Professor Douglas Johnson, of Columbia University, addressed the annual open meeting of the Syracuse University chapter of Sigma Xi, March 16, on "The rôle of geography in world affairs." On March 17, he spoke at Colgate University on the same subject.

The Council of the Paris Faculty of Medicine, has received a gift of 50,000 francs from Mme. Mathias Duval, widow of the eminent professor of histology. The sum having been given without any conditions as to the manner in which it shall be expended, a committee has been appointed to decide how it can best be employed.

Plans to broaden the scope of the Gorgas Memorial Institute in Panama into a research and teaching institution of international scope are being developed by the provisional board of directors for the United States.

Ernest Joseph Lederle, the sanitary engineer, died on March 7, at the age of fifty-six years. Dr. Lederle was health commissioner of New York City under Mayor Low and Mayor Gaynor.

UNIVERSITY AND EDUCATIONAL NEWS

The latest report on the Worcester Polytechnic Institute Endowment Fund indicates pledges of over $900,000 to date. The committee in charge has no doubt that the entire $1,000,000 will be pledged before Commencement Day. This is the second million of the $2,000,000 fund undertaken, the first million having already been pledged, partly in the form of scholarship funds given by industrial corporations in Worcester.

An appropriation by the Oregon legislature of $271,000 has been made for medical work in Portland by the University of Oregon.

The corporation of Yale University has adopted regulations with reference to research associates and research fellows. Research associates are to have professorial rank, and research fellows assistant professorial rank. The titles are to be given to men of distinguished attainments who devote most of their time to research rather than to teaching. It was voted "That the title of research associate should be confined to men of real distinction in research and productive scholarship, and that it should carry with it inclusion in the list of 'Professors and other officers of professorial rank,' the object of the position being to attract to the university men of eminence, who usually wish greater freedom in the use of their time for research than professorial appointments permit."

Eugene E. Haskill, S.E., dean of the combined colleges of civil and mechanical engineering at Cornell University has resigned. His resignation is to take effect in June of this year after his sabbatic leave, which he is now enjoying. Dean Haskill has been at the head of the college of civil engineering at Cornell since 1905, prior to which he was in charge of the United States geodetic survey of the Great Lakes. Dean Haskill is a graduate of Cornell University, class of 1879; his successor, Professor F. A. Barnes, is also a Cornell graduate, having been granted his degree in 1897.

Dr. Paul Weatherwax, for the past two years associate professor of botany in the University of Georgia, has resigned to accept an associate professorship in Indiana University, where he was formerly instructor.

Professor Irving H. Cameron, for many years professor of surgery in the medical department of the University of Toronto, has relinquished that chair, and Dr. Alexander Primrose has been appointed to succeed him temporarily.

DISCUSSION AND CORRESPONDENCE

ARE THE LANCE AND FORT UNION FORMATIONS OF MESOZOIC TIME?[1]

To the Editor of Science: Under the above title Professor Charles Schuchert has recently reviewed in Science (issue of January 14) a

[1] Published with the permission of the director of the U. S. Geological Survey.

publication of the Geological Survey by Dr. T. W. Stanton on "The Fauna of the Cannonball Marine Member of the Lance Formation." Following the review Profesor Schuchert announces his opinion that the evidence

> binds invertebrate paleontologists and geologists together in the conviction that the Lance and the Fort Union are of Mesozoic time. The U. S. Geological Survey should now reverse its former conclusion and adapt itself to the fuller evidence.

In the first conclusion Professor Schuchert adopts the view of Dr. Stanton and of Messrs. Lloyd and Hares, who described and named the Cannonball beds in 1915, as to the Lance formation, but goes even further than they do in assigning the Fort Union to the Mesozoic. However, it does seem difficult to justify a separation of these formations, making one Cretaceous and the other Eocene.

As a geologist long interested in the Cretaceous-Eocene problem of the Rocky Mountain region, I wish to comment that Professor Schuchert is not warranted in assuming to speak for geologists inasmuch as he does not regard much of the geological evidence. Nor does he give due weight to paleontological data, aside from those of the mollusca. Moreover, it seems gratuitous to assume that the Geological Survey, because it has not adopted the conclusion reached by Professor Schuchert, has not considered in its decisions the bearing of facts concerning the Lance secured by its own investigators some years ago. The Survey geologists have also secured much other evidence.

