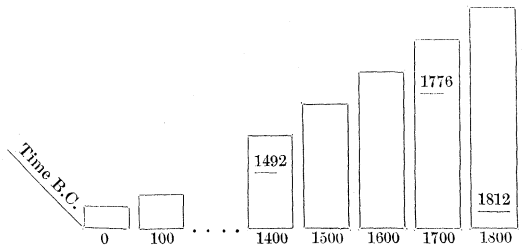
August are so much longer than the other months. This impression of the months applies only to the current year.

When thinking of any event that occurred more than one year ago, I have an entirely different scheme presented. The second figure illustrates it. The centuries are arranged in columns, decreasing in length until the Christian era, beyond which the lapse of time is denoted by an inclined line here shown.

Important dates I think of as lines crossing these columns; and the life of a noted man, or a war, as the space included between two such lines.



I should have said, that in these columns the decades are marked by especially distinct lines. It seems to me there is in these mental associations a large and interesting field for study; and I, for one, would be glad to hear other facts bearing on the subject.

C. RUHEIT.

New Haven, Aug. 6.

The classification and paleontology of the U. S. tertiary deposits.

In the issue of this journal of June 12, Prof. A. Heilprin condemned the second part of my article on the genealogy of the tertiary mollusca of the United States *before* it had been published. Since its appearance, moreover, he has again recurred to the subject in a note in *Science* of July 31. I would here beg permission to defend myself from the charges that he brings forward.

I have shown that the literature affords no evidence, either paleontological or stratigraphical, that the Vicksburgian is the most recent formation; and have given a number of reasons, based upon profiles which I have observed, showing that this bed is most probably the oldest. Professor Heilprin, however, has nothing whatever to say on these subjects. What, then, does he say?

1. "It might appear . . . that the paleontological evidence was in conflict with that derived from stratigraphy. As a matter of fact, however, the paleontological evidence . . . is, as we now know it, absolutely confirmatory of the pregnant facts which the stratigraphy of the region presents; and, indeed, it would be difficult to find a region of similar deposits where it is more so." When he wrote this, Professor Heilprin must have entirely forgotten what he himself had published on the fossils from Wood's Bluffs, (Proc. ac. nat. sc. Phil., 1880, 364–375). There he points out, in spite of certain wrong determinations, clearly and in extenso, the conflict between paleontological and stratigraphical evidence (see pp. 368, 369).

2. "The absence or scarcity of forms of a distinctively old-type facies in the Vicksburg beds, and the introduction there of new forms whose equivalents or immediate representatives are known only from the newer horizon, are sufficient in themselves to establish the position." This statement of Professor Heilprin is new and wholly without proof. My studies lead me to precisely the opposite view. In the Vicksburgian are contained the old forms, while in the Claibornian the new ones make their appearance. The facts upon which I base this statement will be given in another place.

3. I have contested the right to consider and map all localities with Orbitoides as oligocene. Professor Heilprin objects to the older authorities, hence I will here quote the following very recent one. Zittel's Handbuch, vol. i., Munich, 1876–80, p. 103, says, "Orbitoides . . . In der obersten kreide, sehr verbreitet im eocän, im miocän selten." If Professor Heilprin can cite any authority, stating that Orbitoides occur only in the oligocene, I shall be very glad to have him do so.

4. Then Professor Heilprin speaks of Zeuglodon. He argues, Zeuglodon is 'leitfossil' for the Jacksonian; it is known in Europe in late eocene or miocene deposits, hence the inference is that the Jacksonian must be late eocene (or miocene?); the Claibornian is middle eocene (Parisian), consequently the Jacksonian overlies the Claibornian. To give to this argumentation some weight, it will be necessary for Professor Heilprin to prove: *First*, That Zeuglodon occurs only in the Jacksonian, and not elsewhere in America. Having studied the known *facts* which have been published, and having myself seen and collected Zeuglodon at different localities in the South, I have as yet not found evidence to convince me of the truth of this statement. *Second*, That he has a right to parallelize the Jacksonian with any European bed from the presence of a single genus. I found a small bivalve in Jackson, which I should compare with specimens of the genus Kelliella, Sars. If, however, a genus (not a species) can be determined from figures and descriptions alone, there can be no doubt that this fossil belongs to this genus, hitherto only known as recent (and pliocene?) in Europe. Has any one a right to draw from this the conclusion that the Jacksonian bed is recent or pliocene? *Third*, That the Claibornian is middle eocene. My studies and comparisons have demonstrated to me that it would be a laborious and difficult task to parallelize the sub-divisions of the American old-tertiary with those of the European. So far as I am aware, my material for this purpose exceeds that of any other collection. Hitherto I have ascertained nothing to prove that the Claibornian is middle eocene, although it may yet be proved. If Professor Heilprin can prove two of these three-mentioned points, without the third one, there will be nothing convincing in his argument 'to the mind of any unprejudiced paleontologist.'

