Letters

The Meadowcroft Rockshelter

Roger Lewin's Research News article "The first Americans are getting younger" (27 Nov., p. 1230) brings to the forefront a number of stimulating questions in contemporary archeology's search for evidence of the earliest Americans. The article, however, might have been titled "The first Americans are getting older," not "younger," for the development of refined techniques of excavation and analysis in the last 20 to 30 years has consistently pushed back our evidence of the first Americans. Refinements in radiocarbon dating, in particular, and especially our current capacity to assess very small organic samples with accelerator mass spectroscopy (AMS), mean that data that would have gone unnoticed and unassessed 30 to 50 years ago can now be analyzed.

At the beginning of this century, few archeologists believed that humans had entered the New World much before 7000 years ago. We now have abundant evidence that this event had occurred by 11,500 years ago at the latest. At most sites, however, the artifacts suggest a lengthy period of antecedent development. None contain artifacts that one might expect of "new arrivals." We also know that by this time human populations had spread throughout the New World, extending even to the tip of South America; thus, indications are that the initial entry must have taken place well before 11,500 years ago. The question remains, "How long before?"

Meadowcroft Rockshelter's contribution to this search is, as Lewin suggests, a pivotal one. The site was excavated over the course of 7 years at a cost of nearly $1 million, with little turnover in the major participants. Such fortunate conditions are rare in archeology. Under prevailing funding strategies, most archeological sites, even those that promise to yield data in the search for the first Americans, must be dug with a comparative budgetary pittance. We wonder how many other "Meadowcrofts" might now be recognized if the requisite money, time, and personnel could have been made more generally available.

Lewin's article repeats some previously articulated reservations about the validity of the Meadowcroft radiometric dating. In that context, the following data (*1*) may be of interest.

One hundred and two charcoal samples from Meadowcroft were submitted for radiometric dating to three different laboratories. The charcoal came from fire pits, fire floors, or charcoal features, with the exception of two carbonized basketry fragments. To date, 50 of these samples have produced dates. All but four dates are internally consistent and in absolute stratigraphic order. The four reversals are low-magnitude "flip-flops" in the Middle Archaic, Late Archaic, and Late Woodland periods, respectively. Questions have been posed about 13 dates (six of which have clear artifact associations) older than about 12,800 ± 870 years.

Most criticisms about the pre-12,800-year-old dates concern the possibilities for either particulate or nonparticulate contamination of these carbon samples. With regard to particulate contamination, we repeat that there is no coal seam in or near the rockshelter. The nearest actual coal outcrop is more than 1/2 mile north of the site. There are small, isolated, discontinuous fragments of vitrinized Pennsylvania-age wood west of the back (north) wall of the rockshelter, but these fragments are circumscribed occurrences and are not represented within 7 linear meters of the hearths that produced the earliest Meadowcroft dates. Furthermore, every radiometric sample from all Pleistocene-age levels was examined for coal particles with both optical and scanning electron microscopy. No coal particles were identified by three different laboratories despite the fact that, in order to contaminate a sample on the magnitude that has been suggested, nearly 35% of the sample would have to be coal. Selected lower and middle Stratum IIa samples underwent reflectance analysis as well as paleobotanical examination for Densosporites, a common spore in Pennsylvanian-age coal. In all cases, the results were negative.

For criticisms about particulate contamination to be credible, the mechanical introduction of vitrinized Pennsylvanian-age wood into the earlier Meadowcroft fire pits would have required some unknown (and unspecified) mechanism so precise and of such duration that it nevertheless resulted in a consistent stratigraphic order among the early dates. The lack of any feasible mechanism for the selective injection of contaminants solely into the pre-12,800-years-ago samples renders the argument for particulate contamination of the samples unconvincing.

With regard to nonparticulate contamination, the following points are important. (i) Vitrinized wood cannot be dissolved in ground water, nor can it be dissolved in boiling sodium hydroxide or in any other reagent normally kept in a radiocarbon laboratory. Vitrinite can be mechanically ablated (by washing boiling water over it for 200 hours) and physically transported in particulate form. (ii) If vitrinite is not the source of any dissolved contaminant, there are no other candidates, as the underlying shale (Stratum I) that is the basal unit at the rockshelter is not carbonaceous. (iii) If one assumes the presence of an as-yet-undiscovered potential source of contamination, there is no viable mechanism for its transport, as the present water table lies nearly 5 meters below the deepest occupation surface in the site and apparently was not higher prehistorically. The various positions of the drip line, preserved intact in the deposits and marked by different calcium carbonate percentages, would have been erased if ground-water fluctuations had affected the lower and middle Stratum IIa samples. Electron microscopy of the individual sand grains from these levels indicates no postdepositional modification due to water action. (v) In 2 of the 13 samples from the lower and middle Stratum IIa levels humic extractions were arrested for fear that too little sample would be left to assay. Both of these exceptions are from the top of middle Stratum IIa, and the resulting dates are younger than 12,800 years ago. In 2 of the 11 underlying samples, the dissolved fraction was older than the solid fraction. In the other tested samples, the soluble fraction was younger than the insoluble fraction. This was independently confirmed by two radiocarbon laboratories.

