References

1. P. E. Cloud and C. A. Nelson, *Science*, **154**, 766 (1966).
2. M. F. Glaessner, in *Earth Science Reviews* (Elsevier, Amsterdam, 1966), vol. 1, pp. 29–50; ——— and M. Wade, *Palaeontology* **9**, 599–628 (1966).
3. P. C. Sylvester-Bradley and T. D. Ford, *Geology of the East Midlands* (Univ. of Leicester Press, Leicester, England, in press).
4. M. Y. Meneisy and J. A. Miller, *Geol. Mag.* **100**, 507 (1963).

22 March 1967

Ford challenges our interpretation of evidence relating to a crucial question in geology: how, and on what grounds, shall the last one-eighth of earth history, characterized by abundant multicelled animal life (the Phanerozoic Eon), be separated from the first seven-eighths, in which fossils representing such life are absent or doubtfully represented (the Cryptozoic or Precambrian Eon). We approve of this challenge and the manner in which it is stated. We readily admit that the proposition we support is, on current evidence, debatable, as is the one we oppose. What is important is that discussion and the search for critical evidence continue until a rational and broadly acceptable solution of the problem is reached. We will not here elaborate on the specifics of this problem. They are briefly stated in the paper which Ford criticizes (*1*) and are considered in detail in a paper in press by Cloud (*2*). The discussion below is confined to points raised by Ford.

To begin with, let us reiterate what we stated in the report criticized, namely, that we also have reservations about the identification of the California "*Pteridinium*," indicated by the use in that paper of the conventional prefix *cf.*, for "compare with" (although this was confused by editorial revision beyond our control). More specimens are needed to confirm or discredit our provisional interpretation. Nevertheless, at this time there is a reasonable likelihood that this identification is correct, and, while expressing his own reservations, Ford does not quarrel with us on that score.

Rather he observes (i) that we overlooked Glaessner's correlation of the Ediacara and Nama beds with strata of the Charnwood Forest in England, (ii) that it seems not to have occurred to us that *Pteridinium* might have a long chronological range, and (iii) that the Charnwood Forest beds are intruded by, and therefore older than, rocks whose radiometric ages strongly imply a Precambrian age.

Mindreading, of course, is hazardous, and the fact is that neither (i) nor (ii) is true. We are well aware of suggestions by Glaessner and others about the possible correlation of the Ediacara beds not only with strata in the Charnwood Forest, but also in the U.S.S.R., as stated at the end of the first paragraph of our paper (*1*). As for an extended range for *Pteridinium*, that, of course, is a possibility no thoughtful paleontologist would fail to consider for any fossil. It may be, as Ford intimates, that what has been correlated with the Ediacara fauna outside of South Australia is merely an unusual state of preservation. Nevertheless, we are persuaded by the arguments that have been advanced for the approximate contemporaneity of the principal records that the burden of proof lies on those who might prefer a broad temporal transgression. If substantial grounds can be found to question such approximate contemporaneity, then, of course, even if the *cf.* notation is removed from our *cf. Pteridinium*, its bearing on the boundary problem would be seriously weakened.

There remains to consider the position in the geologic column of the Charnian beds, which we do in fact accept as probably correlative with the Ediacaran. Points of evidence that bear on this include (i) the numerical age of the oldest rocks of demonstrably Paleozoic (hence Phanerozoic) age anywhere, (ii) the stratigraphic equivalence of the oldest rocks above the Charnian, and (iii) the dating of the igneous rocks that intrude the Charnian sediments and place a minimal limit on their age. Radiometric ages for the oldest Paleozoic rocks leave much to be desired. The age taken by Davidson (*3*) as most nearly tying down the base of the Paleozoic is a K^{40}/Ca^{40} age on sylvite from Lower Cambrian potash deposits of the Irkutsk region, which gives a figure of 620 ± 20 m.y. (million years) for a point *within* the Lower Cambrian. On this evidence a figure of 650 m.y. before the present for the base of the Paleozoic (and Phanerozoic) is a reasonable working estimate, although it might eventually be found to range down to 700 m.y.

