Letters

Chronology of the Last Glaciation

James B. Griffin in his extremely interesting article, "Some prehistoric connections between Siberia and America" [*Science* **131**, 801 (1960)], states that "the Wisconsin ice advance is thought by some Pleistocene students to have begun about 50,000 B.C., followed by a warmer period corresponding to the Würm interstadial in Europe. This may have provided an ice-free corridor east of the Rockies some 30,000 years ago."

I have no doubt that Griffin is right about the thinking of some, if not many, Pleistocene students. Apparently a short but important paper by H. Tauber and H. de Vries [*Eiszeitalter und Gegenwart* **9**, 69 (1958)] has received less attention than it deserved. According to these authors, samples for radiocarbon dating from the Würm interstadial deposit at Brörup, Jutland, showed no significant activity after thorough decontamination. "This means," they write, "that the interstadial at Brörup and the preceding cold period are older than 50,000 B.C." And, one may add, perhaps much older.

It has been [D. B. Ericson and G. Wollin, *Micropaleontol.* **2**, 257 (1956)] and still is my guess that the Würm I-II or Brörup interstadial is represented in the deep-sea sediments of the North Atlantic by a well-defined faunal zone containing low-latitude species of planktonic foraminifera among which *Globorotalia menardii flexuosa* is especially abundant. From the stratigraphical position of this zone, at the base of a relatively thick layer of sediment with cold-water species throughout, which lies, in turn, directly beneath postglacial sediment, it is difficult to see how it could be anything else than the Würm interstadial. Now, extrapolation of rates of sediment accumulation determined by radiocarbon dating of many samples from long sediment cores from various parts of the Atlantic, Caribbean, and Gulf of Mexico has shown that the time interval represented by the zone containing *G. menardii flexuosa*, or the Flexuosa zone, came to an end about 65,000 years ago.

Of course, my correlation of the Flexuosa zone with the Würm interstadial may be wrong in spite of its apparent plausibility. Even so, the important fact remains that Tauber and de Vries have shown conclusively that the climatic amelioration which separated the early and late Wisconsin glaciations occurred more than 50,000 years ago, and that therefore the short chronology of the last glaciation must be abandoned. Accordingly we conclude that if early man entered America during the Würm interstadial, he must have done so at least 50,000 years ago, and perhaps no less than 65,000 years ago. This does not impair Griffin's argument regarding the time of man's appearance in America. If anything, this longer chronology strengthens his conclusion that man did not enter America during the interstadial between the early and late Wisconsin glaciations.

DAVID B. ERICSON
Lamont Geological Observatory, Palisades, New York

Emotionality and Fear

Harlow and Zimmermann's description of "Affectional responses in the infant monkeys" [*Science* **130**, 421 (1959)] was a gem, but it did, I believe, contain a minor flaw.

Although it clearly described "affectional responses," which are indeed emotional responses, when the term *emotionality* was used, it seemed that its meaning was limited to the disruptive emotion of fear. This can be seen in the use of the term *emotionality index* rather than *fear index*, and in the following statement (p. 425): "Children in the first group (mother present) were much less *emotional* [italics mine] and participated much more fully in the play activity than those in the second group (mother absent)." This first group was not less "emotional" but less fearful; presumably, if the first group was happier, it could also be called *more* emotional than the second.

Harlow and Zimmermann seem implicitly and inaccurately to equate emotionality with fear, an equation which would lead us to see courage, for example, as equivalent to emotionlessness. A very fine critique of the theory "according to which emotions are disorganized or disruptive states" is to be found in V. J. McGill's *Emotions and Reason* (Thomas, Springfield, Ill., 1954).

Hence I think their fine article would be even finer if this inaccuracy in the use of the concept "emotionality" were clarified.

NATHANIEL S. LEHRMAN
15 Canterbury Road, Great Neck, New York

I am in agreement with Lehrman's position and have long been an opponent of those psychologists who would identify and define emotion as "disorganized and disruptive states." In an earlier paper [*Am. Psychologist* **12**, 673 (1958)], I unequivocally define love as an emotion, and I still subscribe to this theoretical position.

The term *fear index* would have been

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