very large compared with the time during which a particular doublet is in the neighborhood of B, is proportional to $ee' - k^2$ where k^2 is the mean value of the square of $f(t)$ for all the doublets which pass from A to B and arrive at B in this interval. In accordance with our previous hypothesis it seems reasonable to conclude that k^2 is proportional to the product of the masses of A and B.

If in the interval of time from t to $t + dt$, no doublets arrive at B while a doublet left A in the corresponding interval $t - (AB/c)$ to $t + dt - (AB/c)$ it is clear that the mean value of the electrical force between A and B in this interval depends on ee' and there is no gravitational action. Other cases may be considered in a similar way and it is clear that the gravitational action depends only on the doublets which go directly from A to B. The action of B on A depends likewise on the doublets which go directly from B to A.

The present theory indicates that there may be a slight screening effect when a third body C is interposed between two bodies A and B, for C may be supposed to receive some of the doublets which would ordinarily go directly from A to B or vice-versa. The recent work of Nipher[2] and Majorana[3] thus becomes of additional theoretical interest when it is considered in the light of the present theory.

Gravitational action may be slightly modified, too, by collisions between doublets travelling with velocity c. In this connection it may be worth while to point out that if P and Q are two doublets travelling along different straight lines with velocity c, then after a certain instant it is possible for a particle travelling with velocity c to meet first one doublet, say P, and then Q but not for such a particle to meet first Q and then P. A series of moving doublets may thus be arranged in a definite order; something which happens to one doublet may affect those which come later in the series but not those which come earlier. This result may have some connection with the damping of oscillations in the emission of light. A more imperfect form of the present electrical theory of gravitation has already been published in *Proc. London Math. Soc.*, T. 18 (1919), p. 95, and in the *Messenger of Mathematics*, T. 48, p. 55. The possibility of a connection with the work of Einstein and Majorana has not been pointed out previously. The present theory seems to be free from the objections raised against the older electrical thory of gravitation (see O. W. Richardson, "The Electron Theory of Matter," p. 596), there may, however, be some other fatal objections to it.

H. BATEMAN

[2] SCIENCE, September 21 (1917).

[3] *Phil. Mag.*, T. 39, May (1920), p. 488.

4

PROTOZOA IN SAWDUST FOR CLASS WORK

IN studying the method of excreta disposal by compositing night-soil with sawdust, the chance observation was made that microscopic examination of old sawdust piles revealed the presence of *Euglypha* cysts. Samples of sawdust were used for experimental culture of hookworm eggs and it was observed that the cultures showed profuse contamination with amœba, flagellates, ciliates, and free living nematodes. Samples from old sawdust piles were then moistened and incubated with the result that numerous specimens of protozoa and free nematodes were found.

The sawdust used was chiefly from southern pine.

This note is published with the thought that it may be of practical service to teachers in providing material for class work.

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CONCERNING DIASTROPHISM

Two papers have appeared during the current year which once again bring before American geologists the vexed question of systemic boundaries. In the first Böse[1] concludes that the ammonites found at Tularosa, New Mexico, 200 feet above the base of the Abo sandstone, are of Carboniferous age. This inter-

[1] Böse, E., *Am. Jour. Sci.*, Vol. 49, pp. 51–60, January, 1920.

pretation involves considering the break in sedimentation at the bottom of the Manzano group of that region, of which the Abo is the lowest formation, as having occurred during so-called Pennsylvanian time, and would place the boundary between the Pennsylvanian and the Permian at some undetermined horizon within the Manzano group of conformable formations, as Lee[2] has pointed out. I would not be in the least surprised if this last is not the true interpretation of the facts. It is just what would be expected by any one disposed to doubt the validity of the so-called diastrophic method of correlation, and, if I mistake not, is exactly what occurs in the Appalachian region.

The second paper to which I refer is one by Twenhofel[3] who concludes that all of the Cretaceous below the Benton in Kansas should be referred to the Comanchean, and that the Cheyenne-Kiowa-Medicine sequence of southern and the Mentor-Dakota sequence of central Kansas are the equivalents of the Washita division of the Texas Cretaceous, although it is conceded that the Washita faunas and floras are probably of Cenomanian age and therefore Upper Cretaceous according to European chronology.

