

Untangling the Issues: A Reply to Dr. Stringer

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My opponent in the modern human origins debate seems displeased with my review (Wolpoff, 1994a) of a book he co-edited (Aiken et al., 1993). I regret any unpleasantness it may have caused him, but see no particular reason to retract anything I said in it. I concur with his suggestion that the reader consult the recently published exchanges on the origins issue—the allegations made by Stringer and like-minded colleagues are answered in full—but this will not quite suffice. In his remark, a public presentation that was one side of a public debate is inappropriately compared with a book represented as the latest word on modern human origins and dating (I do not intend on reviewing the text of this book again, but the cover jacket describes it as “explor[ing] the debate over the ‘single center’ hypothesis of human origins versus ‘multiregional evolution,’” and promises that the “growth of doubts about interpretations of the new evidence by some workers . . . [will] be aired and discussed”). Moreover, Stringer’s remarks review some history and raise additional issues that it is appropriate to expand on.

The symposium I organized for the 1990 AAAS meetings with my colleague A. Thorne (session 14-3, in Games [1990, pp. 20–21]) was prompted by a 1988 NOVA special on modern human origins that only presented the Eve theory (this evidently did not satisfy NOVA’s appetite for one-sided presentations on human evolution) and a plenary talk given before the AAAS the previous year by the late A. Wilson. We organized our symposium to publicly review the other side, in a debate that had become public because of these two presentations. But what

Lewin (1993), and Stringer in citing him, neglect to mention is that there were *two* symposia on at the AAAS that 18th of February of 1990. The second (session 14-2, in Games [1990, pp. 19–20]), on the morning of the *same day*, presented papers supporting the Eve theory by noted scientists such as A. Brooks, J.C. Long, and C.B. Stringer. Stringer is careful to cite Lewin’s comments on our symposium rather than making his own, thereby avoiding the appearance of complaining that he was not asked to read *two papers* on his Eve theory at the AAAS meetings on the same day. But this interpretation of his remarks does come to mind.

For a science writer Lewin seems surprisingly unaware of AAAS procedure. Our symposium was reviewed and approved by the AAAS, as all others are. The assertion that it is unusual to use a press-oriented AAAS symposium to present one side of a public debate is also surprising to hear from a science reporter, especially one who has continually taken strong positions on the issues he has reported, *particularly* the origins issue (see Lewin’s 1987–1991 references, and as his book makes more than clear). Perhaps he never attended AAAS sessions on creationism, cold fusion, or women’s contributions to science. However, placed in the context of the 1990 AAAS meetings, the (unmentioned) morning and afternoon modern human origins symposia each presented one side of the origins debate (no multiregionalist presented a paper in the morning session) and *together* they fully explored all sides of the issue as it was being argued at the time.

Today many of the arguments are quite different. One of the striking developments is the complete collapse of the genetic basis for the Eve theory (which, contra Stringer, was not dubbed by me but by A. Wilson, for instance, the title of his above mentioned AAAS talk was “The Search For Eve”). Ad-

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ressing the genetic evidence, Stringer tenaciously urges his readers to keep their eyes on some recent publications by geneticists. I enthusiastically agree, and suggest we begin with the first of the references he cites, by H. Harpending and colleagues (1993). These authors state (p. 495):

The present data are clearly inconsistent with the strong Garden of Eden hypothesis. If there was indeed a single large expansion from Africa about 100,000 years ago, we should see the signature of it in the mtDNA differences, but instead we see indications of multiple later expansions associated with modern technology instead of modern morphology.

He did not mention Templeton's (1994) most recent statement on the issue, but readers might benefit from examining this as well. In fact readers are *urged* to look over the interchanges that have already been published. Please consult Wolpoff and Frayer (1992), Wolpoff (1994b), Frayer et al. (1993), and, as a sort of grand finale, Frayer et al. (1994).

