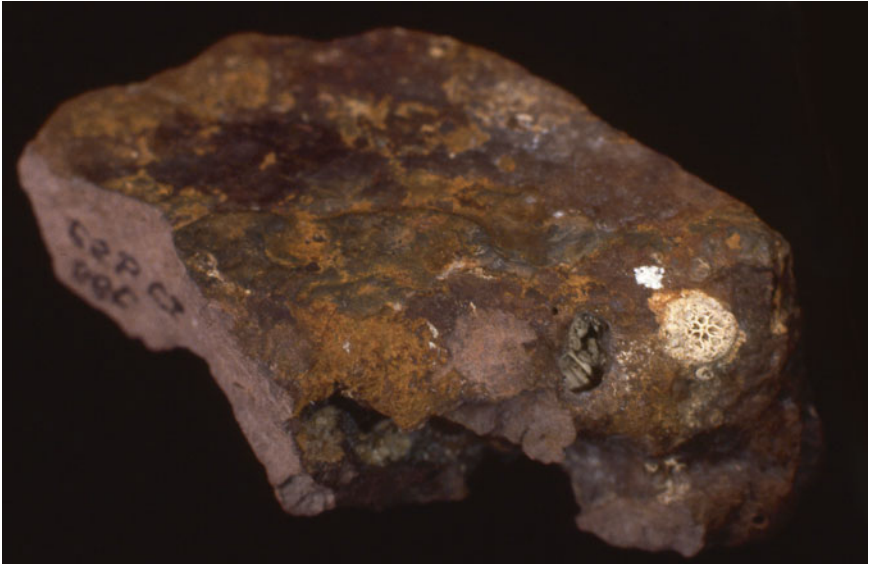


PLATE 4.1 CI Siltstone, 56.3g, 73.9mm length, NCS streak 3545 Y60R. Two adjacent pholadid borings, one with both valves of the pholadid *in-situ*. Unidentified tests of marine organisms attached to adjacent surface.



PLATE 4.2 CF Hematite "crayon". 3.7g, 34.1 mm length, NCS streak 3357 Y80R, 9 facets.



PLATE 4.3 CFC Coarse siltstone "crayon". 0.6g, 17.3 mm length, NCS streak 3257 Y80R, 3 facets.

