REACTIONS

Readers are invited to submit their reactions to the articles in our journal. Please address contributions to: ORIGINS, Geoscience Research Institute, 11060 Campus St., Loma Linda, California 92350 USA.

Re: Lugenbeal: Ancestral Dissonance (ORIGINS 3:52-55).

Revisiting scenes from my early life, I recently came across Mr Lugenbeal's review of Charles Oxnard's book on human evolution.[1] The review is much taken up with the role of multivariate analysis in palaeontological research, and refers to contributions by "Lang and Bronowski." I happen to be the former, and thereby the subject of a couple of typos: I'm Long, not Lang. This can be confirmed by viewing the 1951[2] and 1953[3] papers in the journal *Nature*, through which, coupled with an article in *The American Journal of Physical Anthropology*,[4] Bronowski and I launched multivariate analysis in the field of palaeontological research.

Our papers were triggered by the long-running dispute between Solly Zuckerman's Birmingham school and Le Gros Clark and others on the origins of certain African fossils. As Mr Lugenbeal says, Solly was a vociferous advocate. He had harsh and personal things to say about the parts played by Le Gros Clark and Bronowski in a battle over statistical evidence, but his remarks appeared in his autobiography[5] long after the latter were alive and able to defend themselves, and have since had wide circulation, not least via the Web. As the sole survivor of the four main protagonists in the battle, perhaps a few comments from me would help to restore the balance.

The argument between Zuckerman and Le Gros Clark was in full swing when Solly played the statistics card. He selected a set of dimensions on the milk canine tooth which he judged would bring out the differences between humans and the anthropoid apes, and measured these dimensions on numbers of teeth from each of the four species, to characterise their populations. He then carried out statistical significance tests, and announced that the fossil dimensions didn't differ from those of the apes. Le Gros Clark refused to accept this, and wanted to reply in kind. He had examined the Kromdraii and Taungs' teeth and decided that they looked human, not at all like those of apes. He selected a set of four dimensions of his own which he thought would confirm this. No statistician himself, he turned to Bronowski for help. Now "Bruno" understood very well the logic and power of statistical methods, and was a

persuasive advocate of their use, but he was not himself a practising mathematical statistician. However he had a personal assistant, myself, who was. It was immediately clear to me that Zuckerman's statistical analysis was quite primitive, being based on significance testing of each of the several individual dimensions on each tooth, with no allowance for the correlation between dimensions. (If one dimension happens to be large, then so, probably, will be others, so that his sets of significance tests were not internally independent, their multiplicity merely adding confusion to the argument.) The correct method for the task was clearly multivariate analysis, which, thanks to its development by the Indian statisticians Mahalanobis, Rao and others, was just then starting to come into prominence as a basic statistical technique. We obtained from Le Gros Clark what was needed for such an analysis of *his* sets of measurements. The results, set out in our 1951 paper, confirmed that the two fossils were indistinguishable from Le Gros Clark's human group, and that by no means could they have come from any of his chimpanzee, gorilla and orangutan groups.

The question remained, why Zuckerman's statistics, crude as they were, had led to so different a conclusion. We did not have to wait long for the answer. The opposition soon confessed that they had bungled their analysis, forgetting to divide by the square root of 2 at a critical point. The then-editor of *Nature* wrote us saying with some amusement that this seemed to end the matter, and that no further comment was called for. In his autobiography, Solly tried to play down the mistake, saying that it didn't really matter, and moreover that his statistical adviser Frank Yates had made "strictures" concerning our paper. Personally, I never did understand why Yates, a distinguished statistician in his own right, had sanctioned Solly's method, nor why he hadn't uncovered the arithmetical blunder until our paper appeared. As to "strictures", none ever came to my notice; the only criticism I ever saw was a plea that multivariate analysis was unnecessary, that univariate methods were good enough. Anyway, nothing further about our work was heard from that side, either immediately afterwards or following the appearance of our more extended paper, two years later. (Nor, it is hardly necessary to add, did any further statistical analyses on the subject appear from Birmingham during that time.)

Returning to Mr Lugenbeal's article, may I venture a small — may I be excused the word — "stricture"? His elegant article is fascinating on many aspects of the long-running dispute, but it does rather invest multivariate analysis with a forbidding aura of abstruseness, complexity — "necessitating a computer" — and logical subtlety, even dubiety. It could discourage the uninitiated. Well, I gave up the profession of mathematical statistics and any active interest in palaeontology nearly fifty years ago, but I have not forgotten one thing: our

1951 and 1953 multivariate analyses didn't need a computer. They were carried out with the Monroe electric calculating machine of the day, a wholly nonelectronic device that would now only be found in a museum. The most it was capable of was accumulating sums of squares and cross-products. And during our 1953 collaboration this elementary machine proved quite adequate for a two-dimensional projection graph of the teeth populations. (We never bothered to publish this; I produced it purely for Le Gros Clark's benefit, as a demonstration of the illuminating things multivariate analysis could do. Was it perhaps the first application of multivariate graphics in palaeontology?)

On the score of logic, as I see it the only subtlety involved in multivariate analysis is that of the significance test, and this is no more than the staple test of everyday univariate statistics. Logically, the advance from univariate to multivariate analysis seems simply to parallel the advance, in school algebra, from "x = ax + b" to simultaneous equations. I'd be interested to hear if the computer has altered this in any fundamental way.

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Endnotes

1. Lugenbeal EN. 1976. Ancestral dissonance: literature review. Origins 3:52-55.
2. Bronowski J, Long WM. 1951. Statistical methods in anthropology. Nature 168:1116-1118.
3. Bronowski J, Long WM. 1953. The Australopithecine milk canines. Nature 172:251.
4. Bronowski J, Long WM. 1952. Statistics of discrimination in anthropology. American Journal of Physical Anthropology 10(4):385-394.
5. Zuckerman S. 1988. Monkeys, men and missiles: an autobiography, 1946-1988. London: HarperCollins.