Letters

Terrorism and Animal Rights

Recently, members of animal rights organizations were upset by Health and Human Services Secretary Louis Sullivan's use of the term "terrorism" to describe their activities. The attempted murder of two researchers in England by car bombs that seriously injured a 13-month-old child (News & Comment, 22 June, p. 1485) indicates that Sullivan's characterization is true for the more radical elements of the movement. In the past, when animal rights terrorists used violence in their efforts to stop medical research, many supposed "moderate" members of the movement paid lip service to their dislike of such tactics. These "moderates" suggested that although they themselves would not commit such acts they could understand the motivation of those who did. Now the maiming of a child has starkly brought into focus the misguided ethics of those in the movement who actually value nonhuman animal life over human life. Every member of every animal rights group must decide where they stand on the issue of terrorism. They must either repudiate the terrorists, or by their silence, recognize that they have joined with them in supporting the attempted murder of those who are trying to provide better health care. Silence on this issue will condemn even the truly moderate groups that continue to strive for improved animal welfare, a goal that we all must share.

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Linguistics and the Earliest Americans

In her article "Confusion in earliest America" (Research News, 27 Apr., p. 439), Virginia Morell provides an interesting view of the Conference on Language and Prehistory in the Americas. However, I feel that her description of a major change in consensus among archeologists concerning the initial peopling of North America is overstated.

Morell implies that Dennis Stanford of the Smithsonian Institution is a recent convert to the viewpoint that there is convincing evidence for a pre-Clovis occupation of the Americas and that his conversion represents a common experience among most archeologists working in the Americas. For at least the past 15 years Stanford has been a major proponent of the view that a pre-Clovis occupation was present. His overviews (*1*) and site-specific research (*2*) have focused on early sites, and he has rigorously investigated, and enthusiastically endorsed, sites that are said to have had pre-Clovis occupations. He has also admitted that there are problems in interpreting these sites, including his own (*3*).

To say that most archeologists are convinced of the validity of the Meadowcroft Rockshelter and Monte Verde site data is also not completely accurate. For example, a recent major review (*4*) of early sites in South America noted that the data from Monte Verde are still viewed as problematic. In addition, the bifacial tool from Monte Verde illustrated in Morell's article (p. 439) was found from an eroding stream bank before the start of the controlled excavations at Monte Verde and is not clearly associated with the early dates from the site (*5*). Nor is the Meadowcroft Rockshelter beyond question (*6*), even though James Adovasio and his associates have been able to effectively answer many of the initial criticisms of the site's radiocarbon chronology (*7*).

While I do not claim to know what most archeologists working in the Americas think about the question of pre-Clovis occupations of the New World, I can say that the viewpoints presented at the conference in question are not the heralds of a new consensus sweeping American archeology, but rather the same voices telling the same stories that they have told before.

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As a participant at the recent conference on Joseph Greenberg's classification of American Indian languages, I found that Morell's description of the conference bore little resemblance to what actually took place. While only 2 of 19 invited papers concerned archeology, Morell's article dealt far more with archeology than with language. Furthermore, the main point of the article—that archeological dates before 12,000 years ago cast doubt on Greenberg's classification—is simply incorrect. Whether speakers of Proto-Amerind first entered the Americas 12,000 or 20,000 years ago is immaterial to the Greenberg classification, as he himself has repeatedly stated.

Morell's article makes it appear that Greenberg drew little support from other linguists, yet my tally of the speakers indicates that roughly half of them supported Greenberg. There is no mention of any of these supporters or of their arguments.

While Morell mentions the support that Christy Turner's dental evidence provides for Greenberg's classification, there was no mention of the recent discovery by L. L. Cavalli-Sforza and colleagues that, on the basis of human genetics, the populations of the New World fall into the same three groups that Greenberg had previously defined on strictly linguistic grounds. Yet I discussed this point at length in my presentation, and it has been widely reported in the press. Morell concludes that Greenberg's classification "has begun to show signs of age." Most readers of *Science* will, however, recognize that, when a classification arrived at by means of language is fully and independently corroborated by a study of human genetics, this is usually interpreted as a sign of maturity, not of age.

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Virginia Morell's Research News article of 27 April is misleading. Many Americanist linguists disagree with Joseph Greenberg, and his methods are considered to be out of date. Glottochronology, the basis of the conjectural chronology discussed in the article, has also been discarded. The idea that phonology and semiotics remain stable indefinitely, affected only by vague decay, has not withstood scrutiny. In general, linguistic connections older than 5000 years have been validly traceable only when reinforced by writing.

More than this, the insistence by some American anthropologists on a postglacial entry of man in the New World has long been an anachronism, particularly since archeology in Siberia has established a widespread horizon of Mousterian-type cultures that have been dated at 35,000 to 50,000 years before present. Substantial evidence also exists of human occupancy in the Acheulian, 300,000 or more years ago. The most recent relevant data have been developed by A. P. Derevyanko and his col-

leagues in the Siberian Division of the Academy of Sciences of the U.S.S.R. (*1*).

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Orphan Drug Act

Ann Gibbon's recent article "Billion-dollar orphans: Prescription for trouble" (News & Comment, 11 May, p. 678) addresses several points about the Orphan Drug Act and proposed changes to it. However, the article missed, where erythropoietin (EPO) is concerned, the most crucial point: EPO, even under the current law, should not be an "orphan" product, nor should the act block the access of our EPO product, Marogen Sterile Powder, to the marketplace.

The matter is simple. First, orphan drug law does not supersede U.S. patent law. It is incorrect and illogical for the "orphan" status of Amgen's EPO product to deny market entry to Marogen, whose developers hold the dominant U.S. EPO patent. Second, the best available data suggest that the number of anemic kidney patients who could benefit from EPO therapy is well beyond the 200,000-patient limit for true "orphan" products. Third, the Orphan Drug Act requires that "orphan" designations and product registrations be for the same indications; Amgen's are mismatched. The Amgen product was designated an "orphan" for treating anemia associated with end-stage renal disease, which is not a separate disease state to begin with, while its product was registered to treat both anemia predialysis and dialysis patients.

All of these issues have been raised before the U.S. Food and Drug Administration in the form of a Citizen Petition filed in November 1989 and amended this past January.

Gibbons' implication that Genetics Institute, our development partner, has somehow tinkered with the Amgen molecule in order to enter the EPO market is both incorrect and unfair to scientists at Genetics Institute who carried out brilliant EPO research. The fact is that Genetics Institute alone discovered the drug form of pure, homogeneous EPO and then, in research parallel to and independent from Amgen's, successfully cloned the EPO gene. Had Amgen never conducted EPO research, Genetics Institute still would have isolated and manufactured recombinant human EPO.

The concept of the Orphan Drug Act is sound and, in most cases, the law has worked well. We think the law can be strengthened and together with Genetics Institute and The Upjohn Company, support components of Representative Henry Waxman's initiative toward that end. Gibbons suggests that Waxman's proposal could hamper development of "orphan" drugs; more likely, such changes might "dissuade" firms from trying to squeeze out monopolistic rights for products that are hardly "orphans," rights to which they should not be entitled. Our support for the Waxman amendments, however, does not alter the fact that no changes to the act are needed to make Marogen available to physicians and patients. All that is required is the correct application of the Orphan Drug Act as it now stands.

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The value of the Orphan Drug Act, particularly its 7-year marketing exclusivity provision, was touched on in quotes from a