(Dakota, etc.); they find a sharp floral break between Judith and Lance at a point where no break occurs in the vertebrate fauna; and so far as I understand no serious break between Paleocene and Eocene. I can hardly venture an opinion as to where the majority of invertebrate palæontologists would draw the line, if based wholly on invertebrate data; in practise most of them draw it at the summit of the chalk succession of western Europe.

The great stratigraphic break asserted by some stratigraphers to exist everywhere at the base of the Tertiary is denied by others of no less ability and experience, and its universality and importance seem to have been much exaggerated.

Is it not possible, where the evidence is thus conflicting, to adopt a compromise by mutual concession? It appears to me that the compromise indicated by Schuchert has the best elements for universal acceptance. It is in accord with the historic and universal European usage, including the Thanetian in the Tertiary, but none of the chalk succession. It conforms to the insistence of the palæobotanists that the Lance and Fort Union should be kept together. It gives a satisfactory practical base for the stratigrapher in the widespread and characteristic Wasatch formations. It places all the dinosaur formations and the bulk of the "Paleocene" faunas in the Cretaceous where the former certainly and the latter in my opinion properly belong; but the uppermost Paleocene faunas are placed in the Tertiary. The replacement of the Cretaceous by the Tertiary vertebrate fauna would thus be a little later, of the Upper Cretaceous by the Tertiary flora a little earlier than the line agreed upon.

W. D. MATTHEW

NEWCOMB ON EXTRA-MUNDANE LIFE

TO THE EDITOR OF SCIENCE: As one long interested in the subject matter covered by the inquiry of Professor Clark, published in SCIENCE of May 13, I have read with some care Newcomb's essay to which Professor Campbell refers, in the same issue of SCIENCE. While this essay may be presumed to represent an opinion at some time entertained by its distinguished author, an opinion that merits respect, it seems wholly unresponsive to the request for evidence upon which such an opinion may be based. The author expressly admits that "scientifically we have no light upon the question and therefore no positive grounds for reaching a conclusion." In another place, *Popular Astronomy,* ed. 1890, p. 528, he amplifies as follows:

> The spirit of modern science is wholly adverse to speculation on questions for the solution of which no scientific evidence is attainable, and the common answer of astronomers to all questions respecting life in other worlds would be that they knew no more on the subject than any one else and having no data to reason from, had not even an opinion to express.

It is probable that few astronomers will dissent from either of these statements. Most of them, Newcomb included, will concur in the statement that of the hundred or more millions of celestial bodies known to exist it may be shown with a high degree of probability that, barring our two neighbors, Mars and Venus, no one of them is suited to be the abode of animate beings. As to the numerous worlds alleged to be the abode of life, Newcomb in his essay raises the question: "But where are we to look for these worlds?" and replies to it: "This no man can tell." Nevertheless, as quoted by Professor Campbell, he goes on to say:

> It is perfectly reasonable to suppose that beings not only animated but endowed with reason inhabit countless worlds in space.

A major premise upon which this conclusion might rest would seem to be: We may reasonably suppose anything that does not admit of disproof. In the bald form here stated this premise would doubtless be rejected by those who believe in the plurality of abodes for animate intelligence, but without some appropriate equivalent for it there seems to be a hiatus between the conclusion above set forth and the facts that constitute its minor premise. Possibly Newcomb's own words anent this subject matter, *loc. cit.,* p. 531, may be a less objectionable formula:

Here we may give free rein to our imagination with the moral certainty that science will supply nothing tending either to prove or to disprove any of its fancies.

In this connection one is reminded of a famous apothegm,

Faith is the substance of things hoped for, the evidence of things not seen.

George C. Comstock

QUOTATIONS

COOPERATIVE INDEXING OF SCIENTIFIC LITERATURE

We have shown that the core or *umbra* of a subject is comprised in a body of homogeneous literature which unquestionably can best be dealt with by its representative professional society, but that outside this core there exists a *penumbra* of relevant matter dispersed through a literature of gradually increasing irrelevance, with the result that the recovery of the relevant matter can be effected economically only by cooperative effort. The solution, therefore, would appear to be to bring into existence a central bureau which should deal solely with the indexing of periodicals of the non-homogeneous character—and in the first stages of its work, with a restricted list of periodicals assigned to it by the contributory bodies. These bodies would receive from the central bureau entries from the periodicals examined corresponding to their specified requirements. But as the professional abstracts became more fully representative of progress in their respective fields the need for the publication of the corresponding indexes would tend to disappear. The institution, therefore, of a central bureau would ultimately make for economy in all branches of science in which the publication of abstracts is admittedly indispensable.

So far as science is concerned, it will probably be found that the simplest and most effective method for obtaining the necessary index slips would be to invite the Central Bureau of the "International Catalogue of Scientific Literature" to provide them. Indeed, the possibility of cooperation between the "International Catalogue" and the abstracting journals was one of the subjects considered at the conference held last September. Any such arrangement would probably begin with the year 1921, and, as a preliminary, the "International Catalogue" should be brought up to date by the publication of volumes for 1915–20.—*Nature.*

SPECIAL ARTICLES

THE MOTIONS OF THE PLANETS AND THE RELATIVITY THEORY

Constant reference is made to the motion of Mercury about the sun and to the supposed fact that this motion can not be explained by the Newtonian law of gravitation. This current idea is far from correct: the motion of Mercury can be accounted for fully as well, if not far better, by the Newtonian law than by the Einstein law. The difficulty, which has faced mathematical astronomers for many years, is not how to account for the motion of Mercury, but how to account for that motion without introducing complications in the motions of the other planets.

In 1895 Newcomb[1] showed clearly that the motion of Mercury can be fully accounted for, under the Newtonian law, by one of several possible distributions of matter in the immediate vicinity of the sun and the inner planets. He, however, discarded each such possible explanation of the motion of Mercury because of the difficulties encountered in explaining, at the same time, the motions of the other planets. Each possible explanation of the motion of Mercury introduced a new complication somewhere else in the system.

Now identically the same difficulty is encountered by Einstein. His formulas account for the motion of Mercury, but fail to account for the motion of Mars, and introduce a further complication in the motion of Venus. The supposed explanation of the motion of Mercury by the Einstein formulas has been stressed, but the attendant difficulties in the motions of the other planets have been glossed

1 "The elements of the four inner planets and the fundamental constants of astronomy," by Simon Newcomb.