Sapienization and Speech

by Grover S. Krantz

The anatomy of the modern human skull differs in a number of respects from that of the skulls of our Middle Pleistocene ancestors. These differences are considered to represent a morphological gap sufficient to separate them into two temporal species, Homo sapiens and H. erectus. At this point general agreement ends. The disagreements fall mainly into two categories, one following from the other. First, it is not agreed just which of the many cranial contrasts are the most important or fundamental in drawing this distinction. Second, depending on which traits are chosen, the dividing line between these two temporal species may pass on either side of certain key fossils, such as Solo, Broken Hill, and the Neandertals.

In order to settle this problem we must establish the reason for the evolutionary change from one species into the other. It is often assumed that our own anatomy represents the best that has yet been attained and that our presumed ancestors, where they differ from us, must by definition be inferior. The matter is not that simple. H. erectus existed for over a million years with relatively little change—a kind of evolutionary plateau—and then was transformed rather quickly into H. sapiens. Because anatomy is in the last analysis a reflection of adaptation to behavior, we must look for a change in behavior that can account for this transformation. It is impossible to demonstrate the behavioral change directly, but it is possible to describe a behavioral change that would reasonably account for the anatomical changes. Conversely, we can describe the anatomical changes in such terms that the relevant behavior becomes obvious. This is exactly what we do in relating post-crani- ansal anatomy to locomotion in other fossils. It is well known that selection pressure will favor variations which confer some behavioral advantage and not others. Thus a species must first at least begin to attempt to practice some new behavior—be it brachiation, bipedalism, or whatever—in order to evolve the anatomical adaptations to do it better. While this may sound Lamarckian, it most assuredly is not.

The first step is to list the anatomical traits of H. sapiens skulls which are generally agreed to distinguish them from skulls of H. erectus. This contrast will be mainly between such early sapiens skulls as Florisbad, Wadjak, Cro-Magnon, and Skhul, as opposed to Middle Pleistocene erectus from Peking and Java and some more recent African finds. (Postcranial anatomy is ignored here for lack of demonstration, to date, of any consistent or significant differences.) The order of presentation is for convenience of explanation, as will be seen later. In each case the sapiens trait is given first and then contrasted, as necessary, with the erectus condition.

1. Endocranial capacity is nearly 1,500 cc as opposed to an average of 1,000 cc. (Late Pleistocene erectus specimens like Broken Hill are often larger than 1,000 cc because of the large body sizes suggested by the size of their optical orbits.)

2. The vault thins to 7–8 mm (thinner in most moderns) from 10 mm in the Middle Pleistocene. (This is average thickness as measured away from ridges.)

3. The ridge system of the braincase is rounded out in contrast to erectus’s series of nearly flat planes between angulations in the vault.

4. The foramen magnum is drawn downward, along with the occipital condyles, and is moved forward slightly, with its opening directed more anteriorly.

5. The basicipital slope is steeper, approaching 45° from the Frankfort plane instead of lying nearly horizontally (Lieberman 1975).

6. The cranial base is pulled inward around the sides and back, with the muscle attachments on the supramastoid area and occipital ridge moved more toward the center of the skull. (In erectus these muscle attachments are widely set and usually form the widest and rearmost parts of the skull.)

7. The vault of the braincase is higher.

8. The mastoid processes are larger and longer, with the projection being downward and slightly forward. (Very large erectus skulls can have respectable mastoid processes.)

9. The mastoid processes are vertical, not inflected as in erectus.

10. The occipitomastoid crest noticeable in erectus (Stewart 1964) is absent.

11. The dentition is smaller and set back, with the third molar holding about the same position.

12. The face is reduced and drawn back.

13. Browridges are reduced in size, and the forehead above them is more nearly vertical.

14. The nasal bridge is more prominent in relation to the rest of the face.
Most of these trait contrasts can be found in Coon (1962) and Howells (1973) or, for that matter, in any introductory text. Traits 5 and 10 are credited to other authors, who have called attention to them in Neandertals; I have found them to be characteristic of *erectus* in general. Trait 4, to the best of my knowledge, is an original observation here.

A few of these traits may seem obscure at this point, and even overlapping, but their significance will become obvious when they are described as a single functional complex. Absolute measurements are rarely given in this listing. For most of these characteristics, either they have not been measured regularly in the past or their measurements have not been standardized. Some are affected by the absolute size of the individual, which can sometimes make direct comparisons misleading.

Some *erectus* skulls may show a modern trait or an intermediate condition, such as large mastoid or steep basioccipital. Similarly, individual *sapiens* skulls may show the earlier design, fully or partially, in one or two respects, such as occipito-mastoid crests or small brain. In general, however, almost all skulls can easily be classed either as overwhelmingly *erectus* or as overwhelmingly *sapiens*. The exceptional cases are to be expected if one type evolved into the other. In the *erectus* gene pool, much of the variation from which the new adaptive traits could be selected for should already have been present. In the *sapiens* gene pool, some of the older traits might be expected to occur from time to time. There is always a certain amount of leeway in hard and soft parts so that any one or two exceptional traits can be “covered” by exceptional developments in certain others. No one of these trait contrasts needs to make a perfect separation between the two temporal species. The trait complex as a whole, however, does make this separation possible.

Many Late Pleistocene fossil skulls might now be tentatively classified in light of this list of traits. The Solo skulls of Java are entirely of the *erectus* design in all parts that are preserved. The Broken Hill skull is primarily *erectus*, although a few of its traits are intermediate or on the *sapiens* side of the dividing line. In contrast with this view of Broken Hill, Howells (1973:113) remarked, “Others, more impressed by details, are inclined to consider him an extremely primitive example of modern man.” If this attitude is accepted we would have to redefine the *erectus-sapiens* threshold on the basis of these unnamed “details” rather than by the usual criteria. European Neandertals clearly show the *sapiens* condition in the first three traits, but are essentially *erectus* in the rest. The picture already emerging here is that in terms of morphology they could all be classed with *erectus* and that the dividing line between *erectus* and *sapiens* might well prove to be at about 40,000 years ago. Such an assertion must rest upon the demonstration that these traits fit into a “total morphological pattern” (LeGros Clark 1955).

This line is clearly indicated in some places and is compatible with most of the available evidence elsewhere. Mt. Carmel brackets it neatly with the Tabûn Neandertal at 45,000 years ago, or a bit less, and the Skhul *sapiens* at about 35,000 (Higgs 1961; Howells 1973:92; François Bordes, personal communication). The European transition from Neandertal to Cro-Magnon may have been more recent, between 40,000 and 30,000 years ago. These two cases give just 10,000 years for the change—only 5,000 if they are assumed to cover a single event. In southern Africa the Saldanha and Broken Hill skulls may be only 40,000–50,000 years old, while the modern-looking Florisbad skull may be almost as old (Klein 1970). Recent proposals to back-date the lithic industries of southern Africa (Klein 1973) might also redefine some of the skeletal material. This would not, however, affect the sequence, and it is far from being settled (Rightmire 1979). Similar contrasts of physical types in Java (Solo vs. Wadjak) and in China (Mapa vs. Liu-Kiang) also straddle the morphological transition, but their datings are too unclear to say more than that they would easily fit into the framework suggested here. There are always claims of *sapiens* skulls older than 40,000 years, but these have been disputed with regularity in the past. Meanwhile, the main weight of the fossil record supports the transition date given here.

Cultural evidence on a worldwide scale indicates that something unprecedented was occurring at about this same time. Without going into detail or worrying about exceptions, the evidence can be summarized briefly as follows: (a) Tools became more sophisticated, suggesting that more learning was involved in their manufacture. (b) Tool types became more geographically localized, specific techniques being developed for given circumstances. (c) Stone-tipped projectiles became common everywhere. (Earlier “points” are mostly thick-based and almost impossible to haft.) (d) Fire using and cave dwelling became common everywhere, not just in colder climates. (e) Technological changes began to occur more rapidly. (f) Populations seem to have increased. (g) There were major expansions into formerly empty territory such as northern Eurasia and the African rain forests. (These points are discussed in Bordes [1968:229–41], Braidwood [1967:80], Butzer [1964:384–85], Chard [1975:145–64], and Clark [1967]; c is my own observation.) This all suggests that a more-than-gradual cultural change of some kind happened rather suddenly (if 10,000 years can be called sudden). The apparent coincidence with the *sapiens* cranial transformation merits close examination to see if there might be some connection between the two.

Various models have been suggested to describe and perhaps to explain the *erectus*-to-*sapiens* transition. These will be described here in very general terms, beginning with the monophyletic approach and shifting step by step to the more polyphyletic one (see Howells 1976 for a description of the two extreme positions).

The migration-extinction model begins with some geographically limited population of *erectus* evolving into *sapiens* by some mechanism, stated or not. *Sapiens* then somehow spreads out over the habitable world, exterminating and replacing all of the remaining *erectus* populations. The *sapiens* origin may be put at almost any time if the location has no contradictory fossil record. The replacement timing for other areas must at least roughly correspond to the 40,000 years given here (see Keith 1931, Boule and Vallois 1952, LeGros Clark 1955). This model has the basic flaw of being unable to allow for the continuity of line traits in various geographical regions from *erectus* through the transition and into *sapiens* (Coon 1962). Some of these lines cannot be convincingly demonstrated, but one clear case will suffice to rule out the model. From *erectus* to *sapiens* in eastern Asia the traits continue (Inca bone, shoveled incisors, anterior malars, enamel extensions and pearls, and mandibular tori, to name a few). If *sapiens* had arisen in some other region, its arrival in eastern Asia would have terminated these traits, and if *sapiens* had arisen in China they would now be found to the same degree in all the rest of the world. Thus the migration-extinction model can be rejected.

The migration-intermixture model begins with the same limited origin of *sapiens* and its spread over the world, but this time with some interbreeding with local populations of *erectus* (Birksell 1975). This would put locally adaptive genes in the invaders’ pool from which to select for climatic adaptations—stature, body build, nose form, as well as traits we cannot see in the fossils such as hair form, skin color, and eye folds. This model, however, would also lead to the dilution of the selectively neutral skeletal traits (Inca bone, enamel pearls, etc.) by which the eastern Asian line is recognized. In addition, too much time would have been required to sort out the genes in each mixture. Many generations of selection would be required in each breeding pool to bring the *sapiens*...
genes up to maximum before moving them on without diluting their expression. If the proportion of sapiens entering into each mixture were extremely high this time would be shorter, but then the dilution of local traits would be excessive. Thus the migration-intermixture model fails for any of a number of reasons.

The gene-flow-selection model again begins with the localized evolution of sapiens, but here the similarity ends. The movement of the sapiens condition is one of genes, not to any significant degree of actual people. Normal interchange of occasional mates between local breeding populations spreads the genes, and in-place selection builds up their frequencies. After many generations the genes pass on to the next population, and so on, making a full grade change with no more than a ripple of human movement through the various geographical lines. (Coon [1962] uses a model of which this is a very simplified version; Weidenreich [1946] used the same principle, but with multiple sources for sapiens genes instead of a single place of origin; also see Hrdlička [1927], Brace [1964], Brose and Wolpoff [1971].) Here the problem lies in the time required for the transformation. No likely combination of origin place, gene-pool sizes, amount of mixture, and rate of selection appears capable of accounting for the sapienization of the world in less than 30,000 years; but the target time seems to be no more than 10,000 years, and maybe even less. There would seem to be no way to make the gene-flow-selection model work.

These three basic models fail either because the known line traits would not be preserved or because they require far too much time or both. In addition, they fail to indicate the selective advantage of the sapiens condition. The appearance of a larger brain is understandable, but why not until so late in human evolution in most areas? Why did we develop the more fragile design, smaller teeth, and a braincase which is poised so high and poorly supported?

