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SCIENTIFIC INTEGRITY, CONFLICT OF INTEREST AND MYTHOLOGY RELATED TO ANALYSIS OF TREPONEMATOSES: THE IMPORTANCE OF BEING ERNEST

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I will cite the 1904 words of my ancestor, Walter Rothschild (1983, p. 133): 'I am always willing to accept every criticism so long as I am allowed to defend my own standpoint in return. I therefore venture to ask you to receive this reply to' Gary Heathcote.

The important thing in any debate is that it is a debate and neither side should be permitted to suppress the other, no matter how much it might want to. Manipulation of the publication process to interfere with dissemination of conflicting ideas is contrary to acceptable science.

Criteria based on empirical observation with a validated methodology forms the basis of scientific method and indeed, for the Rothschild criteria for distinguishing among the treponematoses (Rothschild and Rothschild 1995). Objective physical chemistry-based measurements confirm not just precision of periostitis recognition by the authors' technique, but also its accuracy (Rothschild and Rothschild 2003). This is the only non-destructive technique documented for distinguishing periosteal reaction. Mike Schultz' (1995) beautiful histologic sections clearly document the vagaries revealed by Mary Powell (1995), when she reported interobserver variation from 0-100 % frequencies on the same skeletal sample! The abstract did not reflect her presentation. I was fortunate to attend and witnessed her documentation of the lack of reproducibility of previous work on periosteal reaction, something I had only previously suspected. Why are anthropologists apparently not appalled by the non-reproducibility of their techniques? Why work so hard to discredit a technique that they have not even tried - a technique validated for recognition of periosteal reaction and of distinguishing among the treponematoses? When taphonomy and in vivo periosteal reaction cannot be distinguished, there can be no progress.

As there has been resistance to use of validated standardized criteria for recognition of periosteal reaction, there should be no surprise that great efforts have been exerted to denigrate diagnostic cri-

teria for recognition of groups of diseases known as treponematoses. Does the level of discomfort that scientific methods seem to elicit simply relate to failure to understand its precepts?

While those attempting to repudiate validated empirical criteria decry being perceived as mythologists that does not change the fact of lack of scientific evidence for their opinion-based pronouncements. Let's there be any confusion, let us be clear that this is what is at the basis of comments by Heathcote, Stodder and Powell (Heathcote et al. 1996; Powell and Cook 2005; Rothschild and Rothschild 1999).

Could this problem of apparent antipathy to scientific method be responsible for exclusion of anthropology by the National Academy of Sciences? Would not adhering to scientific methodology and open debate be helpful in establishing the credibility of anthropology as a 'hard' science, or is fear of scientific scrutiny the driving force?

A second tenet of science is reproducibility of findings in a separate sample. This can be performed by dividing a sample according to a parameter unrelated to the testing technique or by examining different populations. One must be clear: Scientific scrutiny requires separate samples, not different investigators.

Accuracy in citations is always important. Thus, it is ironic when apparently inaccurate citations are misattributed by one of the original authors of those citations (Rothschild and Heathcote 1993).

Given documentation of yaws on Guam by Stewart and Spoehr (1952) and regional population characterization, the only meaningful question is if there is evidence for pre-Columbian existence of any treponematoses other than yaws in the South Pacific. There is not!

The value of any criteria is what you can learn from their application. Simply expressing belief in a diagnosis does not present a testable hypothesis and therefore allows no advancement in the state of knowledge! Untested assumptions become a

religion. Exemplary is the claim of skull lesion specificity for syphilis, with no supporting data! The skull lesion actually appears to be specific for treponematosi, but certainly not for which one. Science is based on data and criteria; religion and mythology, on belief.

The key is treating each disease as a separate species and defining all characteristics that are different. Distinguishing criteria that have proven reproducible in two sites/groups meets the scientific test. It is clear that there are 4 patterns periosteal reaction, including the null pattern.

Of course there are risks. We found tibial unique flattening in Guam. That subsequently proved to be a population, rather than disease phenomenon. But, that is exactly how science works. That one feature was found to be accurately described, but specific to people, not disease.

Without our criteria, what can really be said about the epidemiology and history of treponemal disease? Presence of treponemal disease, without distinguishing variety? Absence of any treponemal disease from an area obviates that issue. One could study the time course of its introduction, but therein lies the rub.