Without discussing the merits of these conclusions in this place, I wish to call attention to the more general question involved, which is clearly recognized by Lee in connection with the Manzano group, and which is discussed at some length by Twenhofel in connection with the Kansas Cretaceous, namely as to what are the criteria of systematic boundaries.

I can see no fundamental objection to using such terms as Comanchean as a convenient group or descriptive term any more than in using such terms as Mississippian or Pennsylvanian, disregarding even that Mississippian in its original significance was Cambrian, but to use Comanchean as the equivalent of the European Lower Cretaceous, which it is not in either its lower or upper limits, and thus to bring about a situation where Lower Cretaceous in Europe, Asia, Africa, Australia, and South America means Lower Cretaceous, whereas Lower Cretaceous in the United States means early Upper Cretaceous, appears to me most reprehensible.

American geological literature has been deluged, one might almost say diluted, with diastrophism during the last decade, and philosophers, scientific as well as political, stand on every street corner. Whether there is any world wide periodicity in movements of the strand line as Suess and Chamberlin contend, or whether each region has its individual history as Willis contends, I do not know, although what we know of geological history is all in favor of the latter supposition. I should imagine that sometimes one and sometimes the other might be true, depending entirely on the causes that affect the relative positions of the land and sea in specific cases.

I certainly can see no basis for the "law of periodicity" that Willis writes about beyond the partial fulfillment of Newberry's much older conception of cycles of sedimentation, which are no more comparable in chronologic magnitude than are the life cycles of organisms.

If American geology is to finally adopt diastrophism as the ultimate basis for the delimitation of the more important time boundaries, and it is already clear that the geologists of no other nation are likely to follow our lead, we shall have to devise a different terminology for each continent, or even for different parts of the same continent. For example on our Pacific coast there were Triassic floodings that have been successfully correlated by Smith on the basis of their ammonite faunas with those of the Mediterranean region of the old world. On our Atlantic coast there are no traces of any corresponding events. Exactly similar conditions prevailed in the two general regions during the Jurassic. Lower Cretaceous marine sediments are confined to the southern and western parts of North America, and one might start at the bottom of the geological column and point out very many similar contrasts.

The two continents whose geology has been

[2] Lee, W. T., *Idem*, pp. 323–326, May, 1920.

[3] Twenhofel, W. H., *Idem*, pp. 281–297, April, 1920.

longest studied and is best known, namely Europe and North America, show the greatest amount of divergence even in formations as recent and well preserved as those of the Tertiary, and there are the most striking contrasts in the geological history of Mediterranean as compared with central and northern Europe. The same situation exists upon all of the other continents.

Even supposing that changes in relative level have been due to general causes such as the periodic sinking of ocean basins and the filling of oceans by sediments,[4] which has not been demonstrated and is directly opposed by what we know of geological history, the results as reflected in those chapters of geological history available for our study, would vary with the initial attitude of the land in a particular area, its location with respect to the position of the antecedent sea level, etc. A striking illustration of this is furnished by a comparison of the not very remote regions of Belgium and the Paris basin, or even of the center and periphery of the latter during the Cretaceous and Tertiary.

The question resolves itself into whether geological classification in its major outlines shall be local, that is, provincial and nationalistic, or whether it shall be understood and capable of application in any country. I am one of those reactionaries who believe that classification is a means and not an end, and that, however imperfect the scheme may be as devised for the region first and longest studied, namely Europe, the classic names with the historic perspective that goes with them, should be adhered to in this country.

Classifications are all purely artificial, they are the medium of exchange, and geological time boundaries are no more physical facts than are political boundaries, even though it may be difficult to avoid thinking of them as though they are entities. Probably the best ultimate solution would be to have a universal (international) time scale and a local sedimentary scale as we now have for our formational units. After all the best classifications, whether of geological time, systems of rocks, organisms or igneous rocks, are those most easily understood and used, those in which facts and relationships are not obscured or wholly disguised by names.

Time is continuous, boundaries are always subjective, and the Permian, Triassic, Lower Carboniferous or Lower Cretaceous are to me as essential to clear thinking and the interchange of ideas among nations as are the minutes, hours, days and weeks of current chronology, however illogical these might seem in sidereal astronomy.