As for the stratigraphic position of the oldest rocks above the Charnian, this is not clear to us. Information available to us until receipt of Ford's comment above has indicated correlation of different immediately post-Charnian rocks with Triassic, Carboniferous, and Ordovician, but nothing as old as Cambrian. If the Charnian rocks are indeed separated from overlying rocks of demonstrably Lower Cambrian age by a major unconformity, as Ford reports (referring to a work in press), then the chances of the Charnian being of Precambrian age are greatly improved—although Ford would probably be among the first to agree that unconformities are not the primary grounds for worldwide geologic time division.

A Precambrian age for the Charnian would be required if igneous rocks that definitely crosscut it could be shown to be chronologically so ancient as to put them clearly within the Precambrian. Unfortunately the conclusion that such intrusives, and therefore the Charnian, are Precambrian rests on potassium-argon age determinations on whole rock samples of porphyroids. Ages of 574 ± 26 m.y. and 684 ± 29 m.y. were obtained, and the latter was chosen as fixing the minimal age of intrusion (*4*). Inasmuch, however, as a satisfactory basis has not yet been established for evaluation of K/Ar ages on whole rock systems, any such numbers proposed contain the possibility of bias for excessive as well as minimal age. Indeed, Ford himself had earlier quoted an age of greater than a billion years for the same rocks (*5*).

In this state of uncertainty we find no reason at this time to emend the propositions set forth in our earlier paper (*1*).

PRESTON E. CLOUD, JR.
C. A. NELSON
Department of Geology, University of California, Los Angeles 90024

References

1. P. E. Cloud, Jr., and C. A. Nelson, *Science* **154**, 766 (1966).
2. P. E. Cloud, Jr., in *Evolution and Environment*, E. T. Drake, Ed. (Yale Univ. Press, New Haven, Conn., in press).
3. C. F. Davidson, *Nature* **183**, 768 (1959).
4. M. Y. Meneisy and J. A. Miller, *Geol. Mag.* **100**, 507 (1963).
5. T. D. Ford, *New Scientist* **15**, 191 (1962).

7 April 1967

Magnesium Pemoline and Human Performance

Ronald G. Smith's report on magnesium pemoline and its relation to learning and memory in man [*Science* **155**, 603 (1967)] contains certain errors. He bases his conclusions on test-

Table 1. Effect of magnesium pemoline on intelligence in man. There were 24 subjects in the first test, 24 in the second, and 23 in the third. Subjects were individuals showing memory defect due to various forms of aging. The individual dose of magnesium pemoline (Cylert, Abbott Laboratories) was comparable, namely, 25 to 50 mg by mouth daily. N.S., not significant.

Time of test	Mean I.Q.	Diff.	*t*	*P*
Prior to drug	73.5			
1 week later	77.4	3.9	2.010	N.S.
> 1 mo. later	82.2	8.0	4.819	0.01

ing which, he states, he carried out 3 hours after administration of the drug. But this drug does not act in man precisely as it is reported to act in the rat. In reality, the action of the drug reaches statistical significance in man only after approximately 1 month of administration, as shown in Table 1.

My second point concerns Smith's curious extrapolation from our work on RNA. Smith states that since we found that RNA is more effective in the least deteriorated patient, therefore magnesium pemoline should be more effective in normal males. One may ask, more effective than in what—normal males or brain-damaged humans?

Surely medical science has many examples which might well have corrected such a conception. Digitalis is more effective in less-damaged hearts than in extremely damaged hearts. Should, therefore, all of us who have undamaged hearts be on digitalis to benefit from this oddly hypothesized gain?