5. "In that which relates to the oligocene (Orbitoide, Nummulite) rock of Florida, whose existence appears to give Dr. Meyer a considerable amount of anxiety, and which would better suit the requirements of the new theory were it cretaceous, our author need entertain no doubts. The rock is there," etc. I have not the least doubt about the existence of orbitoidic limestone in Florida, observed by E. A. Smith, nor have I anywhere expressed such. This limestone causes me no anxiety whatever. I fail to see why this limestone, if Vicksburg is the oldest bed, should be cretaceous. I have nowhere expressed this belief, nor do I think it will prove to be of this formation. All that I have said is, that there is no reason to map as oligocene localities where orbitoidic limestone is observed, or the larger areas, where nothing at all has been observed.

6. "In such inquiry, it is necessary, however, to

know the relative positions of the different deposits with which one is dealing, and not to proceed, as Dr. Meyer has done, from top to bottom, believing that top was bottom, and bottom top." Professor Heilprin has confounded the limestone at the top of the profile in Claiborne with the limestone at the base, because he has studied the literature regarding it superficially, and because he himself has never seen it in nature. If he puts no faith in the observations of Conrad, Lyell, or my own, he may go there and observe for himself.

7. I fail to see what the fact, that Professor Heilprin has been six years curator of the tertiary shells in Philadelphia, has to do with any conclusion which I have drawn, based upon my larger material. It may be that he has studied these shells attentively; although I have pointed out paleontological mistakes of a surprising nature in his publications on tertiary fossils, and although I had published my regrets that he had not utilized the Philadelphia type-specimens for the benefit of science. The fact, that he once sent me an undescribed tertiary form of the collection as a duplicate in exchange (see 'Terebra trilirata Conr.,' *Proc. ac. nat. sc. Phil.*, 1884, 105, 106) makes me doubt whether this study was as thorough as he intimates.

8. "Pseudo-science, of the kind to which we are here treated, should be exposed." After having worked for years in the old-tertiary formation in Europe, I studied the literature of the corresponding formation in America, following the different opinions from their origin, and tracing out contradictions. Then I went to the spots, observed for myself, and from all these studies I have been forced to conclusions, which, indeed, throw a peculiar light on some publications. Professor Heilprin's way has been to study the literature, and then to write a book on this formation: even to map it, without ever having seen it. He calls *my* way 'pseudo-science!' Without having pointed out one single mistake in my papers, Professor Heilprin uses the expressions, — "geological and paleontological fancies . . . monstrous disregard or ignorance (or both) of the literature . . . misconception of the numerous species . . . vagaries . . . amusing . . . pseudo-science," and warns paleontologists not to accept my species.

I have here fully answered Professor Heilprin's objections, in order to show his method of argumentation, and how useless and how disagreeable discussion with him is. I will not, however, enter into any further controversy with him upon this subject; at all events, not until he has made some examination of the region under consideration, in person, and then confines himself strictly to the discussion of observed facts.

OTTO MEYER.

New Haven, Conn., Aug. 12.

A NEW ENDOWMENT FOR RESEARCH.

IT is usually the case that private endowments for public purposes are made subject to narrowing restrictions, and then it too often ensues that with the lapse of time the very object of the gift is defeated by the restrictions: the letter kills the spirit. It must therefore be a matter of congratulation when a great public donation is left as free as compatible with the general object for which it is made. This is remarkably the case with a noble and munificent endowment established by Mrs. Elizabeth Thompson of Stamford, Conn., an American lady well known for her public benefactions. Her long experience with churches and various charitable enterprises had led her to question whether the money spent in them achieves the greatest possible good. She finally reached the conviction that knowledge is the real source, the impelling power of human progress; and it became her desire, from motives of the highest philanthropy, to contribute to the promotion of science.

When the plan for the establishment of an International scientific association was brought forward at Montreal, and again at Philadelphia, before the great national associations, Mrs. Thompson considered that the proposed international society would be the fittest body to assume the trust she wished to establish. Accordingly she placed in my hands the sum of five thousand dollars as the nucleus of a fund to be controlled by the International scientific association when organized.

Not long since, Mrs. Thompson communicated to me her desire to transfer the above-mentioned sum to a board of trustees, and to add to it at once twenty thousand dollars more, making a total permanent fund of twenty-five thousand dollars. Mrs. Thompson has been as liberal in the conditions she has established as in the amount she has given. According to her letter of conveyance, "The income of the fund is to be devoted to the advancement and prosecution of scientific research in its broadest sense; it being understood that to provide for, and assist in, the maintenance of an international scientific association, is a method of application which seems to me very desirable."

The trustees are left with very great discretionary powers, which are to be guided by certain general directions. It is, above all, expressly understood that the prime object is to contribute from the income towards defray-