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.

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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.

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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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improper, since many of the objective check-list items measured behavior more adequately designated as distress, disturbance, yearning, and displaced aggression. The term *disturbance index* could be substituted for *emotionality index* without semantic loss and, probably, without semantic gain.

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Small Colleges and Small Minds

The lack of interest of some teachers in research, discussed in the editorial of 8 January [*Science* **131**, 71 (1960)], is disturbing, but so also are some aspects of the editorial itself.

The heading "Small colleges and small minds" implies that these go together. This guilt-by-association technique is used several times in the editorial, though the man whose views are cited as the basis for the editorial is the president of a "small college." Is there evidence for a larger proportion of so-called small minds in small colleges?

The following statement is perhaps the worst part: "The core of the argument for scientific research . . . is that while there may be good research scientists who are not good teachers, the evidence is that there are no good teachers whose competence is not increased by good scholarship." Ostensibly this places the research scientist above reproach and leaves the incubus on the teacher. Actually, the statement is a *non sequitur,* and its converse is equally true—and unfair, in turn, to the research scientist. It is suggested that the reader substitute the word *editors* for *teachers* (he will find it equally correct). Competence in any profession would be increased by good scholarship, as the editorial in question seems to illustrate.

After this unfair statement the editor changes from "good scholarship" to "research" in the next sentence, which again helps to put the research scientist beyond criticism and implies criticism of the teacher. Had he chosen to make a straightforward statement of what he implies it might have read something like this: "While there may be good research scientists who are not good teachers, the evidence is that there are no good teachers who are not good research scientists." This is, I suggest, rather untenable.

In the next paragraph we are told: "A prominent figure on many campuses is the instructor who is forever marking exams, grading papers, and drawing curves representing his students' performance." With our present grading system instructors are inevitably marking exams, and so on, but the editor is depreciating the teacher with the guilt-by-association technique again, for he indicates that these instructors have "schemes" of a detrimental nature. But the scientist could be given the same unjust treatment, in very similar phrases: "A prominent figure in many research laboratories is the scientist who is forever looking at figures, evaluating data, and drawing curves representing his results. He is full of schemes . . . that if instituted would require the assistance of all his colleagues." Though, curiously, in this case involving one's colleagues becomes a virtue and is extolled under the name of "scientific teamwork."

But this is not all. Having implied that no research means no scholarship, the editor completes his degradation of the teacher in the next sentence by suggesting that those concerned with students' performance are even against reading books!

In the last paragraph the editor proposes the right question, but for the wrong schools, when he asks: "But why in small colleges should some instructors oppose the recognition of good research as a consideration second to good teaching?" In so far as this opposition exists, it is typical not of the small school but of the large school with an extensive graduate program, where some instructors want recognition of good research first and of good teaching second, if at all. In large measure the apparent hostility toward research in the small college is manifested by instructors who do not oppose research as such, but oppose the evaluation of good teaching as a consideration second to good research, because they have seen the unfortunate results of this practice, especially since all too often a department finds itself with "research scientists who are not good teachers."

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It was with considerable interest that we read your editorial "Small colleges and small minds," for here at Wilkes College the subject of research in small colleges has been of more than academic interest. Wilkes, I believe, would fulfill your criteria for a small independent liberal arts college. Our experience in the initiation and conduct of a research-teaching program has been satisfactory and rewarding. A summary of our findings may be of interest