One of the unequivocal signs of treponematosi, although not specific, is periosteal reaction. Powell clearly documented the phenomenal lack of reproducibility of even recognizing periosteal reaction in the symposium she presented in 1995. Yet, it is ironic that the players in the current drama have so resisted an objective methodology. Recognizing periosteal reaction is an art that requires training to assure standardization. If as few as none and as many as 100 % of tibia are claimed to be affected, how can one even discuss those diseases in which it is so prominent? Criteria and standardized approaches are essential. When opinion-based approaches are critiqued as mythology as opposed to scientific, Powell would have you believe such an observation is histrionic.

Gary Heathcote is attempting to establish a 'simple classification' of reference errors, but in reality, he is source of the errors. The claim that 'assertions literally or essentially repeat a passage from an early work by Bruce and Christine Rothschild' (Heathcote 2005, this volume) does not actually cite the source of the citation. If one is going to cite references, the primary reference should be listed! The reference cited by Heathcote is not the source of the information. The information actual-

ly derived from Heathcote's own publication (Rothschild and Heathcote 1993)! Heathcote is actually criticizing his own prior statement!

Heathcote conveniently leaves out the documentation by Stewart and Spoehr (1952) and Douglas, Pietrusewsky and Ikehara-Quebral (1998) of yaws on Guam and the documentation that Cook was the first European to make contact with the people of Guam (Plato and Cruz 1967). Our citation of the 1988 Baker and Armelagos article (including the 1703 Dampier citation) was based on a fundamental principal of citation: As most articles begin with a title and end with references, anything in between can legitimately be related to that article.

If other citations are wrong, the error was that of Gary Heathcote, who provided the Howell's citations for his 1993 article! I admit that prior to his 1998 article, I had not considered the possibility that Heathcote could not be trusted, that he might present misinformation. As stated in 1999 (Rothschild and Rothschild 1999), collaboration is only as good as the integrity of communication between the parties. It is surprising when one party (Heathcote et al. 1998, 2005, this volume) chooses publication of undisclosed information variant to that disclosed during collaboration (Rothschild and Heathcote 1993).

Heathcote also misrepresents the content of his 1998 article. While attempting to champion the disproved concept that the various treponemal diseases were simply climatic representations of a single disease (Heathcote et al. 1998), Heathcote's only comment on the Guam findings was to question the dates - because a nearby site had different dates. Heathcote et al. (1998) pretended to offer fascinating new dates for the Gognga-Gun Beach Site, but there was a problem. Those dates were based on "materials found in (as yet) unspecified association with a minority subset of burials from the habitation area close to the preserved Latte site. These were not dates from the site (Rothschild and Rothschild 1999)! Heathcote (2005, this volume) would have you believe that the South Pacific is not an anthropological unit. The point is that historical data still document that yaws had a long-standing presence in the South Pacific and that it was only very recently eliminated - and subsequently replaced by syphilis.

Let us now examine the evidence that yaws was the only treponemal disease present in Guam prior to 1668. To our knowledge, that issue has

never been validly questioned. As a disease with almost complete population “penetrance” (everyone had it) and as syphilis does not replace yaws, until the latter is eradicated, “immigrants” and visitors from passing ships should not significantly impact an endemic disease. There also was an inherent inconsistency in Heathcote et al.’s (1998) use of the same argument to claim that yaws is a South Pacific disease and then to say that there is no evidence for long persistence of yaws on Guam.

Heathcote accurately notes that we cite his 1998 critique. It is total non sense to continue to not cite the rebuttal! Whoever convinced Heathcote (Heathcote et al. 1998) to represent himself

as the “collective consciousness” of physical anthropology (Heathcote et al. 1998) and to the intellectual equivalent of micturating into the wind (by attacking the citation he personally supplied) (Heathcote 2005, this volume) should be ashamed of themselves.

I can’t begin to express my disappointment with anthropologists who sacrifice science for mythology and attempt to distract. In conclusion, and again citing Walter Rothschild, ‘I only wish once more to urge strongly that the opponents’ of data-based scientific approach ‘before they criticise (the spelling of the time/country) and run down those who employ them, should for once seriously consider’ examining their own methodologic lapses.

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