P. Bahn (1994), in his review of the Aitken et al. (1993) book, raises the issue of how participants in the Royal society conference that was the basis of the book were chosen: "We have no idea whether the chief proponents of the multiregional hypothesis were invited, and if so whether they were unable to attend or simply declined to do so." In his letter, Stringer makes potentially misleading comments on this question and I believe it is necessary to clarify what actually transpired:

1. I was invited to the Royal Society conference, just as Stringer asserts—he wrote me that while I was welcome to come at my own expense and sit in the audience, there would be no room for me in the program (it was unclear whether questions would be allowed).
2. Alan Thorne informs me that he received no invitation to speak at the CIBA symposium, but was also offered a seat in the audience for the Royal Society conference.
3. Wu Xinzhi was not invited to the conference.

Unlike the AAAS there was no corresponding afternoon symposium on multire-

gionalism at this conference and neither the organizers, the scientists invited to attend, nor their audience were exposed to the multiregional evolution model or its biological, genetic, palaeontological, or archaeological bases. The price of not listening can be not understanding, and perhaps this explains why over the years Stringer has created a veritable cottage industry that manufactures "Multiregional Evolution" theories he can disprove. There was the "parallel evolution" theory (Stringer and Andrews, 1988; Stringer, 1989), the "multiregional model" that differs from a "gene-flow model" (Stringer, 1990), the "universal multiregional model" (Stringer, 1993, in the Aitken, Stringer, and Mellars volume), and the "multiregional continuity" model (Stringer and Gamble, 1994). In this latest representation the strategy is to cast the Multiregional Evolution argument in a non-evolutionary framework, and thereby use evidence of evolution to "show" it is wrong. To wit:

Supporters of multiregional continuity have to resort to explanatory mechanisms such as increases in symbolic behavior, global population contact, or notions like Brace's culinary revolution. In this way they either speed up the clock or slow it down as necessary in a desperate attempt to keep clinging to the wreckage of their argument (Stringer and Gamble, 1994, p. 97).

To accept this logic would require the interpretation that continuity (using their term for our Multiregional Evolution model) means identity, and that evidence of change therefore disproves it. But continuity, in my dictionary, means "uninterrupted connection" and in any event there is a reason why we call our model *Multiregional Evolution*—just to make sure that the opposition will not so miscast it as to be able to suggest that *evolution disproves it*. Clearly we have failed, and may well write yet another paper or present another symposium to explain our model and detail why it works while the other theories do not. Perhaps Stringer should consider the position of one of my friends and colleagues, who has promised to accept the Multiregional explanation if we promise to stop writing about it.

It has been an interesting but uncomfortable experience to debate Stringer on these issues. The discomfort comes from the tone that has crept into the exchanges. He is so sure the exchange in this journal will be acrimonious that as part of another debate, already published, he has already complained about the tone of this reply—even before it was written (Stringer and Gamble, 1994, p. 113). But the source of inflammatory rhetoric is not with me.

The problem creating the heat in these exchanges is what to do when your theory is wrong. When the East Turkana cranium ER 3733 was discovered, Richard Leakey gave Alan Walker permission to carry a cast to Ann Arbor immediately so that I could see it. Alan very kindly did so, and with evidence of that specimen contemporary with *Australopithecus boisei*, I knew the single species hypothesis was wrong. I had been invited to present a paper at a paleoanthropological congress in Nice the following summer. I scrapped the title I had submitted and instead read a paper on the fact that the theory was wrong, and why. Admitting falsified theories are wrong is the best solution. It creates progress in the development of paleoanthropological explanations, and promotes good will among colleagues. I highly recommend giving up and moving on when the time has come.

Aitken et al. (1993) was advertised as exploring a debate and discussing and airing the doubts some workers expressed over recent evidence. It did not live up to this admirable description. R. Klein, in a forthcoming review in the *International Journal of Primatology*, notes that it “suffers from . . . the absence of a chapter by a strong advocate of the Multiregional Theory.” P. Bahn’s (1994) review finds the book “astonishingly one-sided.” He notes that “everybody knows that the debate is polarized . . . [but] I found the lack of a single paper to represent the opposite case disturbing.” The uncomfortable position Stringer now defends stems from the fact that he refused to do what several colleagues urged him to do. Bahn perhaps put it best:

It would have been useful and magnanimous to close the book with a critical appraisal by an arch multire-

gionalist. Readers could have made up their own minds, instead of being spoon-fed by only one side.

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