Another model, that of adaptation to a uniform cultural behavior, could get around all of these objections. A new behavioral trait could have spread over the world in a very short time without involving any extensive migrations. This would satisfy the time requirement as well as fully preserve the line traits through the transition. A trait that was sufficiently advantageous in all environments would have caused a uniform selection for the best anatomy to express it. The characteristics of such a hypothetical cultural trait can be listed quite simply:

(a) It must be a behavior that any erectus individual could have learned to perform, at least to some degree. (b) It must be one that can spread rapidly—the basic learning time must be less than a lifetime. (c) It must be highly advantageous, both to individuals and to groups, that even begin to practice it. (d) It must offer room for significant improvement, with immediate and strong selection favoring anatomical variations to perform it better. (e) The best anatomy to express it must account for the sapiens cranial characteristics.

These requirements can be satisfied by the final step in the development of spoken language. This is not to say that the totality of our linguistic behavior dates from this time, but rather that some critical aspect appeared then which perfected the system of communication. The exact invention involved in speech is not a critical issue here, but it clearly had something to do with the vocal delivery system. The sapiens transformation would make complete sense if it could be shown that the use of speech is correlated with the anatomical changes that occurred 40,000 years ago. We must infer behavior from anatomy in order to reconstrue the past. Of course we will never hear the vocal utterances of Neandertals, but neither will we ever see a pterodactyl fly.

The contrast must be stressed between the conceptual aspects of language, or symbolism and grammar, as compared with its delivery system, or speech. Symbolism is basically the ability to form and to arrange mental images of things and events which are not being perceived directly. This includes images from out of the range of the senses as well as from the past and the future. Communication of these mental images from one individual to another is the essence of language. Other animal communication is limited essentially to the here and now. Many kinds of signals may be used arbitrarily to represent these mental images, or concepts. Other vertebrates use vocal and visual signals of from 15 to 45 distinguishable types which convey only about 12 actual message categories (Smith 1969). Nonhuman communication also differs from ours in that the minimal perceived signaling unit carries the message. Human phonemic speech uses meaningless sounds ( phonemes) in meaningful combinations (morphemes) where the minimal perceived signaling unit does not carry the message.

Symbolic capacity is clearly linked to the size of the brain and is potentially quite independent of the communication system. Some studies indicate that mental time span follows from absolute brain size, regardless of body size or phylogenetic considerations (Rensch and Altevogt 1958). Others see body size and organization as significant variables and attempt to measure the amount of brain that is available above and beyond the needs of running the body (Jerison 1973). Recent studies on chimpanzee language abilities have tested only this symbolic aspect. We have known for centuries that these apes cannot speak even when they understand what is wanted of them. With 400 cc of brain volume, they fall far short of the human level of symbolism and grammaticality in spite of much desire on the part of many people to elevate them to semihuman status. The significance of brain size is often denied and reference is made instead to its internal reorganization (Holloway 1976). No difference has been shown between the brains of apes and men, except size, that can be related to symbolic capacity and mental time-span. Given the obvious advantages of symbolism, it is interesting that it never appears in small-brained animals; only in animals with human-sized brains, like dolphins and elephants, do we find indications of this ability.

The development of symbolic capacity may well have paralleled the growth in human brain sizes and may also be correlated with the increasing complexity of material culture throughout the Pleistocene. It does not necessarily follow that our vocal communication system was being perfected at the same rate; it can be advantageous to have considerable symbolic capacity even without an especially effective method of communication. A highly developed vocal delivery system, however, presupposes something of significance to be communicated. While language and speech could have developed simultaneously, it is at least equally possible that language long preceded speech as we know it.

Lieberman and Crelin (1971) came to much this same kind of conclusion by a different route in examining the vocal anatomy of Neandertals. In their much-criticized reconstruction of the Neandertal pharyngeal cavity (based on the La Chapelle skull) they found that Neandertals would have had about 10% of the phonetic ability of modern man, being incapable of producing the three most critical vowel sounds used by modern man in voice decoding (for a full discussion and bibliography see Spuhler 1977). Probably more important than this, however, is the space available to exhale through the mouth, which most sounds are shaped. According to DuBrul (1958:62), “Man is the first animal able to close off the nasal tubes from the rest of the airway completely, easily, speedily, and habitually.” The short pharynx of the apes means that they must bark out most oral sounds, forcing air between the adjacent soft palate and epiglottis. DuBrul and others have assumed that the elongation of the human pharynx resulted automatically from erect bipedalism. That this was clearly not the case is the crux of my argument. The human pharynx
is a much more recent development. It cannot be shown directly that the *erectus* larynx was high in the throat and that the pharynx was very short. However, it can be shown that if the larynx had been so located, the subsequent elongation of the pharynx would have produced all of the modifications seen in the *sapiens* skull.

It is my suggestion that it was the invention of the phonemic principle that transformed our language expression from calls and gestures in *erectus* into the more efficient speech of *sapiens*. This is the same characteristic that Hockett and Ascher (1964) called “duality of patterning.” The concept is simply that we assign meanings to combinations of neutral sounds, rather than to each sound itself, and thereby obtain an almost unlimited vocabulary that can be spoken and understood by other people, with the minimum skills could have been mastered in a few years. Our own children pick up the principles that transformed our language expression from calls to speech, the ability could be substan-

tially improved with some structural adjustments. The desired improvements could even be disadvantageous in other contexts such as swallowing. It may not have been as simple as this; other factors were certainly involved as well. I have deliberately kept the analysis as simple as possible in order for this new idea to be seen clearly. In any case, the elevated phoneme language, as we know it, into the primary mode of communication can, by itself, account for the anatomical transformation of *erectus* into *sapiens*.

The five characteristics of the cultural trait presumably responsible for sapinization may now be examined considering phonemic speech to be that trait:

a) It is behavior that any *erectus* could have learned to perform. Erect bipedism introduced a bend in the naso-pharyngeal air tract and probably lowered the larynx somewhat. The *erectus* pharynx was probably long enough for at least some of the human vocalization pattern—if *erectus* chose to use it.

b) It is a trait that could have spread rapidly. Given a partially human vocal tract, the minimum skills could have been mastered in a few years. Our own children pick up the principle of phonemic speech rather quickly. In a similar sense, the deaf learn the principle of spelling words. The behavior should have been able to spread, by learning and imitation, over the world in just a few thousand years.

c) It is highly advantageous. Rapid transfer of large amounts of information is useful for individuals to communicate their wishes. For groups, the main advantage probably lies in building flexible social organizations, from hunting parties to clan ceremonies. Man is a political animal above all. Complex social groupings, with divisions of duties, which can be changed rapidly are far more effective than unstructured associations. Many of the previously mentioned cultural advances of 40,000 years ago can now be understood. Superior language would facilitate the teaching of more sophisticated tool making; rapid transmission of new techniques would mean faster technological change as well as easier adaptation to localized resources; hallowed spear points and other compound tools depend on sophisticated techniques; the effective use of fire (and caves) requires coordinated fuel gathering and hearth tending; increased population follows from more efficient distribution of food supplies; and expansion into inhospitable areas would be facilitated by a higher level of social organization.

d) It offers room for improvement. While the *erectus* vocal anatomy would permit speech, the ability could be substantially improved with some structural adjustments. The desired changes are relatively minor as compared with earlier steps in human evolution. Much of the needed variation that would be selected for would already exist within the *erectus* gene pool. Accordingly, the transformation into *sapiens* cranial anatomy ought to have been fairly rapid and could have been largely accomplished in a hundred generations or so. The best vocal anatomy and associated cranial alterations would be the same in all environments, regardless of the language spoken. Selection would be for the same traits everywhere, making *sapiens* a more unified species than was *erectus* immediately before this grade change.

e) The best anatomy for expressing it is that of *sapiens*. The central anatomical change is the elongation of the pharynx, an entirely soft structure which leaves no fossil remains. The longer pharynx becomes the major vowel-forming organ through control of its muscular walls. Its length separates the vocal soft palate (fig. 1) from the epiglottis, permitting easy exhalation through the mouth. In addition to the vocal mechanism, a certain cerebral quantity is desirable, if not essential, and this subject will be dealt with first. In the following discussion I shall address each of the 15 points of contrast between *erectus* and *sapiens* in turn:

1. An endocranial capacity of nearly 1,500 cc is evidently optimal for information storage and/or mental time-span in average-sized human bodies. This conclusion follows in part from the fact that this size of brain appears in all parts of the world with the advent of *sapiens*. In the Neandertals it had already been achieved. Some modern populations have capacities averaging nearer to 1,300 cc, but these are usually rather small people. The allometric effect of body size on the brain is easily on the order of 200 cc, more or less.

2. A vault of 7–8 mm thickness would automatically result from the increased brain size if the amount of bone in the vault remained constant, simply being spread over a larger area. As might be expected, the large-brained Neandertals had skull thicknesses reduced to this amount from the more usual 10 mm in other *erectus*.

3. Given a half-span endocranial expansion without any corresponding increase in bone mass, the most efficient vault design approaches that of a sphere. The typical *erectus* construction, with sagittal, parasagittal, and transverse ridges and intervening nearly flat surfaces, cannot economically be maintained. Again, the Neandertals anticipated the *sapiens* condition, but this is where their similarity to us ends.

4. The drawing down of the foramen magnum, along with the occipital condyles, results from changes in the pharynx. The upper end of the pharynx is fixed in its location by the level of the palate and the entrance to the nasal passages, which cannot be moved. Any lengthening of the pharynx must be by extending its lower end downward into the neck. This means moving the larynx and epiglottis down by as much as 2 cm from an otherwise apelike location higher in the throat. Since the larynx and its supporting structures are attached to the esophagus and this in turn to the cervical vertebrae, they too must move down some distance relative to the level of the palate. These vertebrae must be followed down by the occipital condyles and consequently by the entire margin of the foramen magnum (fig. 1).

5. The anterior end of the basioccipital is part of the solid foundation for the upper end of the pharynx and cannot move. The posterior end of the basioccipital is in the skull bottom, which becomes lower in position. This piece of bone, which lies nearly horizontally in *erectus*, becomes steeply sloped in *sapiens* (fig. 1).

6. The basal parts of the brain are of fairly constant volume and are located in specific positions relative to the lower cranial structure. As the bottom of the braincase drops (Point 4), taking that part of the brain with it, the sides of this basal area must shift inward to maintain the same volume for that region. On the outside of this basal area are located the cranial muscle attachments of the supramastoid and occipital crests. These thus move inward slightly toward the center of the skull (see figs. 1 and 3).
Krantz: sapienization and speech

Fig. 1. Midsagittal sections of Neandertal (solid line) and sapiens (dotted line) skulls, showing the rear two-thirds of the braincase, facing left, and illustrating the relative downward movement of the foramen magnum, the tilting of the basioccipital, and the consequent moving inward of the most posterior area. (In this and the following drawings I have used the large-brained Neandertals, but the processes are the same for the other late erectus types with smaller brains.)

7. Hypsicephaly (high vault) results first from the lowering of the foramen magnum region, which makes the braincase taller (Point 4), and second from the pulling in of the base around the sides and back, which makes it shorter and narrower (Point 6). The increase in cerebral size (Point 1), which affected most populations, would also enlarge the upper parts of the braincase without extending the basal diameters significantly. Finally, the facial reduction, yet to be described, would reduce the cranial length and add an appearance of loftiness to the vault.

8. As the occipital condyles and foramen magnum move down into the neck (Point 4), they will also move forward slightly. This follows from the rotation of the basioccipital, which hinges at its fixed anterior end. The posterior part, with the occipital condyles, swings through a short arc, moving down by 10 mm and forward by as much as 5 mm. The atlanto-occipital joint is a sliding articulation on which the skull rotates front and back. The axis of this rotation runs from side to side and is located 10–15 mm above the lowest point on the condyles. In order for the sternomastoid muscles to tilt the skull upright from a far back position, these muscles must attach to mastoid tips located below and in front of the axis of rotation (Point 8), leaving this crest much less conspicuous (see fig. 3).