The last remaining, diminutive sample from the precultural levels at the site, already scrutinized for particulates and Densosporites spores, was processed at the Oxford AMS laboratory, which found no contaminants. The solid fraction was extracted without arresting the reaction and was subsequently dated (*2*) at 31,400 ± 1,200 years (OxA-363). The soluble fraction was dated (*2*, p. 241) at 30,900 ± 1,100 years (OxA-364). These dates conform almost exactly to a previously calculated Smithsonian date of 30,710 ± 1,140 years (SI-1687) for this level, which is not associated with any artifacts and apparently dates to well before the initial occupation of the site by humans. It therefore appears that there is no evidence for either particulate or nonparticulate contamination of the Meadowcroft deposits.

If the deepest dates within lower Stratum IIa are correct, and if the dates after 12,800 years ago are accurate, then only the important group of dates between these ages could be in error. We believe this possibility is intrinsically unlikely.

Applying a conservative interpretation of the data, we conclude that, even if only the youngest date from upper middle Stratum IIa is valid, the minimum age for the presence of human populations in this portion of Pennsylvania is on the order of 10,600 to

12,000 years ago. If the six deepest dates unequivocally associated with cultural material are averaged, then humans were definitely present at this site (and, by implication, throughout much and perhaps all of the Americas) sometime between approximately 13,955 and 14,555 years ago.

In the final analysis, however, it matters little what the earliest occupation date from Meadowcroft is. This site has produced a vast array of geological, archeological, paleofloral, and paleofaunal data that collectively help us to understand more about the full temporal range of aboriginal human life in this part of the Ohio River system. Although the incipient occupation of the site has captured the spotlight, the lion's share of the site's deposits is an eloquent testimonial to some 10,000 subsequent years of human cultural adaptation. If excavation of the site accomplishes nothing more than to draw increased attention to this sometimes subtle, sometimes radically shifting relationship among humans, their technology, and the conditions of their natural environment, it will be enough.

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Brainwave Counting

I read with interest William Booth's article on keystroke counting by "Big Brother" (News & Comment, 2 Oct., p. 17), which dealt with the plight of clerical and secretarial workers. Lest scientists take a "so what?" attitude about such piecework performance management, I want to report on just that kind of situation in the U.S. government.

The union of which I am president (Local 2050, National Federation of Federal Employees) represents professional workers at the U.S. Environmental Protection Agency (EPA) headquarters, and we have been facing one form or another of piecework performance management for some time. In general, these systems have arisen when managers, who are often not intimately familiar with the scientific details of a project and who are often under statutory or court-ordered deadlines themselves, unilaterally assign work and deadlines to professionals, not taking into account important factors influencing professional work.

A particularly egregious example is found in EPA's Pesticides Program, where our toxicologists, chemists, and other professionals are the public's and the environment's first line of defense against potentially harmful pesticides. Largely on the basis of scientific evaluations by these professionals, pesticides are either registered for use in the United States or are denied such registration. Because of the significance of the work these scientists do, piecework performance management does more than subject professionals to antiprofessional working conditions—it puts the public health and the environment at risk.

Under this system a scientist is credited with a certain number of points, or "TECH-hours," for reviewing an LD_{50} study, another number of points for reviewing a teratology study, and another number for reviewing a 2-year cancer bioassay. All LD_{50} studies are worth the same number of points, as are all 2-year bioassays, and so forth, regardless of the complexity or length of individual studies. We have evidence that a professional's performance is rated essentially on the number of "TECH-hours" accumulated.

It is obvious that this piecework performance management rewards hasty reviews of vital toxicological studies, while it penalizes reviewers who may take longer than the "standard" amount of time to carefully and conscientiously question data upon which far-reaching public health decisions must be based. It is also obvious that the end result of this system will be that—sooner or later—a pesticide that ought not be registered will be registered.

Our resistance to this system is well documented (1), but the message I want to convey is that keystroke—or brainwave—counting is not just an issue for secretaries and it is not just a question of antiprofessional working conditions. It is a clear and present danger to the professional integrity of scientists employed by a U.S. government agency, one that will have tragic effects on the environment and public health—the only question is, when?

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Flora Project

With respect to the case for a "Flora of North America" (News & Comment, 28 Aug., p. 967), all of the tools of plant taxonomy (including the newest) should be brought to bear on the problem. New sources of information, such as that from chloroplast DNA as noted by Theodore Barkley (Letters, 20 Nov., p. 1027), will be vital for providing a clearer understanding. The range of technologies available for such a project (and consequently the costs) depend to a large extent on how much those in the project use existing data. On one extreme is the opinion (which I do not hold) that everything is already there and only needs assembly. On the other is the costly and probably unmarketable alternative of gathering all data anew. This latter would likely place the costs well beyond the reach of any funding effort. The answer, of course, is a compromise of these two extremes. This, I believe, is the approach of the Missouri Botanical Garden.

As a scientist, I find such an effort to be of critical importance. The fact that this country does not have a comprehensive compilation of its flora is truly sad. Such an information base would be of great value not only to scientists but also to planners, developers, and politicians. It is a task that would seem essential for a nation that considers itself a leader in scientific research. Unfortunately, the realities of limited funding opportunities require us to balance this with other, equally important needs. It is my hope that agencies such as the National Science Foundation will see this project as important and provide at least some level of support.

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Erratum: Robert E. Ricklefs' name was misspelled throughout Roger Lewin's Research News article "Egg-laying in birds remains a hot issue (29 Jan., p. 465).