France, Kent and East Anglia, the flaking on a great many of the specimens is seen to be intentional. I have had the opportunity of examining a very large number of specimens from the Upper Miocene (or Lower Pliocene) of Central France, and the systematic edge flaking on most of these seems to me definitely intentional. I have not been able to examine a large collection of Harrisonian eoliths from Kent, but such as I have seen appear to have the same systematic series of flakes. The sub-Crag specimens present rather a different problem, including as they do primitive palæolithic types, but those among them which were partly made by steep flaking again show series of edge flakes which are too systematic to have been done by natural means.

As regards modern experiment, I have had the opportunity of examining specimens of steep flaking done by Mr. Reid Moir, and of watching him remove these flakes with a hammer stone. I have also succeeded in doing the same type of flaking myself, and am convinced that it is not very difficult. It is also relatively easy to take off an adjacent series of such flakes so as to produce an edge of desired shape. My own experiments in making and using various types of stone implements are as yet incomplete, but according to my experience eolithic edge-work is fairly easy to do both on flint and on Bunter pebbles (the latter approximating to Darmsden and Soan types); also various pre-palæolithic types, including rostro-carinates, can be made with practice, involving some use of steep flaking. Experimental copying of Aurignacian and Magdalenian types also shows that flaking at about 90° is necessary for removal of the flakes from the cores. It is my experience that copies of eolithic and pre-palæolithic forms can be used for cutting wood, though they are not so efficient as copies of Acheulean or Clactonian types; I have also found that those made of quartzite do not take as keen an edge as those made of flint, and are therefore less efficient.

It seems to me that recognition of intentional work on flaked stones claimed as primitive implements involves two definite questions: (1) *Could* they have been made intentionally? (2) If so, *could* they have been used for any purpose? As modern man *can* make copies of eolithic and pre-palæolithic types, the answer to question (1) is that these types *could* certainly have been made by primitive man. As such implements can be used, for example, for cutting wood, there would have been some purpose in making the originals. If this is so, the question of a natural origin scarcely arises, and those who may believe in the possibility of such a process will have to explain why certain restricted eolithic forms were made naturally at an Upper Miocene or Lower Pliocene date, to be followed by more diverse and advanced forms in Pliocene and Lower Pleistocene times.

D. F. W. BADEN-POWELL.
University Museum, Oxford.
Oct. 28.

[1] Moir, J. Reid, NATURE, **152**, 78 (1943).
[2] Barnes, A. S., NATURE, **152**, 477 (1943).
[3] Moir, J. Reid, "Pre-Palaeolithic Man", p. 15.
[4] Barnes, A. S., *Amer. Anthrop.*, **41**, 110 (1939).

IN his letter in NATURE of October 23, Mr. A. S. Barnes says: "The high proportion of high-angle scars in the Tertiary flints . . . suggests that the Tertiary flaking was due to soil movement under pressure arising from solifluxion, foundering, or ice-action". But I think most geologists would agree that these three factors (or at any rate the first and third) were far more active in Pleistocene than in Tertiary times. Why have they failed to produce any comparable effects in the later period? It is an old question, but not less cogent on that account.

Mr. Barnes ignores the evidence of his own figures[1], that there is a connexion between the number of these high-angle scars and the age of the implements concerned; for we pass in percentage from 0 in Solutrean, through 1 in Mousterian, and 5 in Early Acheulean, to 18 in "Chelléen et Préchelléen" (Warren Hill). It is, of course, impossible to construct an accurate graph from these figures, since we do not know the time-intervals involved; but the curve is obviously rising rapidly as we pass from the later and more skilled to the older and rougher work, and it seems by no means impossible that it might rise to 60 or even 70 per cent by the time we reach the Kentish plateau.

It seems unfortunate in this connexion that Mr. Barnes has omitted all mention of the Cromerian flints, and of the "Prechellean" implements of the Zambezi River[2]; for the former might supply some intermediate stages, while the latter are free from all ice-action, and (so far as one can judge by the contours) from any powerful effects of solifluxion and foundering. Admittedly our knowledge of these Zambezi implements is still very imperfect; but until we have fuller information about them, any conclusion along the lines of Mr. Barnes's arguments would seem to be premature.

HENRY BURY.
The Gate House,
Alumdale Road,
Bournemouth.

[1] Barnes, A. S., *l'Anthropologie*, **48**, 221 (1938)
[2] Armstrong, A. L., *J. Roy. Anthrop. Inst.*, **66** (1936).

Radio Fade-Out on February 10, 1943

O. E. H. RYDBECK, Chalmers Institute of Technology, Gothenburg, in NATURE of June 19, 1943, has requested information about magnetic disturbances on February 10, 1943. At the Magnetic Observatory, Hermanus, where the magnetogram sensitivities were: H, $2\cdot32\,\gamma$/mm.; D, $0\cdot534\,'$/mm.$=$ $2\cdot20\,\gamma$/mm.; Z, $3\cdot69\,\gamma$/mm.; disturbances were recorded and published in *Terrestrial Magnetism and Atmospheric Electricity* (June 1943).

Micropulsations, which were little more than a broadening of the D trace, appeared at 07h. 47m. G.M.T., February 10. About three hours later disturbances were recorded on all traces, the D disturbance being greater than the H disturbance, which at this Observatory is characteristic of magnetic disturbances at the time of a radio fade-out. The beginning of the disturbance was not sharply defined. On the H trace the beginning was at about 09h. 45m. G.M.T., and on the D and Z traces at about 09h. 43m. G.M.T., February 10. Seventeen hours later there were abrupt changes indicating the beginning of a magnetic storm. A time interval of 17 hours would indicate a speed of about 1,500 miles per second for the transmission of impulses from the sun.

A. OGG.
Magnetic Observatory,
Hermanus, C.P.,
South Africa.
Sept. 13.