9. Another function of the sternomastoid muscles is to rotate the head from side to side. The tips of the mastoid processes should retain their lateral positions, well apart from each other, for good leverage in this action. The drawing inward of the skull base (Point 6) pulls in the upper part of the mastoid process and the supramastoid crest but should leave the mastoid tips in their original positions. This makes the whole process nearly vertical instead of inflected (see fig. 3). (I wish to thank J. Brantley Jackson for suggesting this interpretation.)

10. Between the mastoid processes and the foramen magnum there are two prominent crests in erectus skulls which run along the occipitomastoid sutures. These crests are normally absent or much reduced in sapiens, but their presence has been noted in the Swanscombe and Neandertal skulls (Stewart 1964). Actually they are in the same position in both species, and their apparent reduction in sapiens results from the extension of structures on both sides of it. Medially the foramen magnum drops (Point 4), and laterally the mastoid process extends (Point 8), leaving this crest much less conspicuous (see fig. 3).

11. The elongated sapiens pharynx incorporates the back of the tongue into its muscular anterior wall. This is not from any
enlargement of that part of the tongue, but rather from the drawing downward and into a vertical position of the previously nearly horizontal structure. Since the size of the tongue remains constant, as the rear part is drawn down into the throat the forward part must be pulled back. This shifting back and down involves a distance of as much as 2 cm. The movement can be seen in the positions of the topographic features on the tongue itself, notably the foramen cecum. If the tip of the tongue is to maintain its relationship to the lips and anterior teeth, then they too must be drawn back by a like amount. With the third molars holding a nearly fixed position, the entire dentition must be reduced in size in order to move the incisors back (see fig. 4).

12. As the anterior dentition is drawn back by a centimeter or two, the nasal floor and optical orbits will tend to move back similarly. These parts of the face now come to be located more underneath the frontal lobes of the brain instead of partly in front of them. Because the palate must retain its high position at the upper end of the pharynx, much of the face now becomes vertically compressed and thus smaller (see fig. 4).

13. The browridges are the bony upper margins of the optical orbits. As these orbits and their contents are drawn back and under the front part of the brain, their upper margins no longer stand out as separate structures in front—they are simply incorporated into the now-vertical forehead. Any increase in general brain size will only further reduce their projection (see fig. 4).

14. The length and height of the nasal chamber must retain their size in order to warm and moisten inhaled air (Coon 1965). As the structures of the face below and to each side of the nose draw back by a centimeter or more, the nasal bridge stands out in correspondingly greater relief (see fig. 4).

15. The space behind the chin is essential for the tongue muscles, swallowing space, and action of the digastic muscles. This is even more critical because the occipital condyles and cervical column have swung forward, tending further to shorten the available space. The chin region basically retains its original posterior while the alveolar part of the jaw above it is drawn back (see fig. 4).

Thus each of the 15 basic traits that distinguish sapiens from erectus skulls follows automatically from the presumed development of the vocal apparatus—specifically, from the downward elongation of the pharynx. The exact cause-and-effect relationship has been described for each trait. While pharyngeal elongation cannot be demonstrated directly in the fossils, all the details of the sapiens skull design can be explained as necessary consequences of it.

Other causes have been proposed for various aspects of the sapiens change, and some may well have contributed to this picture. However, no other explanation, or combination of other explanations, comes anywhere near accounting for the entire trait complex. The explanation given here goes even farther in that it also accounts for the simultaneity and rapidity of this event, for the continuity of line traits through the transition, and for the uniform results after it happened. Again, this is not to say that there were no other factors involved in this evolutionary change, but only that they are not necessary to account for it.

When formulating this idea some years ago I briefly considered the possibility that the practice of group exogamy might have been the cultural factor responsible for the sapiens transition. This would satisfy the requirements of erectus’s learning it, of rapid spread, and of great social advantage. Exogamy did not, however, imply any obvious selection for concomitant anatomical changes—certainly not those of sapiens cranial architecture. It may nevertheless be pertinent here because systems of exogamy (and perhaps even the incest taboo itself) would seem to require the kind of social organization that only spoken language can provide. If the basic breeding unit, the dialect tribe, is far larger than the exogamous hunting bands of which it is composed, then gene flow is facilitated over greater distances. This should then have become instrumental in maintaining species unity after the transition. I would rather leave further investigations along this line to others.

The process of sapienization becomes essentially an in-place phenomenon from the preceding erectus condition. Many genes may have flowed, some migrations may even have occurred, but these are not essential to the picture. There are no phylogenetic implications here other than a simple, straight-line evolution through a significant morphological change in grade. The lack of full speech capability in Neandertals does not in any way remove them from our ancestry, as some have claimed. There were no other contemporaneous fossil forms to draw on which showed the sapiens condition or even a significant part of it (Crelin’s 1973 reconstruction of the Rotheim skull using part of a modern cranium notwithstanding).

It must be emphasized again that this interpretation of the sapiens transformation is based only on the development of full vocal communication, presumably of phonemic speech. This is not language. Language involves many neurological and conceptual processes and behavior that were well developed before that time. Late erectus would have been fully capable of symbolic communication in general in order to have so quickly adopted, and adapted to, this new emphasis on vocalization.

The various late erectus populations were evidently not equally endowed with linguistic-symbolic capacity. If endocranial volume is any measure of this, the fact that large brains appeared everywhere as a part of this transition strongly suggests that the originators already had brains of the requisite site. This would seem to indicate an origin area either somewhere in the northwestern quadrant of the Old World, where Neandertals lived, or in the largely unknown northeastern quadrant, which includes China. Known dates of the relevant
fossils currently do not prove Europe to be the source. A northern environment with dramatic changes in the seasonal duration of daylight would most favor the development of a vocal language to replace one of visible gestures. Beyond this we can only speculate as to where it all began.

Comments

by Robert L. Blakely

Department of Anthropology, Georgia State University, Atlanta, Ga. 30303, U.S.A. 7 vt 80

Like its progenitor by Lieberman and Crelin (1971), Krantz's work should generate meaningful discussion and debate among anthropologists. Out of the polemic may emerge testable hypotheses regarding the acquisition of phonemic speech in the hominization—but not necessarily sapienization—process.

Biological anthropologists likely will argue with several of Krantz's contentions, among them that the transition from erectus to sapiens can be placed as recently as 40,000 years ago; that, by contrasting cranial differences between Middle Pleistocene erectus and "early" sapiens (Florisbad, Wadjak, Cro-Magnon, Skhul), the author has overstated the magnitude of anatomical change from erectus to sapiens; that, Krantz's protestations notwithstanding, he does sound Lamarckian, at best a tactical error and at worst a flaw in logic.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

Still more perplexing, as Ina Jane Wundram points out (personal communication, 1980), is the author's failure to address the critical issue of causality. Because the advent of phonemic speech is placed in an evolutionary framework, the impetus for the critical issue of causality. Because the advent of phonemic speech is placed in an evolutionary framework, the impetus for any cultural development, such as the shift to phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.

More nettlesome than these problems, however, are questionable assumptions upon which the author's argument rests. For example, biological anthropologists and other investigators dispute the extent to which cranial morphology can be used to infer speech capabilities. Following Crelin's reconstruction of the supralaryngeal vocal tract of the (Neandertal) La Chapelleaux-Saints skull on the basis of structure of the cranial base (which elicited criticism of the reconstruction and of the selection of that particular specimen; see Morris 1974, Falk 1975, Burt 1976a), it has been suggested that osteological dimensions are inadequate indicators of speech capabilities (Carlisle and Siegel 1974). Thus, although Krantz may be entirely correct in using cranial morphology to deduce the capacity for phonemic speech, confirmation must await further study of modern human variation in skeletal anatomy and vocal apparatus.
readily. The fact that these structures are juxtaposed rather than connected in any real sense makes me question whether the skeletal and soft-part changes need be related phylogenetically. The larynx might well have elongated and descended, if such a change were useful, independently of any skeletal modifications.

by CARLTON S. COON
207 Concord St., Gloucester, Mass. 01930, U.S.A. 23 rv 80

Knowing Krantz quite well and being familiar with his ideas, I find little here to surprise me and a few items to query. His uncomplimentary reference to Lamarck mirrors a popular misconception. Lamarck adumbrated the modern theory of evolution by saltation.

Krantz follows the old Hrdličkaan date of 40,000 years ago for the emergence of Homo sapiens out of H. erectus, leaving Swanscombe and Steinheim either wrongly dated or erecti—neither of which they are.

His main argument about the sequence from the erect posture to a modern larynx and the power of speech ends with the reduction in the size of the teeth from prosthion to third molar, as if tooth reduction were a product rather than a causative factor. Didn't teeth grow smaller as a result of eating cooked food, which followed the theft of fire and fire making? Didn't fire making require intelligence above that of the early erecti? Kocher-Kov (1978), the world's authority on the endocasts of the brains of fossil men, has traced the evolution of the parts of the brain concerned with speech, image making, judgment, and manual dexterity. They grew larger and more complicated from the Neanderthals (whom Krantz admits could speak, although with a paucity of vowels) to Cro-Magnon. In her book I cannot find any reference to differences between the left and right frontal lobes of any of the fossil endocasts.

Krantz knows his anatomy and loves to theorize. If in this paper he has not exactly hit the nail on the head, neither has he crushed his own thumb.

by DEAN FALK
Department of Anatomy and Caribbean Primate Research Center, Medical Sciences Campus, G.P.O. Box 5067, San Juan, Puerto Rico 00936, U.S.A. 3 vi 80

I find Krantz's article interesting and stimulating, but I think that its emphasis is too much on the peripheral and too little on the central. It will not do to justify concentration on peripheral anatomical structures to the exclusion of the central nervous system with the disclaimer that speech is not language. In fact, speech is language (or, rather, its motor aspect, the sensory aspect of language including comprehension of speech, writing, etc.). It is extremely unlikely that speech "is behavior that any erectus could have learned" without the central neurological equipment to do so. The best relevant paleoneurological evidence is based on cranial capacities of fossil hominids (Falk 1980a), and this evidence suggests that brains attained their modern size well before 40,000 years B.P.

Evidently Krantz accepts Lieberman and Crelin's (1971) assumptions that (1) the location of laryngeal soft parts can be reliably inferred from skulls and (2) hominoids with larynges located high in the throat were (are) incapable of producing the full range of vocalizations involved in human speech. The first assumption has been criticized extensively in the literature (e.g., Falk 1975). Although the second assumption has received less attention, Jordan has shown through acoustic analyses of voice sounds emitted by chimpanzees that these hominoids are "capable of producing sounds with physical properties . . . similar to those emitted by human beings. The fact that chimpanzees do not make use of these physical properties in articulated voice is due to the lower degree of development of their central nervous system" (Jordan 1971:337). Bipedalism does correlate with descended larynges (Negus 1949, Falk 1975). If chimpanzees with superiorly located larynges can produce human-like vocalizations, there is no reason to assume that big-brained bipedal Neanderthals or, for that matter, small-brained bipedal Australopithecines could not.

Finally, Krantz accepts the idea that vocal language replaced gestural communication during human evolution (Hewes 1973). On the contrary, I have argued (Falk 1980b) that evidence from comparative primate neuroanatomy, archaeology, and studies of vocalization systems of nonhuman primates suggests that right-handedness and gestural communication (which could not have preceded freeing of the hands) succeeded speech and may have been due to selective pressures for increased complexity of communication, causing a field effect upon the brain.

by MARK STEWART FLEISHER
Department of Anthropology, Washington State University, Pullman, Wash. 99164, U.S.A. 27 v 80

My criticisms are centered on four issues.

