Letters

The Holly Oak Shell

In his article "Mammoth fraud exposed" (Research News, 2 Dec., p. 1246), Roger Lewin makes several factual errors and inaccurately describes the history of the controversy surrounding the Holly Oak shell.

1) Lewin attributes a 1976 article (*1*) to John C. Kraft and Jay F. Custer. The article in question was written by Kraft and Ronald A. Thomas.

2) Lewin notes that Kraft and Custer "vigorously defended the pendant's authenticity in a subsequent exchange of correspondence with Meltzer and Sturtevant" (*2, 3*). At no time have we ever defended the authenticity of the artifact, but authenticity is one of the multiple hypotheses required by normal scientific analyses. In addition, we note that the web of innuendo and circumstantial evidence previously published by Meltzer and Sturtevant (*4*) has not proved that the pendant is not authentic. We invite interested readers to review the published correspondence (*2, 3*) to evaluate the degree to which the shell's authenticity has been defended. One of us (J.F.C.) has maintained since 1980 that the shell is a fraud and is publishing that opinion in a forthcoming book (*5*).

3) Lewin notes, "Meltzer told *Science* that during the past decade only one request was made to the Smithsonian Institution for permission to date the pendant, and that was using amino acid racemization, a notoriously unreliable technique." In 1976, and again in 1981, we proposed to conduct amino acid racemization (AAR) analyses on small samples cut from the Holly Oak pendant in order to determine whether the shell material was late Pleistocene, or Holocene, in age, these being the two most likely age options given the reported geologic setting for the artifact. When the proposal to conduct the AAR study was made, this chemical method of estimating sample ages was one of the few that could possibly be used on the small fragments that might be taken from the pendant. Our proposal to conduct the analyses was based on an objective plan to compare the Holly Oak shell enantiomeric ratios (D/L values) with those obtained on Pleistocene, Holocene, and modern shells of the same genus (*Busycon*) from the region. This is a standard and widely accepted approach to the use of amino acids in chronostratigraphy. Analyses of these control samples were performed before the request for the sample was made to demonstrate the age resolution of the method, which proved to be more than adequate for the purpose. We pointed out, however, that the D/L data might be difficult to interpret because of the preservation characteristics of the shell in the region where it would be sampled and because of the unknown effects of the chemicals used to "preserve" the shell. Our request was rejected by the Smithsonian both times, for reasons that remain unclear, although contamination of the shell through application of preservatives was cited as a potential problem in the rejection of the 1981 request. We can also note that our written request in 1981 received no timely formal reply until we telephoned the Smithsonian Institution on several occasions.

4) Although Griffin *et al.* (*6*) address the issue of recent contamination of the shell by preservatives, they do not address the well-known problems with radiocarbon dates on shell in the Middle Atlantic region (*7*). Until they do so, by providing some kind of control studies, their date is not conclusive, merely comforting. The possibility remains that the shell enclosed a living *Busycon* in the 19th century, as Sturtevant and Meltzer said: "radiocarbon dating of the shell is generally unreliable and would be particularly so in this case" (*2*, p. 244).

5) Lewin perpetuates the insensitive innuendo that because Hilborne T. Cresson committed suicide in a disturbed mental state, he was therefore capable of lying and perpetuating frauds. Such opinions should not be part of a scientific argument. Nor should they be uncritically presented by a deputy news editor employed by the American Association for the Advancement of Science.

In conclusion, although we applaud the fact that the Smithsonian Institution has finally allowed the kind of studies that we originally requested more than a decade ago, we deplore the way the results of the study have been reported.

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REFERENCES

1. J. C. Kraft and R. A. Thomas, *Science* **192**, 756 (1976).
2. W. C. Sturtevant and D. J. Meltzer, *ibid.* **227**, 242 (1985).
3. J. C. Kraft and J. F. Custer, *ibid.*, p. 244.
4. D. J. Meltzer and W. C. Sturtevant, *Mus. Anthropol. Univ. Mich. Anthropol. Pap.* **72**, 325 (1983).
5. J. F. Custer, *Prehistoric Cultures of the Delmarva Peninsula* (Univ. of Delaware Press, Newark, DE, in press).
6. J. B. Griffin *et al.*, *Am. Antiq.* **53**, 578 (1988).
7. J. F. Custer, *Archaeol. East. North Am.* **16**, 121 (1988).