D. EWEN CAMERON
Albany Medical College of Union University, Albany, New York
6 April 1967

Lunar Transient Phenomena

Recently [*Science* **155**, 449 (1967)] Middlehurst and Moore presented a preliminary analysis of a survey of lunar transient phenomena (LTP). Their critical reevaluation of historic observations is valuable in the light of recent reliable observational evidence of such phenomena. Nevertheless, one must be very cautious in analyzing past observations, even assuming that observations by famous observers are reliable.

In particular I question the conclusion that LTP occur preferentially on the edges of maria, in prominent ray craters, and along the moon's central meridian. That they have been seen mainly in those places is beyond question, but probably 95 percent of all observing time has been given to just those places. It seems to be natural for visual observers to concentrate on craters, moderate to large in size, in or on the borders of maria and to avoid the difficult lunar highlands. Since most observers like to draw pictures of "objects," little attention has been given to the flat surfaces of maria or to regions between craters. The list by Middlehurst and Moore of LTP locations is practically a roll call of the most popular craters on the moon, to which systematic observers and casual observers alike (including me) have devoted most of their attention. Most of these craters are popular because they are moderately large, relatively prominent, and easy to sketch with a pencil. A few (Aristarchus and Plato, in particular) have received special attention because of past reports of peculiar phenomena.

Moreover, Middlehurst and Moore discuss the size of the field of view as an important factor in analysis of LTP. While the size could be significant in relation to a visual patrol for LTP, it is much less so relative to past observations. Observers usually have confined their attention to single craters or other features covering only a few percent of the area of the field of view. With their eyes fixed on these features, the observers very likely missed anything short of a catastrophic phenomenon occurring elsewhere in the field of view. Most recent well-documented LTP have been rather subtle and would have been missed by an observer not looking directly at them (or for them).

This discussion leads to several conclusions. First, investigators should be thoroughly familiar with the nature of visual observation of the moon and of LTP so that they can (i) select intelligently the reliable observations from among the much greater number of incorrect or fanciful reports, and (ii) make physically meaningful interpretations of the selected reports. While Middlehurst and Moore are not unaware of some of these systematic factors and do qualify their conclusions to some extent, I believe they are overconfident. In particular, the effects of observational selection are so severe as to invalidate any conclusion at this time from the nonrandom distribution of LTP sites, except concerning the psychology of visual observers of the moon. Lastly, I should emphasize strongly the suggestion of Middlehurst and Moore that observers pick random locations on the moon (and not just craters) for future LTP patrols.

CLARK R. CHAPMAN
15-B Eastgate, Massachusetts Institute of Technology, Cambridge
9 February 1967

Chapman correctly points out that many observers tend to pay greater attention to the more interesting features of the lunar surface. It is also possible that past reports of lunar phenomena may not be entirely reliable. If our conclusions had been reached from consideration of the topographical distribution alone, I would agree with Chapman's suggestion that we may have been overconfident in our deductions.

Our assumption of the reliability of the reports, however, rests also on the appearance of significant correlations of the data with quantities unrelated to observational factors. For example, a strong peak in the frequency of events reported near perigee and a smaller peak at apogee were found, although the lunar orbital period (average, 27.6 days) is incommensurate with the synodic month (29.56 days from new moon to new moon). Lighting and other observational factors could not affect this distribution materially, and the presence of these peaks at a significant level led to the conclusion that internal causes were implicated. The ratio of tidal forces on the moon to lunar gravity is about 100 times greater than the similar ratio on the earth. The strength of the lunar material at depth is unknown, but the release of internal pressures could be affected by the tidal forces to a much greater extent than on the earth (*1*).

In considering the distribution of sites of reported events on the lunar surface, we were aware of and pointed out the possibility of observational bias, but Chapman's questions about distribution may apply more to the degree of concentration than to the gross patterns. It is unlikely that 238 observers (*2*), including most of the famous lunar mappers of earlier times (Beer and Mädler, Schröter, and Elger) and men of lively intellect, such as W. Herschel, Barnard, and J. Schmidt, should have confined their attention throughout their careers to large craters only, as Chapman suggests. Our list (*3*, table 1) does in-