1. The argument is, in part, teleological. Krantz writes that "a highly developed vocal delivery system . . . presupposes something of significance to be communicated." What he is saying is that speech did not appear until the potential speakers had something to say; in effect, he is arguing for the self-realization of a preexistent notion, i.e., information to be communicated. Moreover, his line of reasoning begs the question of why the symbolic capacity was selected for at all. If there was no apparent behavioral change (and there could not be until speaking existed), then why would the capacity for symbolizing be of any selective advantage? Given this, any advantage bestowed on hominids because of their sophisticated cognitive capacity would have gone "unnoticed."

2. The notion of "symbolic" is highly restrictive. Krantz writes that "while language and speech could have developed simultaneously, it is at least equally possible that language [symbolic capacity] long preceded speech . . . ." Symbolic communication involves a good deal more than the denotative content of verbal communication; it includes ritualism, painting, sculpture, theatre, music, dance, etc. If morphology prevented or seriously inhibited speaking, why do we not find evidence of other sorts of symbolic behavior appearing long before cave art? The symbolic capacity should express itself in ways other than verbal behavior.

3. Krantz writes that "the invention of the phonemic principle . . . transformed . . . language expression from calls and gestures in erectus into the more efficient speech of sapiens. This is the same characteristic that Hockett and Ascher . . . called 'duality of patterning'" (emphasis added). First, Krantz's "phonemic principle" is not the one commonly known in linguistics (Swadesh 1934). He should have labeled it differently to avoid conflating the concept of the phoneme with more abstract grammatical and morphological notions. Second, Krantz's idea of "neutral sounds" is very different from the notion of neutral sounds in phonology. Third, the "duality of patterning" discussed by Hockett (1958, 1960) has implications that go beyond Krantz's simplification that "meanings" are "assigned to combinations of . . . sounds." Hockett's is a single characteristic of language systems—his 1960 article lists 13. Duality of patterning, productivity, and displacement (the latter two of which Krantz considers under "symbolic capacity" without using the terms themselves) are part of a semantic system, which is a matter of cognition, not sound production. By Krantz's own argument—i.e., duality of patterning—any sounds, not just human-like sounds, can be combined into meaningful combinations if semantic capability is present. The central question, avoided by Krantz, is on the acquisition of which neurological structures symbolic capacity depended.

4. The argument is ethnocentric. It is structured along the lines of symbolic oppositions at work in American culture. For
example, “Rapid transfer . . . of information is useful for individu-
als to communicate their wishes . . . Superior language would facilitate the teaching of more sophis-
ticated tool making; rapid transmission of new techniques would mean faster tech-
ological change as well as easier adaptation” (emphasis added). New/old, fast/slow, easy/difficult, sophisticated/primitive, superior/inferior are oppositions that, as far as anthropologists
know, were not important features in early hominid life. They are
important in today’s American culture (e.g., fast food, fast
and easy mental health and sex, sophisticated and superior
cars, home computers, and toys). Until Krantz can show that
these oppositions were important in the cognitive structuring
of early hominid culture, his argument remains no more than a
Rorschach test for a physical anthropologist who uses as stimuli images drawn from hominid evolution.

by Maciej Henneberg
Zakład Antropologii Uniwersytet im. A. Mickiewicza, ul.
Fredry 10, 61-701 Poznań, Poland. 16 v 80

The main points of departure for Krantz’s interesting hypothe-
sis are very sound: adaptation in morphology to a uniform
behavior, efficiency of phonemic speech as a cultural adaptive
mechanism, presence of preadaptations (linguistic skill, some
capacity for vocalization). From these assumptions it follows
that selection for improved vocal communication was not im-
peaded by the gene flow, minor local environmental constraints,
etc., that usually complicate the situation. However, the
author’s explanation in terms of pharyngeal cavity enlarge-
ment, though attractive, seems simplistic.

The assumptions just mentioned raise a complex question of
group selection. The use of language is advantageous not for
individuals, but for their aggregates; language is essentially a
mode of interindividual communication. Since, as the author
points out, phonemic speech must be learned and, more im-
portant, taught, its spread depends on relations between groups.
I cannot see how the “invention” of phonemic speech could
rapidly spread throughout various populations without spreading
more or less uniform meanings for phoneme combinations.
And yet there is no similarity between words denoting the
same objects (or ideas, etc.) in various languages. It seems,
rather, that phonemic speech was “invented” simultaneously
by various human groups.

In discussing the advantages of phonemic speech, Krantz
neglects one very important one. The formation of sounds in the
primate vocal tract is much less “expensive” in terms of muscu-
lar work than the performance of gestures or facial expressions.
These require visual perception, which in primates is of the
“one-thing-at-a-time” type because of the accuracy of their vision (focusing). Moreover, visual contact with the environ-
ment is in primates of primary importance for survival. The
motor-visual channel of information transmission is so busy that
only with great energy expenditure can it encompass volumi-
nous linguistic communication. The very nature of hearing
allows one to perceive many signals at the same time “all
around the head” (not limited to a narrow beam of acute vision
straight ahead) and almost passively. The same applies to the
“broadcasting” of signals. The larynx and pharyngeal and oral
cavities do not perform long-lasting functions involving large
muscle mass, these being practically limited to chewing, swal-
lowing, and occasional “tool” functions. Therefore signals can be
uttered without interrupting most other activities.

These facts point towards something different from Krantz’s
interpretation of the causes for the development of sapiens
anatomy. Firstly, the utterance of complex sound sequences in
the presence of a large chewing apparatus is relatively “costly.”
As long as this apparatus had to be robust, there was no advan-
tage in vocal communication strong enough to bring about mor-
phological “sapientization.” Therefore one must look for a
mechanism allowing decrease in masticatory functions as a pre-
requisite for efficient vocal communication. The mechanism is
the development, by means of cultural evolution, of techniques
of food preparation. We must also not overlook the replacement
by tools of certain “tool” functions of the teeth and jaws (hold-
ing objects as if in a vise, scraping, etc.). Not all the morpholog-
ical changes described in the article can be due to the elonga-
tion of the pharynx. At least some are the result of evolution
towards diminution of the muscular energy expenditure in-
volved in information transmission.

One may hypothesize that at a certain stage of evolution
phonemic communication became highly advantageous for
humans on a group level. Each pre-sapiens individual was able to
learn it, once properly taught. Such conditions have proba-
bly begun to exist at an earlier stage of hominization. One may
further hypothesize that groups gradually acquired technolo-
gies that made less necessary a large chewing apparatus. Under
such circumstances, minimization of energy expenditure for
communication would become advantageous. Individuals with
more “sapientized” anatomical structure would utilize less
energy for a given amount of communication than more “erec-
tus-like” ones. Since, according to Fisher (1930), the essence
of selective advantage is minimization of energy expenditure for
reproduction (staying alive and producing offspring), individu-
als expending less energy for such important adaptive be-
havior as interindividual communication would possess a
selective advantage.

What the author calls the invention of phonemic speech was
probably a gradual process begun very early in hominid evolu-
tion, while its rapid spread and the spread of concomitant
anatomical changes some 40,000 years ago were due to the
decline of selective forces maintaining a large masticatory
apparatus, which “uncovered” less strong forces favoring
individuals speaking less expensively. This is still individual
adaptation to a uniform cultural behavior, but it does not
require reference to “inventions,” “individual wishes,” and
group selection. Furthermore, it shows that phonemic speech
developed gradually in constant interaction with other com-
ponents of culture, while the anatomical response was the result
of the particular interposition of the development of a cultural
complex (speech, technology, etc.).

by Gordon W. Hewes
Department of Anthropology, University of Colorado, Boulder,
Colo. 80309, U.S.A. 5 v 80

Krantz has presented a stimulating and elegant hypothesis. To
counter it one would have to show that most or all of the sapiens
features he claims to have been selectively impacted by phone-
emicized speech can be better explained in other ways, but also
at the same prehistoric time period. Others will be commenting
on Krantz’s neglect of the effects of changed food habits around
this time, on the modern-sized brains already achieved by the
Neanderthals, and on the fact that domestic dogs and pigs
exhibit many cranial and mandibular features analogous to
those of H. sapiens sapiens (although this evidence also indicates
that such changes in mammals can be very rapid, under appro-
priate environmental conditions). I shall comment on the impli-
cations of the Krantz model for language evolution, concerning
which there has been a marked upswing in serious interest
(Hockett 1978, Dingwall 1979, Parker and Gibson 1979,
McNeill 1979). It has long been supposed that the explosive
cultural manifestations of the Upper Paleolithic were in some
way connected with a surge in cognitive competence, perhaps
triggered by linguistic improvements (Gamble 1980). Krantz’s
hypothesis raises the following questions about language: (1)
What was language like before the advent of phonemes? (2)
How did phonemes come to be “invented”? (3) Why were
phonemes invented at the Middle Paleolithic/Upper Paleolithic
Vol. 21 · No. 6 · December 1980 781
boundary, rather than long before? (4) What is there about phonemicized speech that would have had such a dramatic biological impact on its users?

Foster (1978) has suggested how a language in which phonemic elements still had semanticity could have changed into a phonemic one. Numerous previous contributions to this topic exist (Trombetti 1905, Swadesh 1971, Stopa 1968, Wescott 1974). It would be a useful Gedankenexperiment to construct a model prephonemic language to see how it might have worked.

I suspect that a mixed language system, with both (prephonemic) speech and gestural signing, continued into the Middle Paleolithic. Groce (1980) reports that a somewhat comparable mixed system was in everyday use until the late 19th century on the island of Martha's Vineyard, off New England, where there was an unusually large number of born-deaf people. To be sure, the spoken language, English, was fully phonemic. Foster suggests that phonemes could arise through overgeneralization of a phonetic element used as an affix, for example. That phonemes can be diffused is well established. As to why phonemes only came into language at a particular prehistoric time period, I would suppose that there had been some sporadic formation of phonemes for some time, without reaching sufficient numbers in any one language to permit "take-off" into a phonemicized system. The confluence of several different phonemes (i.e., phonetic units deprived of semanticity) would be most likely in a region of greater than average Paleolithic population density and linguistic diversity, and it does not seem implausible to place such a situation at about 40,000 years ago.

The last question is the most profound. That phonemes permit the building of larger lexicons seems clear, but large lexicons—of several thousand entries—would have been of little value unless their users could quickly store and retrieve word information in their long-term memories. The fundamental value of phonemically based language is great efficiency for memory storage and retrieval, starting necessarily with the virtually unimprovable, strict limitations of our short-term memories.

Many fascinating ramifications stem from Krantz's short paper, having to do with the extent of hemispheric lateralization for language in the Middle Paleolithic (cf. Spuhler 1977, Myers 1978, McNeill 1979, Falk 1980), the fact that the human fetus, in utero, during the third trimester, exhibits responsiveness to human speech sounds, and the fact that the neonate can discriminate between maternal speech and that of others (Armitage, Baldwin, and Vince 1980, DeCasper and Fifer 1980). Nor is phonemicization in our species fully achieved, the advantages of the larger brain outweigh the disadvantages associated with the scheme. They have the right conformation—enlarged brain (symbolic capacity) preceding remodeled skull base (for speech)—but the opposite combination is seen in the Sámi skull, which is dated to at least 160,000.

3. If symbolizing capacity and/or the acquisition of favorable genes led to rapid remodeling of the Neanderthal cranial, then what processes led to the equally dramatic and rapid change from the massively robust and short-legged body to the contrasting form of the Upper Paleolithic?

Other readers will doubtless raise other queries, such as the suggested mode of dissemination of the impulse to new linguistic behavior. All I am saying is that Krantz has burdened his hypothesis with elements, as objected to above, which it could probably get along without.

by Doris F. Jonas
24-B, Wellington Court, Knightsbridge, London SW1X 7PL, England. 23 iv 80

1. Krantz states, "Because anatomy is in the last analysis a reflection of adaptation to behavior, we must look for a change in behavior that can account for this transformation." While this statement appears unexceptionable at first glance, interrelationships in the evolution of anatomical and behavioral changes are far more complex than it would imply. One must not overlook the modifications of physiology involved in anatomical change or the effect that these may exercise on behavior. Thus modified physiology and anatomy may determine behavior quite as often as changes in behavior eventually modify anatomy. In the end each exerts an influence back on the other in a mutually reinforcing positive feedback.