Response: Although the tone of the Custer *et al.* letter is difficult to respond to, the specific issues raised are not. I will take them point by point, as in the original letter, bearing in mind that the central question here is the authenticity of the pendant.

1) The minor correction of the reference citation is welcome.

2) My statement that Kraft and Custer "vigorously defended the pendant's authenticity" was based on a reading of the 1976 article and the 1985 letter. For instance, in the 1976 article the pendant is described as "an interesting discovery pertaining to early man in the New World." The same article cites the opinion that most experts who have examined the pendant "indicated that they think this object is legitimate, and do not see any possibility of even suggesting the remote conception that it is a fake." The bulk of the long article concerned establishing the age of the pendant—10,000 years or 40,000 years—in the context of early man in the New World. Only one sentence mentions the possibility that it might not be an authentic early artifact.

The 1985 letter by Kraft and Custer was a response to the suggestion by William Sturtevant and David Meltzer that the pendant was not authentic. Kraft and Custer's letter begins by stating that "We find nothing new or persuasive in their arguments" and goes on for a full page in an apparent attempt to demolish each of Sturtevant and Meltzer's arguments that the pendant is a fraud. Whether this defense of the pendant's authenticity can be described as "vigorous" is perhaps a matter of judgment, but readers are invited to examine the literature. Readers might also wish to consult a further reference [*Ann. N.Y. Acad. Sci.* **228**, 35 (1977)], in which Kraft states that the pendant should be considered as "definite evidence of association of early American man with the woolly mammoth."

3) I am puzzled as to why the statement "Meltzer told *Science* that during the past decade only one request was made to the Smithsonian Institution for permission to date the pendant" is described by Custer *et al.* as a "factual error." Documentation at the Department of Anthropology at the Smithsonian Institution shows that the only formal request made during this period was in 1981, by Custer and his colleagues. And, contrary to Custer *et al.*, the same documentation shows that the reason for the refusal was clearly stated. There was no mention of potential problems of contamination.

4) This does not refer to my Research News item, but Custer *et al.* must be aware of the different constraints of conventional as against accelerator mass spectrometry carbon dating, as well as the recent calibration

of marine shell samples.

5) That Hilborne T. Cresson committed suicide while his mental state was disturbed is not a matter of dispute. The problems with some of Cresson's archeological work are also well established. These issues are legitimate background to a story about the Holly Oak pendant, as he was its "discoverer." If Custer *et al.* wish to infer from this that Cresson was "capable of lying and perpetuating frauds," then this is a matter of judgment. It was not presented as such.

In their concluding paragraph, Custer *et al.* say that "the Smithsonian Institution has finally *allowed* the kind of studies that we originally requested more than a decade ago" (emphasis added). This is an interesting view of the progress of science, because, to an outsider in this affair, it seems that dating was done just as soon as the techniques became available that would offer a secure answer.—ROGER LEWIN

Demand for Electricity

Mark Crawford (News & Comment, 18 Nov., p. 1005) is correct in noting the likely power crunch parts of the country will experience in the next decade, but misses the most important point. We need to start building capacity to meet demand as well as continue to improve efficiency. Crawford points out that electricity demand has been growing since 1983. In fact, it has continued to grow for at least the past 20 years, with the exception of 1982. The demand for power has directly matched growth in the economy for over a decade, while the demand for oil and gas has largely declined.

The Energy Information Administration estimate of the annual growth rate in power demand of 2.4%, Crawford states, is viewed with "caution, because the utility industry has overestimated its capacity needs in the past." It appears, however, that the opposite is now the case. In 1987 electrical demand grew 4.5%. Capital investments in new capacity is now a high-risk game for utilities, and thus there is great incentive for downplaying demand projections.

The energy analysts Crawford quotes as demonstrating the opportunities for great electrical savings have one thing in common—they do not have the responsibility to serve that is incumbent on the utilities. If the analysts are wrong, they suffer no consequences. If a utility underestimates electrical demand, millions of individuals are affected, either through reduced economic growth due to insufficient supply or through reduced reliability of the network.