The problem of correlation would be immensely simplified if diastrophism could be demonstrated to be of universal application. This is I believe the reason it has so appealed to many, but like its prototype devised by Werner it is altogether too simple to be true.

It seems to me that the most reliable basis of correlation must remain paleontologic until such time as it can be shown that changes of relative elevations are due to changes of sea level, and if this be true there is a disturbing factor in attempting to settle the relative merits of homotaxis versus synchroneity. I have a feeling, however, that homotaxis, although theoretically true, has been greatly overestimated in its bearing upon our interpretations of geological history where we do not have continuous sedimentation to deal with.

Paleontologic correlation, it should be needless to remark, rests ultimately upon the synthesis of all classes of organic evidence, not merely upon invertebrates, vertebrates or plants. How little this truism is observed in practise and to what an extent geological thought is still permeated by Cuvier's cataclysmal philosophy can be appreciated by reading any recent discussions of the boundary between the Devonian and Carboniferous, the Triassic and Jurassic, the Jurassic and Lower Cretaceous, or the Upper Cretaceous and Tertiary. Accepting the doctrine of evolution for life and of uniformitarianism for earth history, the average stratigraphic paleontologist seems determined to prove cataclysms and special creation.

[4] A simple quantitative computation will show how trifling would be the maximum change in sea level from the latter.

The ultimate solution, or at least the one that is most to be desired, it seems to me, will be a universal time scale which shall have its basis in paleontology and shall adhere to the classic names, and in which the cycles and epicycles of diastrophism will be regarded as probably the most useful criteria for delimiting formational or larger sedimentary units, but never *per se* as criteria for the division of geological time.

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SPECIAL ARTICLES

THE INFLUENCE OF DRY VERSUS FRESH GREEN PLANT TISSUE ON CALCIUM ASSIMILATION

In early work on mineral metabolism with both the cow and the goat we showed that milking animals, receiving grains and dry oat straw as a roughage, are brought into a decided negative calcium balance. In the case of a goat the interesting observation was made that after a period of negative calcium balance, followed by access to fresh green grass, a positive calcium balance was observed, using the same ration as was used in the period preceding the access to green plant tissue. In extensive experiments Forbes and associates and Meigs and his associates have observed negative calcium balances with milking cows receiving rations liberally supplied with calcium. The rations used were from air *dried* materials, supplemented in some cases with silage. The striking feature of all the data obtained in these experiments was the large amount of fecal calcium, indicating a failure to assimilate satisfactorily this base.

In these our preliminary experiments, we have used milking goats. They have readily been brought into negative calcium balance on a ration consisting of air dried grains and air dried straw, with more calcium excreted in the fecal residue than was ingested with the ration. When the *dry* cereal straw was displaced by an equivalent in dry matter of fresh *green* material, with no increase in the total calcium intake, the negative calcium balance was reduced in one animal from 1.6–2.7 grams CaO to .6 CaO per day. With another animal it was reduced from 1.5–2.5 grams CaO per day to .3–.8 gram per day. On the low calcium intake of 8 to 9 grams of CaO per week we could not expect a positive calcium balance to ensue, but this remarkable difference in the amount of calcium assimilated from the two rations we believe, has very great significance.

These changes in calcium assimilation are not to be attributed to variation in water intake or to unavailability of the calcium. Apparently there is something having its source in fresh green materials, which controls or assists calcium assimilation. It is suggested that under the extra strain of rapid growth or milk production not enough calcium can be assimilated for the liberal uses made of this element, unless there is present an abundance of calcium in the diet as well as an abundance of this something that assists calcium assimilation. Possibly we are dealing with the anti-rachitic vitamine, assumed as the fourth food accessory factor. In any case this problem touches growth, milk production and egg production. In the case of nursing women the relation of diet to a positive or negative calcium balance and to dental conditions will assume new aspects.

The supposition that we are dealing with something influencing calcium assimilation and which is more abundant in green than in dried plant tissue and consequently variable with the season's milk, would explain the variations in the seasonal frequency of rickets, as observed and commented upon by Hess.

Our data are not yet inclusive enough to indicate definitely the factors involved in this problem, yet we have been sufficiently impressed with the constancy of the observations made that it appears desirable to reemphasize this relation to mineral metabolism which we anticipated some years ago and expressed in an earlier publication.

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