An overriding factor distinguishing primates from other mammals, and H. sapiens from other primates, is neoteny, which involves a bias toward prolongation of all developmental phases. This is not the place to go into the adaptive advantages promoting selection for neoteny, which are generally understood under such shorthand phrases as extended period of brain plasticity, making possible extension of juvenile curiosity and exploratory propensity; learning; postponement or total loss of rigidly stereotyped behavioral patterns; and so on. Suffice it to point out that the anatomy and physiology of a species which has found selective advantage in prolonged developmental phases necessitate behavioral adaptations to the emerging new form and function. Thus, for example, the extreme immaturity and helplessness of human infants at birth demand an exceptionally high propensity for care-giving behavior and also an intensive bonding between mother and infant if the species is to survive. Nothing else is as important to human individual and species survival as this bonding. While it is true that behavior (in this case, intelligence) promotes form, in fact form in turn promotes behavior (in this case, care-giving).

2. In his list of sapiens traits which differ from the erectus condition, Krantz says he believes the position of the foramen magnum to be an original observation. Yet it was noted by the Dutch anatomist Bolk (1926) as a factor in upright stance and a neotenic trait; Montague (1960) refers to it, and so have I in connection with work on behavioral equivalents paralleling neoteny (e.g., Jonas and Jonas 1973).

3. Krantz asks, "Why did we develop the more fragile design, smaller teeth, and a braincase which is poised so high and poorly supported?" The answer is clear: These are all aspects of human neoteny, and selection for them has occurred because the advantages of the larger brain outweigh the disadvantages of the concomitant retention of other "young" characters.

4. I would disagree with Krantz's statement that the size of the chimpanzee's brain precludes symbolic capacity. First, most

by W. W. Howells
Peabody Museum, Harvard University, Cambridge, Mass. 02138, U.S.A. 29 iv 80

We need hypotheses on the dynamics of the appearance and establishment of the H. sapiens skull, and language/speech is an important possibility. I agree that something was apparently occurring at 40,000, and probably earlier also. Krantz's hypothesis suffers, however, from being unnecessarily rigid and all-embracing; also, he is disrespectful of the rule that hypotheses should be trimmed by the facts, not the other way round.

Here are some sample problems:

1. He should not so blithely sweep under the rug all dates for sapiens skulls before 40,000. The best present authorities put Broken Hill, the Kitish Formation Omo skulls, and Border Cave at 100,000 or earlier, with corroborating archaeological and geology. While, as he says, this preserves the general order, it makes Africa the likely area of origin, not the northwestern quadrant (Neanderthals) or the northeastern as he suggests.

2. The above dates put Neanderthals seriously out of sync with the scheme. They have the right conformation—enlarged brain (symbolic capacity) preceding remodeled skull base (for speech)—but the opposite combination is seen in the Sa'le skull, which is dated to at least 160,000.
animals’ communicative signs and signals are largely symbolic; second, human microcephalics exist who are capable of language.

5. I believe Krantz to be mistaken when he says that “a highly developed vocal delivery system . . . presupposes something of significance to be communicated.” Many organs or functions serve as serendipitous preadaptations when new functions become advantageous. Thus the swim-bladder served as a matrix for the evolutionary development of lungs, a sphincter cleft in the foodpipe for eventual vocal cords, etc. (Negus 1949). A “babbling” vocal delivery system is found in chimpanzee infants among several traits which serve to attach their mothers to them, but it dies out in them as the infant becomes more independent. The prolongation of human infancy and its babbling phase promotes a tendency in human mothers to feed these sounds back to their infants, reinforcing what is essentially attachment behavior until it becomes extended into speech and ultimately available for communication (Jonas and Jonas 1975, 1979).

6. Of the list of functions Krantz considers to be so “highly advantageous” as to promote selection for speech, none is so fundamentally important to our species survival as the bonding of care-giving mothers to helpless infants. It is here that we must look for the significance of the genesis of language. Speech became possible when the morphological changes involved in upright stance (one of the many facets of slowed development resulting in fetal retentions—in a word, of neoteny) had already taken place. It is not until then that the vocal apparatus descended in the larynx, making speech sounds possible. The changes could not have taken place to facilitate speech. Nature is not “purposeful,” but uses what is by chance available.

by JEFFREY T. LAITMAN

Department of Anatomy, Mount Sinai School of Medicine of the City University of New York, 1 Gustave Levy Place, New York, N.Y. 10029, U.S.A. v 80

When speech developed has been a provocative topic among anthropologists for some time. While Krantz’s study raises important questions and presents an interesting hypothesis, I disagree with certain of his conclusions. My objections stem from what I perceive as his lack of emphasis on basicranial data in (1) assessing the taxonomic affinities of fossil specimens and (2) reconstructing the vocal tract of these hominids.

As Krantz notes, determining which specimens should be classified as Homo erectus and which as H. sapiens is a difficult task. A major premise of his argument is that erectus existed until 40,000 years B.P., at which time it rapidly evolved into sapiens. He dismisses the idea that sapiens existed before this date with a statement that claims of older sapiens “have been dispatched with regularity in the past.” If he had relied more heavily upon recent literature, he would have noted that some studies have gone beyond “unnamed details” to provide quantitative data which strongly indicate that skulls such as Broken Hill show modern features in areas relating to vocal anatomy. For example, Burr (1976b) and Laitman, Heimbuch, and Crelin (1979) have shown that the basicranium of Broken Hill is largely indistinguishable from that of modern sapiens. While dating of Broken Hill is uncertain, Klein’s (1973) work indicates that it may be much older than 40,000 years B.P.

Other specimens may also indicate the presence of early sapiens. Analyses of Steinheim, for example, have shown that its base is also similar to that of modern sapiens (Laitman, Heimbuch, and Crelin 1979, Crelin 1973). Steinheim is believed to be older than 200,000 years B.P. (Oakley 1966, Gieseler 1974).

Our examination of the Sahel cranium from North Africa indicates that this skull as well exhibits basicranial features that are modern in appearance (Laitman and Crelin 1980). Sahel is at least 95,000 years old and perhaps considerably older (Jaeger 1975a, b). While no one feature can determine the taxonomic affinity of a specimen, it is unfortunate that the basicranial evidence has not been considered more closely by Krantz.

Krantz emphasizes that the basiocciput is sharply angled in modern sapiens and largely horizontal in erectus. Even a subjective examination of Broken Hill shows that its basiocciput is sharply angled. By his own criterion, then, Broken Hill must be viewed as sapiens, or at least as exhibiting a key feature associated with sapiens. Krantz, however, appears to use his criteria when they suit his hypothesis and discount them when they do not. Although he acknowledges that Broken Hill and certain other specimens he considers erectus exhibit a modern basiocciput, he discounts this on the grounds that some modern traits appear in erectus skulls and vice versa. The angulation of the basiocciput, in particular, and the skull base, in general, is, however, not just “another” trait. Because of the contiguity of the basicranium to the muscles of the pharynx and, in turn, the rest of the vocal apparatus, the orientation of the skull base is perhaps the most significant factor influencing, or being influenced by, the position and shape of the vocal tract (Laitman, Heimbuch, and Crelin 1978).

As the skull base is structurally and functionally tied to the vocal tract, it seems most probable that skulls with a basicranium modern in appearance would exhibit a vocal tract positioned similarly to ours. Thus, the vocal tract of Broken Hill, Steinheim, or Sale was probably very similar to ours. This leaves us with the problem of how to classify these specimens. If one ignores the basicranial evidence (as Krantz does) and considers Broken Hill or Steinheim as erectus, one will be asserting that forms with modern vocal tracts existed in both erectus and sapiens. While this is conceivable, a more parsimonious approach is to acknowledge the basicranial affinities and proposed vocal-tract similarities between these specimens and modern sapiens. Therefore, Broken Hill and the others could best be considered as archaic sapiens.

Sometime between the australopithecines (which our studies have shown probably exhibited vocal tracts similar to extant apes) and modern forms such as Broken Hill or Steinheim, the upper respiratory system altered to accommodate speech. This may well have occurred during the Middle Pleistocene in the erectus phase of human evolution. Unfortunately, basicranial features which tell most about the position of the larynx and pharynx are often missing in much of the erectus material discovered so far. While Krantz provides an interesting hypothesis, he offers few new data to elucidate the relationship between the skull and vocal tract of Middle Pleistocene forms. A greater understanding of this relationship is needed before we can answer questions of how and when the vocal tract changed during the erectus phase of evolution.

by MARJORIE LEMAY

Massachusetts General Hospital, Boston, Mass. 02114, U.S.A. v 80

This presentation is a rather jumbled attempt to delineate the anatomical differences between anatomically modern Homo sapiens and more archaic Upper and Middle Pleistocene members of the genus Homo, all of which are lumped into H. erectus, and to explain the evolutionary transition between these two groups in terms of the “development of spoken language.” There are innumerable points which are at variance with our current state of knowledge of the hominid fossil record (as opposed to our interpretation of it), but I will comment only on a couple of more general issues.

Extensive archeological materials associated with early hominids suggest that detailed communication between individuals was present long before the days of the Neandertals (Isaac
Although the linguistic repertoires of early hominids may have been considerably smaller than those of recent humans, given their simpler cultural systems, there is no a priori reason to expect that the linguistic abilities of early hominids were similarly diminished. In addition, studies such as those of Wind (1978) have made it sensible to believe that the whole process of hominization was intricately interrelated with the development of speech. This view becomes all the more acceptable when one recognizes the anatomical flaws in, and rejects, the pharyngeal reconstructions of early hominids by Lieberman and Crelin (1971, Lieberman 1975; see Falk 1975, Burr 1976a, DuBrul 1977, Wind 1978).

The attempt here to relate a variety of changes in the shape of the vault of the cranium and in the face, to the inferred lengthening of the pharynx in recent humans ignores the basic principle, pointed out in the 1st century by Galen, that the shape of the neurocranium is controlled mainly by the shape of the brain. Numerous studies of craniofacial growth and development (e.g., Enlow 1975; Moss 1964, 1975; Young 1959) have shown that the shape of the vault and face, including the orientation of the basicranium and foramen magnum, is influenced more by the size and shape of the brain than by extracranial forces.

This paper attempts to explain a complex set of features with reference to a single, albeit important, behavior. I can only refer to Hooton (1946-49): "Experience has taught me to beware of simple and seductive explanations of bodily changes."

by Frank B. Livingston

Department of Anthropology, University of Michigan, Ann Arbor, Mich. 48109, U.S.A. 22 v 80

It is now well known that there was an acceleration in the rate of human evolution about 40,000 years ago. This increased rate was primarily concerned with the face, teeth, pharynx, and perhaps also the frontal lobes of the brain, but these changes did have effects on the browridge, mastoid process, and other features of the skull. Krantz, following Lieberman and others, attributes this rapid evolution to the adaptation of the speech mechanism to the development of modern human language.

There are simpler hypotheses for this change—Brace and Wolpoff attribute it to the decreased selection for large teeth that occurred with increased tool use and the development of many specialized blade-tool types. Nevertheless, the origin of human language was surely one of the most dramatic changes in human behavior and should have had considerable effect on our physical characteristics. I agree with Krantz that the rapid evolution he calls sapienization was due to language development but disagree as to the process by which this evolution occurred.

Krantz considers the presence of similar morphological traits in erectus and sapiens populations in major areas of the Old World to be evidence for genetic continuity and lineal evolution. This requires him to assume either that language, having originated in a small restricted set of populations, spread very rapidly throughout the erectus populations of the Old World by diffusion or imitative learning or that language originated simultaneously all over the Old World. Both of these alternatives—particularly the latter—seem highly unlikely to me. Thus, I do not find his evidence for genetic continuity convincing.