It would be disastrously imprudent to not plan for new capacity additions in the hope that we can impress conservation on a diverse, free society. The conservation efforts being proposed require individual actions and investments by millions of people. How can that be *assured* without overt regulation or coercion? And if it is not assured, then how can utilities safely assume they do not have to build capacity on the basis of their current view of demand growth?

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Response: It would appear that Bessman makes electricity the old-fashioned way—by building new billion-dollar power stations. He does not acknowledge that significant amounts of reliable power can be obtained by making commercial buildings more efficient. The nation's electric utilities can capture these power savings if regulatory commissions will move to reward them for doing so. Yes, as I said in my article, new power plants must be built in parts of the United States. Is it wise, however, to burden the country's economy with these capital projects without aggressively pursuing less costly efficiency programs in the commercial sector?—MARK CRAWFORD

Orangutan Tool Use

Since my copy of *Science* sometimes comes late to my field site in Central Indonesian Borneo (Kalimantan Tengah), I am only now responding to the Research News item of 15 May 1987 by Roger Lewin concerning ape tool use. Discussion of pongid tool use is always timely.

Contrary to what is stated in the article, wild orangutans *do* spontaneously use tools in the wild. While captive orangutans are the most adept pongid tool users in capitivity, wild orangutans are said by Lewin to "have never been observed to use tools in the wild, uninfluenced by humans." If human "influence" means that a human observer is below the wild orangutan's tree unobtrusively watching from 30 to 50 feet away with binoculars, then we will probably never see wild orangutan tool use "uninfluenced by humans" unless the observers are robots.

However, in my study of wild orangutans at Tanjung Puting National Park, now in its 17th year, while tool use is by no means common, it does occasionally occur (*1*). For instance, a wild orangutan adult male was observed breaking off a dead ironwood branch and using the stick to scratch himself (*2*). In another instance, a juvenile was seen tearing off a branch and whipping it frantically around him to drive off wasps.

Nonetheless, observations by Suzanne Chevalier-Skolnikoff and me indicate that the high cognitive abilities of orangutans are most frequently used in locomotion (*3*). The levels of cognition involved can be equated with the levels that are assumed to be required for what anthropologists typically call tool use (*4*), but since the pole trees, branches, and vegetation orangutans manipulate in a very sophisticated manner are still attached to the substrate, these manipulations are not generally called tool use.

If one understands wild pongids and their environments as well as their particular adaptations, ape tool use is not confusing. In the wild, orangutans are constantly manipulating their three-dimensional environment as they move and as they forage. It is not surprising that they perform well in captivity with sticks and other materials no longer attached to the substrate. Orangutans demonstrate the same high cognitive abilities observed in nature as they do in captivity, but the usual barren cage is a totally different environment from that of the dense, supple, tridimensional world of the tropical rain forest canopy.

It would be a mistake to assume that higher cognitive abilities in the pongids evolved as an adaptation for tool use or as a result of tool use. Rather, tool use is an expression of a more general adaptation for solving problems. Obviously, the problems faced in captivity by orangutans are different from those faced in the wild.

A more interesting question not addressed by the Research News article is, why do orangutans, unlike chimpanzees, *not* exhibit complexes of tool-*making* behavior in terms of extracting resources from the wild?

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REFERENCES

1. B. M. F. Galdikas, *J. Hum. Evol.* **10**, 19 (1982).
2. ———, *Primates* **23**, 138 (1982).
3. S. Chevalier-Skolnikoff, B. M. F. Galdikas, A. Z. Skolnikoff, *J. Hum. Evol.* **11**, 639 (1982).
4. B. Beck, in *Socioecology and Psychology of Primates*, R. Tuttle, Ed. (Mouton, The Hague, 1975), pp. 413–447).

Erratum: In the picture accompanying the News & Comment article "NIH holds a science fair" by Gregory Byrne (4 Nov., p. 661), Dale Kiesewetter was incorrectly identified as Ronald D. Finn.

Erratum: In the News & Comment article "U.S.–Soviet weapons journal launched" by Eliot Marshall (2 Dec., p. 1243), Herbert L. Abrams, a member of the editorial board of *Science and Global Security*, was incorrectly identified as Herbert L. Adams.