



## Commentaries

### Science and Antiscience: In Response to Kenneth Weiss

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Aside from characterizing it in passing as “politically correct” and “antiscience,” Kenneth Weiss is rather kind in his review of my book, *What It Means to be 98% Chimpanzee*. Nevertheless, he devotes nearly half of the review to criticizing my criticisms of two bodies of work: the Human Genome Diversity Project (HGDP) and DNA hybridization.

As to the first, Weiss is an interested party, having coauthored some of its earliest promises (Weiss et al. 1992), and later withdrawing from its leadership. I would happily read an account of what led to his resignation from the HGDP.

As to the second, I am an interested party, as Weiss rightly notes that I “repeat [my] decade-old accusations” (Weiss 2004:418) against the work of Charles Sibley and Jon Ahlquist. Curiously, he cites only their 1987 article, and not the subsequent 1990 article, in which they acknowledge that they had indeed subjected their data to unreported manipulations, which significantly altered the conclusions, as per those accusations (Marks et al. 1988). He tries to sidestep the central question here with the clause, “Whether or not his characterization of their work is accurate” (Weiss 2004:418), but I would suggest that coming from a senior representative of the molecular anthropology community, that position is unwarranted.

The unreported statistical treatments described in Sibley et al. (1990) include discarding and substituting experimental controls and moving correlated points into the regression lines describing them and then treating them as independent data points. The researchers acknowledged that these manipulations determined the conclusions and

that they had failed to mention any of this unconventional methodology in their copious publications. They also subsequently withheld the data from many other interested scholars. The best case that could be made is that these odd problems arose entirely independently of one another.

Finally, Weiss succinctly articulates his view that, removing gorillas from the midst, "the closer relationship between chimps and humans is by now accepted" (Weiss 2004:418). But three things are clear: (1) There are plenty of genetic data that fail to unite humans and chimps, separately from gorillas, strongly implying that simple dichotomous models are inadequate to describe the biological history of these genera (Barbulescu et al. 2001; Chaline et al. 1990; Marks 1993; Ruano et al. 1992; Satta et al. 2000); (2) the most zealous advocates of the Sibley–Ahlquist work have tried to diminish the fraud accusations with the defense that they got the correct answer, so it was all OK, and have disproportionately advanced work that appears to link human and chimp (Pilbeam 1996; Ruvolo 1995; Wildman et al. 2003); and (3) the history of biological anthropology is riddled with false consensus that reflected derivative work citing other derivative work and deference to power, rather than the intensive application of the critical faculties by the field's intellectual leaders (Proctor 2003).

Of the two points raised at length in Weiss's review, then, ultimately the only place for DNA hybridization is alongside Piltown Man; and his genetic consensus of human–chimp is of a piece with the old paleontologic consensus of the hominid status of *Ramapithecus*. The cause of science, especially anthropological science, might be better served by its senior spokesmen worrying a little less about "science bashing" and a little more about the primary evidence and literature. Interested readers can see my website for details about the Sibley–Ahlquist work at <http://personal.uncc.edu/jmarks/DNAhyb/DNAhyb.html>.

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