While the other major populations do not show good evidence for continuity, the one clear case according to Krantz is the Mongoloids of East Asia, with continuity in Inca bones, shovel ed incisors, anterior malar, enamel extensions and pearls, and mandibular tori. However, in most cases these traits are not restricted to Asian populations, but only vary in frequency among human populations. For example, Inca bones are found in all populations, and their frequency in some South African Bantu populations is as high as in many Asian ones (Eriksen 1954). Furthermore, Krantz assumes that these traits indicate common ancestry because they are selectively "neutral," but there is some evidence that in some populations Inca bones are associated with clinical syndromes of the central nervous system (Pyeis and Khan 1979).

Although, as Krantz emphasizes, any erectus group could have learned modern speech, it does not follow that language could have spread rapidly by diffusion through relatively stable populations. A language is a complex system that is very difficult for an individual or group to adopt. Instead, language acts as one of the most effective isolating mechanisms among humans, primarily because it is a complex system as compared with smoking tobacco or drinking alcoholic beverages. Thus, I would suggest a different scenario. Language evolved in a small set of erectus populations, and it evolved because they were adjusted to a new ecological situation. The erectus stage was an adaptive plateau for hundreds of thousands of years and was adapted to the same tropical and temperate habitats to which other Old World primates are adapted and in which they occupy a temporary or permanent home range with a food supply available throughout the year. With the development of language and then alliances between autonomous groups, which imply a sophisticated communication system with the ability to refer to the past and the future, some human populations could begin to exploit a new habitat, the large areas of northern Eurasia that were seasonally exploitable. At some point these hominids were able to expand beyond their original habitat, and, with language as the isolating mechanism between them and other groups they encountered, their rapid population expansion was responsible for the diffusion of language. Of course, there could have been some gene flow during this intergroup competition, as has happened in recent expansions of human populations, but the major process was population growth and expansion. This is also the explanation of the great genetic similarity among the populations of our widespread species. It should be noted that this new communication system evolved in erectus populations, and since demographic expansion is a much more rapid process than genetic change, most of the late erectus groups throughout the Old World—including the Neanderthals—had the new system. The evolution of sapiens everywhere is the later genetic evolution due to new selective pressures.

by Iwataro Morimoto

Department of Anatomy, St. Marianna University School of Medicine, 2055 Sugao, Takatsu, Kawasaki, Kanagawa 213, Japan. 31 v 80

This valuable article stimulates my interest in human evolution. Krantz has suggested that phonemic speech is essentially responsible for the evolutionary change from H. erectus to H. sapiens. The emphasis on speech in sapienization makes us recall the significance of language acquisition in hominization. Evolutionarily, the earliest man is distinguishable from the ancestors of anthropoid apes by several unique human traits, such as achievement of upright bipedalism, relatively large size of brain, use of language, capacity for abstract thought, and tool making. It is, however, very probable that the use of tools preceded the use of language and that language is the most valuable of man's possessions, although our record of linguistic change is much less complete. Krantz has stressed the contrast between the conceptual aspects of language, or symbolism and grammar, and its delivery system, or speech. He has pointed out the major role of language in the communication of mental images from one individual to another. While language and speech could have developed simultaneously, it is at least equally possible, according to him, that language long preceded speech as we know it.

The organs which are correlated in vocal activity are not structures primarily formed for speech, but the inherited products of our ancestral history. In relation to the capacity for language and speech, the really important human physical characteristic is the possession both of the pharyngeal space

784 CURRENT ANTHROPOLOGY
from the epiglottis to the soft palate and of the speech-associated area in the cerebral cortex. It should be noted that the alveolar part of the mandible in H. sapiens is drawn not only posteriorly, but also medially from the original position. The lower edge of the mandible is, therefore, exerted anteriorly as well as laterally to protect the cervical viscera. The motor speech area of Broca in the frontal lobe of the brain is concerned with the muscular activity of the speech apparatus. It should be taken into consideration that the comparative percentage of the surface of the frontal lobe of the brain is greater in H. sapiens than in H. erectus. Speech and writing belong to the characteristic system of human communication. The primary motor area for speech and writing lies in the precentral gyrus of the frontal lobe, near the motor speech area of Broca. In speech and writing, impulses pass from the precentral gyrus through the different nerves to the speech apparatus or hand. The resulting movements of the speech apparatus musculature in vocalization and the precise movement of the hand in writing are conditioned reflexes. From the fact that speech and writing are correlated, it might be argued that the elongation of the pharynx is due basically to man's erect posture and locomotion.

by Aly El-Nofely

Medical Division, National Research Centre, Dokki, Cairo, Egypt A.R. 19 v 80

This interesting paper is mainly based on the concept of inferring behavior from anatomy and, conversely, explaining anatomical variations between species on the basis of the inferred behavior. This is because its only source of direct information is bones. However, the argument is presented with enthusiasm and considerable detail.

The biologist Wolff (1892), Benninghoff (1934), and others have demonstrated clearly and objectively the interrelation between the form of bones and the functional demands on them. Moss (1962) introduced the theory that the soft-tissue functional matrix is the primary determinant of the form of the enclosed bone. The results of studies on muscles in action using electromyography (Carlsöö 1982) have shown that the degree of participation of a group of muscles in the accomplishment of a mechanical action varies for different individuals. For example, masseter and temporalis muscles both participate in the elevation of the mandible, but in some individuals the masseter is more active, in others the temporalis, while in a third group both participate equally. This implies that the source of muscle force exerted varies in nature (compressive or tensile: Endo 1970), direction, and resulting vectors. The craniofacial muscles are controlled by cranial nerves. Increase in brain size might be accompanied by a wider range of variation in neuromuscular activity. If the characteristics of neuromuscular activities are transmitted from one generation to another (as morphological traits are), then the range of variation will grow wider and wider, perhaps allowing selection to operate and favor the evolution of both form and function in optimal balance for performance of a new behavior. In short, it might be useful to infer behavior from anatomy and try to identify interdependence of form and function in available living organisms; then our explanation would have objective foundations. Perhaps combined morphological and functional studies on the living (cerebral-potential electromyography), comparing various human populations and various primates, would help in elucidating the problem.

In addition, I would like to mention the following:

1. There is no mention of the paranasal air sinuses, which are known to play a role in sound resonance and speech.
2. The author tends to view the development of the chin as a result of reduction of the dento-alveolar arches while the basal part of the jawbone (the chin region) retains its original position. However, a main point of his argument is elongation of the pharynx, which might compensate for the forward swing of the occipital condyles and cervical column.

3. In Item 12 he writes "the face now becomes vertically compressed and thus smaller." Perhaps he means anteroposterior compression and vertical elongation.

Krantz: sapienization and speech

by Georges Olivier

Laboratoire d'Anthropologie Biologique, T. 16, 2 place Jussieu, 75230 Paris Codex 05, France. 2 v 80

L'auteur presente ses idees personnelles sur le role du langage dans la sapienisation. Bien des critiques a lui faire sont sans objet si l'on admet la liberte d'opinion et d'expression. Pour etre bref, je passerait sous silence des remarques que je pourrais formuler concernant certains details anatomiques et de nombreux auteurs non cites. Le seul reproche reel a adresser a Krantz est de n'etre pas assez convaincant; pourtant nous savons qu'il s'agit d'un auteur fonce et aux hypothese originales et enrichissantes.

Il n'est pas convaincant parceque l'evolution du volume cervebral est un phénomene que l'on peut suivre et situer dans le temps, que l'on sache quil n'existe pas d'intermediaire entre les capacites cranienes d'erectus et de sapiens (en tous cas pas entre les relations biometriques des dimensions du cran); par contre le langage articule ne peut etre daté ni etre consideré comme ayant evolué.

Il n'est pas juste de critiquer les idees de Krantz, mais on peut lui objecter les points suivants: Le langage va sans doute de pair avec un cerveau plus complexe, plus specialise. Or Jerison a prouve, contrairement a Holloway, qu'une complexification du cerveau necessite un plus grand volume. Comme on ne peut guere imaginer que ce soit le langage qui ait provoque une complexification du cerveau, il ne nous reste que deux hypotheses logiques: ou bien (1) l'accroissement de volume du cerveau et la differenciation interne plus poussée ont permis le langage (ce qui n'explique pas pourquoi il y aurait eu d'abord accroissement du cerveau) ou bien (2) le langage et l'accroissement cérébral se serait produit simultanément, sous l'influence d'un tres dernier facteur. Krantz souligne fort justement la soudainete de l'evénement, mais il invoque un mécanisme adaptatif aussi classique que peu convaincant. En realite cette derniere hypothese est celle de l'apparition de caracteres nouveaux, achevant l'homisation et dont nous ne connaissons pas le mécanisme.

The author has presented his own personal thoughts on the role of speech in sapienization. If one accepts the concept of freedom of opinion and expression, there is no point in levelling a large number of criticisms at him. In order to be brief, I will leave unsaid certain remarks which could be made on the subject of anatomical details or on the many authors who could have been cited. The only real criticism one can level at Krantz is that he is not sufficiently convincing; nevertheless, it is clear that he is a prolific author whose hypotheses are both original and fruitful.

He is not convincing because the evolution of brain size is a phenomenon which can be traced and situated in time; it is well known that there is no clear intermediate stage between the cranial capacities of erectus and sapiens (at least not so far as biometric relations of cranial dimensions can determine). In contrast, speech cannot be dated or considered as having evolved.

It is not fair to criticize Krantz's ideas, but the following objections could be raised to his hypothesis: Speech certainly goes with a more complex and specialized brain. Jerison has proved, contrary to Holloway, that a brain which becomes more complex requires more volume. Since one can scarcely imagine that it was speech which caused the brain to become more complex, there are only two logical hypotheses left: either (1) the increase in the size of the brain and increased internal
by Ordean J. Oyen

Case Western Reserve University School of Dentistry, Cleveland, Ohio 44106, U.S.A. 10 VI 80

The development of the capacity for speech has often been cited as a major factor in human evolution. Krantz has provided a new twist to this view: that with regard to the transformation from Homo erectus to H. sapiens, “elongation of the pharynx would have produced all of the modifications seen in the sapiens skull.” On the basis of the arguments used and the limited observations he has provided, Krantz’s hypothesis seems to be correct; closer inspection of his logic and additional data, however, indicate that his thesis may be incorrect and is probably untestable. Briefly, some of the problems are as follows:

If there is a significant correlation between browridge morphology and the anatomy of the pharynx, as Krantz maintains, why do infant humans, infant apes, infant monkeys, and infant fossil hominids lack browridges even though the pharyngeal morphology of the extant forms resembles that seen in adult apes (Lieberman, Crelin, and Klatt 1972) and, with the exception of humans, the adults have pronounced browridges (Oyen 1977, Oyen, Rice, and Cannon 1979, Oyer, Walker, and Rice 1979, Oyen and Rice 1980)? Why do adult human microcephalics have distinct browridges (DeBeer 1937) in spite of their seemingly normal pharyngeal anatomy?

With regard to the relationships Krantz has perceived between cranial capacity and vault thickness, what is the nature of the processes involved if they apply between H. erectus and H. sapiens but have never been demonstrated between H. erectus and its antecedent forms? How could one test this part of Krantz’s hypothesis?

As for the often purported relationships between language/speech abilities and the teaching of “sophisticated tool use,” what empirical evidence is there supporting this claim? Why is it exceedingly difficult to put into words all of the steps required to make a simple stone tool, to weave a basket, or to play the violin but relatively simple to teach these tasks through imitative learning—a point which Beck (1972), Sarles (1977), and Suzuki Method violin instructors have been making for years?

