

Notes and Comments

On Falk's 1989 Accusations Regarding Holloway's Study of the Taung Endocranial Cast: A Reply

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In a recent article in this journal, Falk (1989) accuses me (Holloway, 1988a) of misrepresenting her identification of the lunate sulcus on the Taung endocranial cast, ignoring her measurements of chimpanzee brains, and attributing my own measurements to her. I am obliged to reply, because her accusations impugn my honesty and integrity.

1. Falk complains that I excluded from my tables and statistics her estimates of the OP-LS/OP-FP ratios (tape-measure distance from occipital pole to lunate sulcus, divided by that from occipital pole to frontal pole) of a chimpanzee brain in her personal collection. Indeed I did, since I have never seen this specimen and do not agree with her measurement techniques.

2. Falk protests that "the ratio Holloway attributes to me for Taung is 0.254, not the correct value of 0.242." This is not true. Nowhere in my 1988a paper did I attribute the 0.254 ratio to Falk. The ratio to which Falk objects is based on my measurements from the dimple that she supposes to represent the lunate sulcus (Falk, 1980, 1983, 1985). Both the measurement and the ratio are listed in my Table 1 (Holloway, 1988a) along with all my other measurements, and are not attributed to Falk. The only thing attributed to Falk is the identification of the dimple. As in all of my papers, (Holloway, 1981, 1984, 1985, 1988a) the phrase "Taung LS (per Falk)" in Table 1 refers to the identification and location of the supposed sulcus, not measurements from it to other landmarks. (Cf. Table 5.1 of Holloway, 1988b, which shows differing arc measurements "per Holloway" and "per Falk.")

3. Falk objects to my having drawn a line on a Wenner-Gren cast of the Taung endocranial and described it as "the Taung lunate sulcus proposed by Falk." I should have written—as I wrote in previous publications—that this "mark is where Falk proposes the

lunate sulcus to be" (Holloway, 1981, p. 48). However, the choice of wording makes no real difference. It is clear from Falk's diagram (1989, Fig. 2) that the line on my photograph is in the same place as the oval patch of stipple on her drawing. Informed readers will realize that both my short line and Falk's stippled ellipse can only be markers of the sulcus's position, not representations of the sulcus itself (which would presumably have extended to the midsagittal plane above and almost to the sigmoid sinus below). I have published the same photograph in the past (Holloway, 1981), and displayed slides and the marked cast itself at meetings when Falk was present (e.g., at the 1987 Stony Brook conference on "robust" australopithecines), without occasioning any protest from Falk.

Falk's current protest concerning this third point may be justifiable as a quibble, but it is irrelevant to our real disagreement, which concerns the relative length of the OP-LS arc of the Taung endocranial and the degree of its difference from that of chimpanzees. Our estimates of Taung's arc length differ by only 2 mm. I have suggested (Holloway, 1988a) that the difference may reflect where we locate OP rather than LS. So far, Falk has ignored this suggestion.

The tiny difference over which we are arguing is of interest solely because Falk's slightly lower estimate has the effect of lowering the Taung specimen's OP-LS/OP-FP ratio enough to place it almost within the range of my chimpanzee sample. This fact is immaterial to my contention that there is a significant difference between *Pan troglodytes* and *Australopithecus africanus*. Minimal overlap is hardly surprising in morphometry, and overlapping distributions are often significantly different. The undeniable fact, which is just as clear from Falk's (1989) Fig. 1 as from my own analyses, is that Taung plots above the upper limit of my sample of 32 chimpanzee brain hemispheres (which, once again for the record, includes young juveniles as well as adults).

Falk's chimpanzee sample consists of a single juvenile. She claims an unusually high OP-LS/OP-FP ratio for this specimen on one side (but not on the other). Even if her claim is correct, this atypical datum lies

more than three standard deviations outside my sample mean, and must on the basis of our present knowledge be interpreted as representing the extreme of the chimpanzee distribution. It has no effect on my statistical reasoning. One could in fact predict from my statistics alone that some chimpanzee brains would show ratio values exceeding the Taung datum.

It is not possible to say how widely the distribution of the OP-LS/OP-FP ratio in *A. africanus* overlapped with the chimpanzee distribution, because we do not know what the australopithecine distribution looked like. However, the reasonable conclusion from my data is that if the Taung australopithecine had a lunate sulcus, it was significantly more posteriorly placed than that of living pongids. If so, it shared this derived character state with *Homo*. It is also possible that it had no lunate sulcus at all, as is the case for most humans.

The issue of the Hadar AL 162-28 endocast is peripheral to this dispute, but it presents some interesting parallels. Falk's drawing of this endocast (Falk 1985b, Fig. 1) shows a patch of stippling labeled as the lunate sulcus. The feature in question is far less evident on the endocast than her figure suggests. She contends that the sulcus lay about 5 mm in front of the lambdoid suture. Holloway, (1983), Holloway and Kimbel (1986), and Boas (1988) examined the fossils and independently concluded that the sulcus lay in a slightly more posterior (i.e., humanlike) position, only 2 mm in front of the suture. Falk (1989:338) has retorted that all three of us are putting our calipers down wrong or are simply mistaken.

Falk (1989:339) concludes her most recent riposte by simply reiterating her opinion that "all australopithecine natural endocasts appear ape-like in their sulcal patterns." In fact, none of them show any discernible *Pan*-like details (Holloway 1981: 56-57, 1988a:32). Falk's unsupported opinion concerning these other casts is immaterial to our debate over the Taung specimen. So are the current controversies she cites concerning its supposedly apelike dental ontogeny. Settling those controversies one way or the other will imply nothing whatever about the position of the lunate sulcus on the endocast. The remarks of Tobias (1987) that Falk quotes are equally immaterial; they represent nothing more than a failure to find a credible lunate sulcus, a failure earlier reported by Clark (1947). None of these find-

ings are at variance with my own (1981:49, italicized statement: "We cannot prove where the lunate sulcus is located, but only demonstrate where it is not." The historical context of this controversy has been more fully presented elsewhere (Holloway, 1985).

I find it vexing that these issues have been approached with so much rancor and sarcasm by Falk in the past. In her 1989 paper, she continues in the same vein but descends for the first time to questioning my honesty and integrity. The grounds adduced for her various complaints strike me as being either specious or trivial. I cannot help but feel that if the Editor and referees of the AJPA had read my earlier articles carefully, this attack on my integrity would not have been published, and this further exchange would not have been necessary.

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