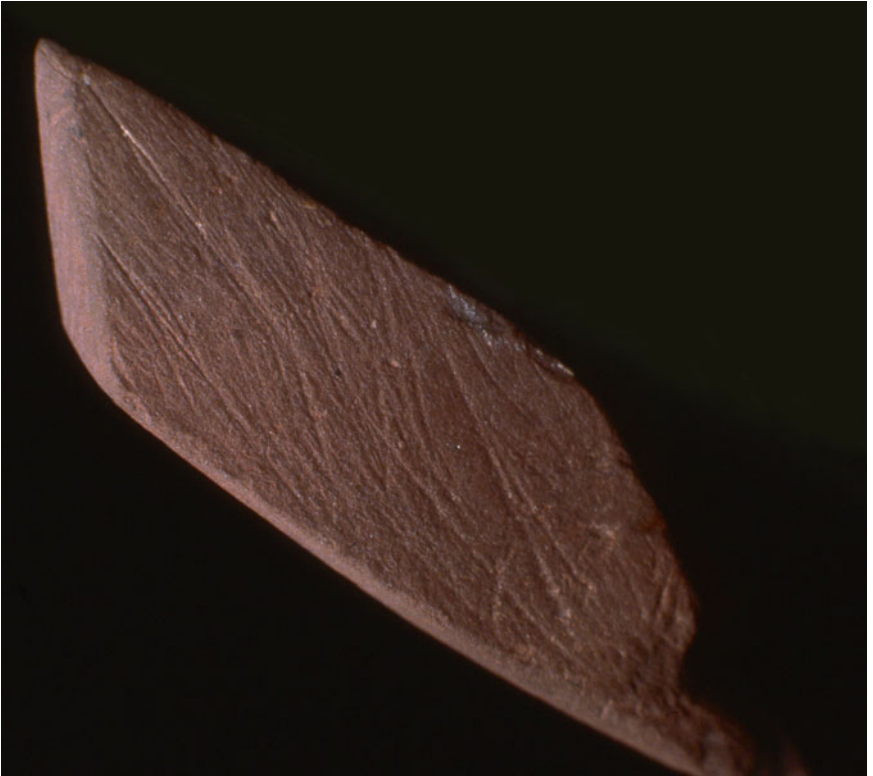


PLATE 4.4 (a and b). CI Siltstone 'crayon'. 3.9g, 41.3 mm length, NCS streak 4040 Y75R, 7 facets.



PLATE 4.5 CB Coarse siltstone, intensively ground. 1g, 16.9 mm length, complete, NCS streak 3550 Y70R, 9 facets.



PLATE 4.6 CF Hematite, 1.5g, 21.6 mm length. NCS streak 5030 Y80R. Ground on part of one main surface and two edges, illustrated facet 3.2 mm wide.

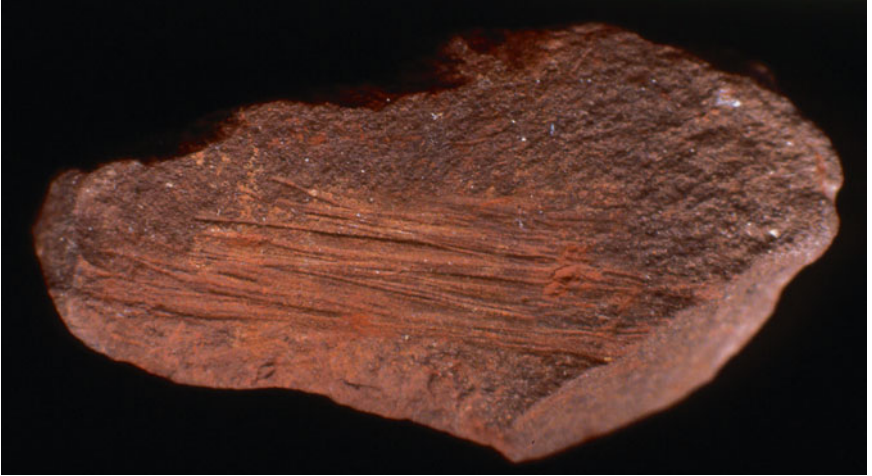


PLATE 4.7 CI Coarse siltstone, lightly scraped. 13.6g, 38.3 mm length, NCS streak 3258 Y60R.



PLATE 4.8 CGB Shale, lightly (edge) ground 1.3g, 33.9 mm length, NCS streak 3555 Y70R. Facet width 3 mm.





Embongo / Leopard



Njoku / Elephant



Mboko / Buffalo



Sumbu / Chimp



Ebobo / Gorilla



Ngwai / Pig



Moseme / Bay Duiker



Mbomu / Marsh Duiker



Bemba / Yellow-backed Duiker



Bongo / Bongo Antelope



Jongi / Aardvark



Mkwake / Crocodile



Kema / Monkey



Kalu / Colobus Monkey



Misime / Snake

PLATE 5 Some examples of Mbendjele hunter's sign-language (all photos by Nico Lewis)