These inquiries are indicative of the problems which characterize the arguments Krantz has offered. These criticisms are insignificant, however, in light of a much greater flaw: Krantz has correctly made some structural observations and on the basis of this “evidence” has drawn some causal inferences about evolutionary development. In actuality, it appears that he has confused what has happened with how and why it happened, a common problem in morphological studies (Enlow 1973). Moreover, he has formulated such an all-encompassing hypothesis that it is virtually impossible to test. My factual criticisms do not refute the hypothesis any more than Krantz’s anatomical descriptions verify it. One has to wonder about the utility of such a hypothesis.

Having placed this concept in the marketplace of ideas, Krantz has an obligation to demonstrate its testability and then its validity. Most of us might benefit more from additional testing of this deterministic view than from being further exposed to its enthusiastic promotion—or to its recall.

by J. Anthony Paredes

Department of Anthropology, Florida State University, Tallahassee, Fla. 32306, U.S.A. 6 VI 80

As a nonspecialist, I am greatly impressed—even dazzled—by Krantz’s deft handling of the osteological evidence to infer pharyngeal elongation in the transition from erectus to sapiens. Likewise, his critique of the various models for description of the transition to sapiens is most instructive, though his subsequent comments on exogamy tend to blunt his critique of the “gene-flow—selection” model. Indeed, Krantz’s argumentation is so convincing that one might easily be led to accepting uncritically its apparent general cognitive and cultural implications. There can be no question that the appropriate vocal apparatus was a necessary condition for the emergence of phonemic systems as we know them, but phonetic precision is not sufficient in itself to explain the phonemic principle. In addition, there must be the appropriate neurological organization to process the meaningful/meaningless phonetic differences upon which phonemic systems are built. Although Krantz is quite explicit in acknowledging the distinction between speech and language and even proposes that “it is . . . possible that language long preceded speech as we know it,” he seems rather vague on the relationships between the capacity for symbolization in general and the neurological bases for language in particular. More precisely, despite his admirably convincing arguments for the evolution of the phonemic subsystem of language he sometimes appears to fail to distinguish between syntactic structuring and semantic encoding and then to reduce both to the phonemic principle. This is particularly disturbing given recent experimental evidence that there “appears to be a natural neurological separation between the function of processing sentence form and that of processing semantic representations” (Zurif 1980:311). Perhaps it is this apparent “lumping” of the various subsystems of language among themselves and with symbolization and cognition in general that leads Krantz to what may prove to be an overemphasis on language-like abilities for the explanation of the development of Upper Paleolithic technologies. This is not to say that these linguistic cognitive capacities are not implicated in the increased specialization and sophistication of tool types; rather, it is to suggest that simultaneous selection for increased acuity of other, qualitatively distinct modalities of neurological functioning may have been at least equally as important. (On this point, Tunnell’s [1973:28–30] analysis of “tool use and manufacture in terms of cerebral specialization” is especially pertinent and appealing.) Indeed, it is very difficult to imagine what immediate, direct advantages phonemic speech per se had for the manufacture of tools. I wonder if Krantz has ever tried to tell someone how to tie a square knot or a clove hitch? (Lashing and knot tying presumably were behaviors necessary for making hafted tools, for example.)

In fairness, most of the advantages of phonemic speech which Krantz lists are social, and thus he seems to suggest that it was in the social realm that phonemic speech, and any associated cognitive abilities, were most immediately advantageous, with technological advances being by-products. In this connection, his brief remarks on the possibility of speech as a necessary condition for the development of exogamy, and its attendant implications for gene flow, are particularly welcome. The view that language and speech were selected first for their advantage in social adaptation parallels and amplifies Kummer’s (1971:145–50) solution, following Michael Chance, for the problem of why nonhuman primates have evolved with far greater mental capacity for object manipulation, as demonstrated in the laboratory, than they use in the wild: these abilities are used and were selected for naturally for the manipulation of the social environment and not for technical exploitation of the habitat. All of this leads to the most perplexing question raised by Krantz’s article: What exactly was the nature of the adaptive advantages
which language and speech bestowed upon Pleistocene human-kind? From our present vantage point the answers may seem self-evident, but for me the standard, broad statements about increased efficiency of information transfer and information storage remain naggingly incomplete and unsatisfying.

by G. PHILIP RIGHTMIRE
Department of Anthropology, State University of New York, Binghamton, N.Y. 13901, U.S.A. 28 v 80

Certainly an important problem of current paleoanthropology is where and how the first representatives of *Homo sapiens* evolved, presumably from more archaic mid-Pleistocene ancestors. Whether this speciational event occurred rapidly, in a restricted geographic area, or more gradually in many parts of the Old World is not known. At present, there is scant agreement even as to how early *H. sapiens* may be recognized, with the result that some fossils (e.g., Arafag, Petralona, Ngandong) are viewed differently by different workers.

Krantz seems to have fairly definite opinions on all of these matters. Such confidence allows him to outline the course of later human evolution in general terms, largely as a response to a major shift in vocal ability of Upper Pleistocene populations. This scenario has some appeal, but many scholars will worry about a multitude of exceptions in the fossil record and details of process left unexplained. Some of these problems can be ignored, but others loom large as contradictions to Krantz's thesis.

Several minor comments concern Krantz's listing of features distinguishing *H. erectus* from *H. sapiens*. An average endocranial volume of 1,500 cc is given for the latter, although most sources suggest that 1,350 cc is a better figure for modern humans. Basiocipital slope is said to be more steeply inclined in *H. sapiens*. This is probably correct, although little is known about the orientation of this structure in mid-Pleistocene hominids. The basilar portion of the occipital is not preserved in any of the Choukoutien skulls examined by Weidenreich, for example, and it is damaged or undescribed in other Asian specimens. Size of the mastoid process is variable, even in smaller *H. erectus* individuals. This process is relatively large in ER-3883 from Ileret. Missing altogether is any specific reference to occipital curvature and the greater extent of nuchal coverage in most *H. erectus* crania.

Much more serious is Krantz's casual assertion that many later Pleistocene hominids should be sorted with *H. erectus* on the basis of this listing. The Ngandong crania do seem to resemble *H. erectus* (Santa Luca 1978), but other fossils from Europe and Africa may still be viewed most reasonably as early *H. sapiens*. Certainly the Western Neanderthals differ in so many ways from mid-Pleistocene populations that to lump them with *H. erectus* (Santa Luca 1978), but other fossils from mid-Pleistocene populations that to lump them with *H. erectus* is to strip this species of all biological meaning and utility. Much the same can be said for Broken Hill, although there are some similarities between this individual and Olduvai Hominid 9 (Rightmire 1976).

Even if several of these more archaic skulls are accepted as *H. erectus*, there are severe problems with the idea that *H. sapiens* evolved only 40,000 years ago, with the advent of modern humans. Florisbad, which Krantz agrees is *H. sapiens*, is definitely older than 42,600 B.P. Although it is a matter of fact that one can say with some confidence that *H. sapiens* is 100,000 years old (Butzer, Beumont, and Vogel 1978). Also, what of the hominids from Klasies River Mouth, Broken Cave, and the Omo Kibish Formation? All are pretty clearly *H. sapiens*, and strong evidence placing this material in the early Upper Pleistocene has been presented. Some or all of the Qafza skeletons are anatomically modern rather than Neanderthal-like, although the age of these individuals, presumed to be substantial, has not been reliably determined. In any case, it seems that we are well beyond a time when "claims of sapiens skulls older than 40,000 years" can be ignored.

Given that Krantz's scenario for *H. sapiens* emerging recently and rapidly, over a span of 10,000 years or less in all parts of the Old World, is subject to so many contradictions, there is hardly a need to explain the fossil evidence by resurrecting the views of Lieberman and Crelin. The attempt to provide a functional view of traits distinguishing modern humans is forced at best and vorges occasionally on the ridiculous. To envision the bottom of the braincase "dropping" while supramastoid and occipital crests "move inward toward the center of the skull" in response to mechanical demands of a lowered pharynx is difficult. Not much more helpful is the assertion that "as the occipital condyles move down and forward" into the neck, the "tips of the mastoid processes must extend in the same direction" in order for the sternomastoid muscles to function. Browridges disappear apparently as a consequence of backward shifting of the teeth and tongue.

Changes roughly of the sort that Krantz describes have occurred in human evolution, but increasing size of the brain must be a primary determinant. Even if the acquisition of speech has played a role in shaping human bony anatomy, this cannot have happened as Krantz suggests. The Broken Hill cranium, said to be *H. erectus* and probably of later Middle Pleistocene antiquity, is essentially modern in basioccipital structure, as noted recently by Laitman, Heimbuch, and Crelin (1979). Orientation of the palate and basilar part of the occiput must attest most directly to reorganization of the upper respiratory system, and in Broken Hill these modern aspects of the cranial base are coupled with facial and frontal features which are surely archaic, species identification aside. Apparently the speech apparatus did not appear suddenly in company with a suite of modern cranial traits 40,000 years ago, and Krantz's modeling seems to weaken the whole case for speech as important to evolutionary change.

by RAYMOND RIQUET
Laboratoire d'Anthropologie, Université de Bordeaux I, 33405 Talence, France. 2 VI 80

Travail intéressant utilisant au maximum les données de l'anatomie comparée. C'est un grand mérite. Toutefois il faut souligner que *Homo sapiens sapiens* remonte probablement aux environs de —100,000 ans (Qafzeh et Omo PHS et KHS). Cela peut changer certains aspects du problème car le langage est d'abord affaire de psychisme et d'apprentissage. Les aptitudes anatomiques sont nécessaires mais non suffisantes.

Le système phonique par lequel s'exprime le langage parlé comprend en définitive (a) une soufflerie (les poumons, la musculature diaphragmatique et costale); (b) des tuyaux ou conduites sonores à diamètre variable (pharynx et surtout cordes vocales supposant une commande motrice intentionnelle très sélectrice—la part psychique est capitale); (c) des modulateurs (langue et diaphragme pharyngé); (d) des modulateurs (langue et lévres supposant aussi des structures neuroïdes très fines et une commande encéphalique de circonvolution frontale ascendant). J'ai eu l'occasion de dire que Krantz le rappelle ainsi échapper à trop réduire le problème à ses aspects esquelettiques.

This is an interesting piece of work that makes maximum use of data from comparative anatomy. It is a fine achievement. All the same, it should be emphasized that *Homo sapiens sapiens* probably goes back about —100,000 years (Qafzeh, Omo PHS and KHS). This may change certain aspects of the problem, for language is primarily a matter of consciousness and learning. Anatomical suitability is necessary but not sufficient.

The system of sound by means of which spoken language is conveyed comprises the following elements: (a) breath (lungs and diaphragmatic and costal muscles); (b) sound tubes or conductors of variable diameter (pharynx and particularly vocal
cords, which presuppose a very selective and consciously operated control center—the role of the mind is very important); (c) resonators (nasal and oral cavities); (d) modulators (tongue and lips, which presuppose a very fine nerve structure and encephalic control of the increasing frontal convolution). I would have liked Krantz to have kept this in mind in order to avoid reducing the problem to "skeletal" details.]

by C. B. STRINGER

Department of Palaeontology, British Museum (Natural History), Cromwell Rd., London SW7 5BD, England. 16 v 80

The search for a kind of anthropological "philosopher's stone" which would almost magically transform archaic or Neanderthal forms of man into anatomically modern Homo sapiens has been continuing for many years. I do not find Krantz's suggestion any more convincing than that of anterior dental reduction. The deus ex machina chosen by him is the invention and rapid spread of phonemic speech and selection for anatomical features associated with a downward elongation of the pharynx. I will not dwell on the anatomical problems of interpreting the form of the vocal apparatus in fossil hominids, which have been discussed fully elsewhere (Falk 1975, DuBrul 1977), but I would certainly point out that not all premodern hominid crania show the extreme flattening of the cranial base and "open" sphenoidal angle of some of the Neanderthals. Even if we discount the reconstruction of the Steinheim basicranium (Crelin 1973), it is apparent that the Broken Hill and Saccopastore crania do display certain basicranial features found in modern crania, and this has been interpreted, correctly or not, as implying that they may have possessed an upper respiratory system more like that of modern adult H. sapiens (Laitman, Heimbuch, and Crelin 1979). I believe that anatomical study of other Middle and Upper Pleistocene crania (e.g., Petralona and Djebel Irhoud 1) could demonstrate the presence of a basicranial morphology more comparable in some respects to that of modern man than is that of the Neanderthals. Thus the extreme Neanderthal basicranial form (and it should be remembered that there is considerable variation in this respect in Neanderthals alone) is probably not a primitive condition but may be derived and specialised compared with the inferred ancestral morphotype. Since it is difficult to believe that the Neanderthals would have evolved a less effective vocal apparatus at a time of increasing behavioural complexity, I believe that Neanderthal basicranial morphology was dictated by functional considerations other than pharyngeal form (although such a morphology could certainly have imposed constraints on the linguistic repertoire).

Krantz treats the appearance in the fossil hominid record of his 15 "modern" anatomical characters as punctuational rather than gradual. He admits that 1–3 appear before anatomically modern H. sapiens (and, incidentally, are not restricted to Neanderthal crania); but certain Neanderthal crania (e.g., the Shanidar sample [Stringer and Trinkaus n.d.]) display characters such as 7, 8, 9, 10, 14, and 15 as well. Furthermore, although Krantz dismisses claims for the precociously appearance of elements of the modern cranial form, the Omo (Kibish) 1 cranium displays all of the "modern" characters in the parts preserved (i.e., 1–3, 6–10, 13, 15), and the evidence from Border Cave and Klases River Mouth in South Africa is also suggestive of an early Upper Pleistocene appearance of at least some of these traits (Rightmire 1979). Furthermore, while the appearance of phonemic speech could be related to the proliferation of cultural complexity in the Upper Paleolithic of Europe, it is not clear how explain why skeletons of fundamentally "modern" morphology are present at Qafzeh and Skhul in a Mousterian cultural context. Here an anatomical form containing most, if not all, of Krantz's 15 characters is related to a culture and inferred behavioural patterns little different from those of the Neanderthals. We must, then, ask Krantz why the cultural evidence is apparently lagging behind the anatomical changes or why in southwestern Asia there is a morphological dichotomy between Neanderthal and anatomically modern fossil hominoids without good evidence of a behavioural dichotomy. Finally, although there is not the space to discuss it here, I would question Krantz's dismissal of the "migration-intermixture" model of the evolution of anatomically modern H. sapiens. Reliance on the kind of nonmetrical traits used by Coon (1962) to establish continuity in East Asia is extremely dangerous given the problems posed by the complex interaction of environmental stress and genotype on the expression of such traits (Corruccini 1974, Trinkaus 1978). Additionally, Krantz's lumping of diverse fossils into his H. erectus category obscures real differences that display from each other and leads to the same kind of confusion as that caused by lumping all late Middle and early Upper Pleistocene hominids into the category "Neanderthaldoids."
Krantz: sapienization and speech

make better auto mechanics (the fact that many auto mechanics never read underlines the secondary importance of information). Second, although some learning may be relevant to the complexity of tools (see Guilmet 1977), it is not the only factor. Intelligence, in the sense of organizing ability, is crucial. It is indispensable that the knapper be able to conceive of the tool and the process necessary to achieve the end result. It is not necessary that he be able to talk about it. The prerequisite conceptual ability for such "complex" tools ashafted points and composite tools was probably achieved 200,000 years before the Upper Palaeolithic (Wynn 1979). In sum, the invention of the phonemic principle is not necessary for the invention of Upper Palaeolithic technology, nor is it sufficient. Krantz does not make a convincing connection between rate of speaking and culture change.

His argument may be unconvincing in part because the invention of phonemes is unconvincing as a cause for change. Implicit in Krantz's argument that superior language facilitates many behaviors is the idea that the only thing holding back cultural change is an inadequate language and that as soon as this barrier is breached culture will advance—including an increase in population and expansion into inhospitable areas. In other words, progress will occur. Progress is not a reasonable cause for culture change. Even if couched in terms of adaptation, Krantz's phonemic principle is at most a constraint to culture change and, as I have tried to show, not a very convincing one at that. The cause of culture change must lie elsewhere.

Reply

by Grover S. Krantz

Pullman, Wash., U.S.A. 17 vii 80

The comments include a gratifying spread from approval to disapproval, with most of them offering criticisms that might modify or improve the concept. As an original contribution to our thinking about human evolution, my paper was bound to elicit reaction, and I am pleased to see that the reaction has been both wide and varied. The comments range from approval to outright rejection, and I appreciate the efforts of both the critics and the proponents to improve the concept.

The most common complaint is that some early skulls and teeth are depicted as sapiens, which is beyond 20,000. It must also be noted that we are dealing with an ancient common origin postulated here was some 40,000 years ago and even glottochronology makes no claims of city.

It is argued that languages are difficult for people to adopt. In other contexts, several linguists have argued exactly the opposite with me. In any case, the parallel is poor because I am referring not to the learning of a second language, but to something more like the learning of the first. A better comparison is with our children, who gain fluency in a spoken language in just a few years.

One reader asks why, if speech has a single origin, we do not find evidence of a common world vocabulary. Another points out that we have such evidence, as presented by Foster (1978). This evidence is indeed slim and arguable, but it must be remembered that the common origin postulated here was some 40,000 years ago and even glottochronology makes no claims beyond 20,000. It must also be noted that we are dealing with phonemic speech, which could be overlaid on any preexisting set of morphophones. It is not obvious how much of the phonemic vocabulary should have spread along with the concept itself.

In the area of archeological interpretation, several commentators indicate that improved cooking methods and tools served to relax selection pressure for maintaining large teeth and jaws. Worldwide changes of these kinds, abrupt and simultaneous, would overlap with the cultural changes I have focussed on. Separating speech and social organization from cooking and tools may not be possible at present. More to the point is that masticatory reduction per se does not adequately explain the other changes from primitive to modern cranial design. In fact, the leverage of the withdrawn sapiens dentition results in a mechanically stronger bite, all else being equal, than in the more projecting erectus jaws. The most conclusive evidence would be to show that occlusal wear decreased, along with the reduction in tooth size, but it actually increased with the appearance of modern man in Europe (Smith 1977). This alone shows that tooth reduction was not a response to a lessened need for maintaining tooth size, but occurred in spite of the more rapid wear resulting from it.

Two readers point out that we have a few good sapiens skulls (by my definition) dated well before 40,000 years ago. My first impulse is to respond, as Weidenreich did to the same kind of problem, that if the morphology does not fit the date, then the date must be wrong. We must remember that 30 years ago any acceptable phylogeny had to account for Piltdown man, the "fully modern" Swanscombe skull and Galley Hill skeleton in the Middle Pleistocene, and progressive Neandertals who were more modern than later forms. More soberly, we are faced with two choices: either a very few dates are wrong, for currently known reasons, or else the sapienizing change began early in southern Africa and some geographical aspects of my interpretation are wrong. Obviously, I'm betting on the former.

One reader calls attention to the contradiction that sapiens skulls from Qafzeh and Skhul are associated with Mousterian tools. Assuming that these associations are direct, the only accounting is to note that this is just lithic technology and does not necessarily reflect social organization. This is the opposite of the situation at the Vindija site in Yugoslavia, where the technological change preceded the morphological. I would like to see more analysis of the total way of life in these late Mousterian sites where there appears to be a major change that is not easily seen in the lithics.

In the area of linguistic behavior, the most common objection is that tool-making techniques are learned more by observation than by verbal instruction. This is largely correct, and some good examples are given. What has been missed here (and I thought it was obvious) is that the instructor explains mainly why a tool is made, not how. Words are used to give the reasons for each step in a complex technological process while the eyes can usually follow the actions themselves.

The distinction between symbolic language and speech has been either missed or misunderstood by some readers. The basis of symbolic behavior is having mental images of things that are not being directly and currently perceived by the senses. All problem solving by mental trial and error relies on this ability, and it would have been of great use to early man, even without speech. (We find no direct evidence of this in the form of cave paintings simply because there are no preserved cave walls over 30,000 years old.)

It is argued that languages are difficult for people to adopt. In other contexts, several linguists have argued exactly the opposite with me. In any case, the parallel is poor because I am referring not to the learning of a second language, but to something more like the learning of the first. A better comparison is with our children, who gain fluency in a spoken language in just a few years.

One reader asks why, if speech has a single origin, we do not find evidence of a common world vocabulary. Another points out that we have such evidence, as presented by Foster (1978). This evidence is indeed slim and arguable, but it must be remembered that the common origin postulated here was some 40,000 years ago and even glottochronology makes no claims beyond 20,000. It must also be noted that we are dealing with phonemic speech, which could be overlaid on any preexisting set of morphophones. It is not obvious how much of the phonemic vocabulary should have spread along with the concept itself.

In the area of anatomical problems has elicited the greatest criticism. The most common complaint is that some early skulls (Broken Hill, Steinheim, and Sale) have modern cranial bases. This assertion depends on exactly what one is looking at. Broken
Hill shows some modern traits that may have been within the *erectus* range of variation (and were rapidly selected for later); alternatively, the specimen may be from a population which was in the process of transition (whatever its date). The Steinheim cranial base can be reconstructed within broad limits that could include an essentially *erectus* design, with due allowance for the allometries of its small size. I have not seen the Salé skull.

I have been criticized for accepting Lieberman's reconstructions and criticized for rejecting them; enough said. Along this same line, it has been asked why human infants don't have pronounced browridges if their vocal anatomy is so apelike. Browridges result from the relative sizes of face and braincase. In ontology it is necessary that the adult brain grow at an early age in order to function at all. No matter how primitive its facial design, the infant's brain is relatively so large that it overhangs the face. Conversely, a microcephalic with fully human vocal anatomy will have browridges because its brain is so small relative to the face. The contrasts described in the figure are between adults with brains over 1,000 cc.

More serious is the observation that the cervical viscera are not physically tied to the vertebral column. The fact that they behave as though they were so attached is perhaps not a satisfactory answer. I would rather ask other anatomists if positions of innervation or vascularization are critical here or if the esophagus must retain its absolute length. This merits further investigation.

Another well-made point is that dental withdrawal is medial as well as posterior. If this shift were only to the rear it would greatly change the shape of the dental arches, and this might involve difficult problems of tongue shape and masticatory movements. This also merits further investigation.

I made no reference to neurological restructuring, and this has bothered a few readers—I had expected more. The subject area of this paper emphasized those aspects of vocal anatomy that distinguish man from apes. (Thus I did not deal with breathing, vocal cords, resonators, or tongue and lip mobility.) Certainly there would have been some species-specific changes related to the production, reception, and decoding of speech sounds, but at present there are no suggestions of how these might be read or inferred from the fossil record.

One reader questions my rejection of the gene-flow—selection model for the transformation. I presented it in terms of a series of breeding populations, instead of an interbreeding continuum over the world, because that seemed more realistic on a descriptive level and because it served to estimate the actual rate of propagation. It is true that intermittent flow between such populations would have the same effect as a constant flow at a lower selection rate. I would welcome further discussion on how fast such genetic changes could flow and select over the world.

Another commentator prefers to see the transition as being accomplished more in the form of actual human movements. That some line traits in the East Asian area do not hold up well at an early age in order to function at all. No matter how primitive its facial design, the infant's brain is relatively so large that it overhangs the face. Conversely, a microcephalic with fully human vocal anatomy will have browridges because its brain is so small relative to the face. The contrasts described in the figure are between adults with brains over 1,000 cc.

References Cited


Endo, B. 1970. Analysis of stresses around the orbit due to masseter and temporalis muscles respectively. *Journal of the Anthropological Society of Nippon* 78:251-66. [AN]


Falk, Dean. 1975. Comparative anatomy of the larynx in man and...