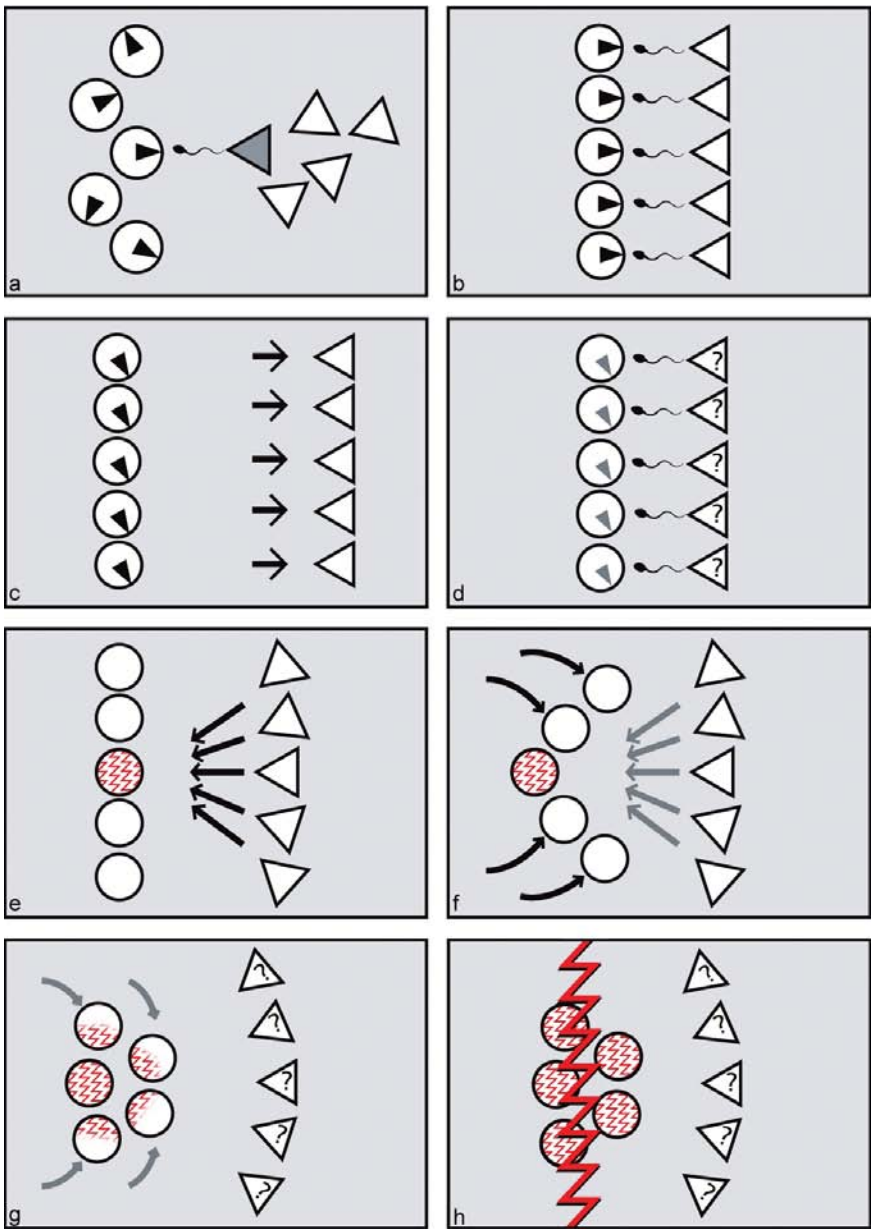


PLATE 6 The Female Cosmetic Coalitions model.

- Legend:
- a. Females compete for good genes, staggering signs of ovulation.
  - b. Females compete for mating effort, synchronising signs of ovulation.
  - c. Males abandon females once ovulation has passed.
  - d. Females counter this problem by concealing ovulation and extending receptivity.
  - e. Menstruation now attracts disproportionate male attention.
  - f. Coalition members respond to this threat by controlling male access to the (imminently) fertile female.
  - g. To prevent males from picking and choosing between them, members of the coalition join forces and 'paint up'.
  - h. "Wrong sex, wrong species, wrong time" is established as an institutional fact.



PLATE 7 On the morning of a Himba marriage, female friends apply ochre to the bride.

Photo: Carol Beckwith and Angela Fisher (reproduced with permission)





PLATE 8 With her arms raised to imitate horns, an ochred Himba woman stamps her feet as though they were hooves in the Ondjongo courtship dance. Photo: Carol Beckwith and Angela Fisher (reproduced with permission)