Now it is perfectly well known to Professor Schuchert that the question as to the age of the Lance and Fort Union beds is a part of a very large problem, involving a conception of the geologic evolution of the whole Rocky Mountain Province from Mexico to far north in Canada. More than a score of more or less local formations, younger than the great continuous Cretaceous section and older than the Wasatch Eocene, are to be correlated and interpreted. These formations present a great deal of varied evidence as to the history of the Cretaceous-Eocene transition period. The Survey has, in fact, based its action, with which Professor Schuchert disagrees, on a consideration of all available evidence.

Investigations of the Rocky Mountain Province and adjacent lower country to east and west, made within 30 years past, have surely proved that the older idea of the diastrophism which characterized the transition from the Cretaceous to the Eocene period was very faulty. The change was gradual, not abrupt, and, while over a large area the great Cretaceous succession was ended, the uplift was epeirogenic for a long period during which erosion and prevailingly continental deposition proceeded, and there was no such abrupt environmental change affecting life upon the land as has been assumed. In general the newer picture of Rocky Mountain development, after Laramie time, gives no basis for the belief that dinosaurs and some other dominantly Mesozoic land forms could not survive into the Eocene. In fact, dinosaurs of the type found in the Lance lived in the Denver epoch, that is, they survived during the period in which the entire Cretaceous section was removed from a large part of Colorado and adjacent regions.

The Lance and Fort Union formations of eastern Montana and adjacent portions of the Dakotas present an exceptionally interesting and important association of stratigraphic and paleontologic data, the subject of conflicting ideas which must eventually be harmonized. Their correct interpretation will contribute much to our understanding of Rocky Mountain history. The most striking data will be briefly specified.

The Lance in some places rests with erosional unconformity on the Fox Hills Cretaceous, the gap being of undemonstrated extent. It may be large, and not small, as Schuchert assumes. In some districts Lance and Fort Union form an apparently continuous section reaching 5,000 or more feet in thickness. In one limited area only, the Ludlow lignitic and Cannonball marine shale members are seen to separate the formations.

A well defined flora runs through both Lance and Fort Union. It is considered

clearly Eocene by Knowlton. This view was not seriously opposed until the flora, first found in the Fort Union, was traced down through the Lance almost to its base. The flora thereby lost much of its interest to vertebrate and invertebrate paleontologists, but not to paleobotanists or geologists.

The Fort Union beds have a mammalian fauna of small forms considered to prove the Eocene age of the strata containing them until allied types were found in the Lance associated with dinosaurs and other supposed Cretaceous forms. The significance of the poor little mammals has seemed to disappear, from certain standpoints, but not from all. The Ceratops fauna of the Lance is closely similar to that of the Denver beds, correlated by the Geological Survey, together with other Colorado and New Mexico formations, with the early Eocene beds of the Gulf region.

The Cannonball shales demonstrate the temporary return of marine waters from an unknown and as yet undiscussed region to the Dakota district, after an absence which was of considerable duration. Where was this sea meanwhile? The known Cannonball fauna consists of two sharks, several corals and foraminifera, all of which range into the Tertiary, and 60 molluscan species. The molluscan group, according to Stanton, has "the general aspect of a Tertiary fauna," but he considers 24 species to be identical with forms in the Fox Hills or Pierre formations of the Cretaceous nearby, while not one is identical with any known form in the lowest Eocene of the Gulf region and 35 are new species.

Dr. Stanton has given, in the excellent publication reviewed by Professor Schuchert, a careful description of the Cannonball fauna and discussed its relationships to Cretaceous and Gulf Eocene faunas. Elsewhere he has discussed the age of the Lance on general grounds but he has always given the greatest weight to the character of the invertebrate fauna, as is natural considering his special point of view.

Professor Schuchert has gained wide reputation for his broad studies in paleogeography. His mature opinion was no doubt expressed in his "Text-book of Geology," (1915, p. 581) where he says:

> It is, therefore, the principles of diastrophism and paleogeography that will eventually correctly define the periods or systems.

It may seem at first thought that this principle guided Professor Schuchert in his opinion that two paleogeographic maps presented by Stanton "are a most striking summation of the problem in hand . . ." That judgment seems, to the writer, far from the truth.

One of these maps (after Schuchert) represents the Pierre Cretaceous ocean as extending from the Gulf of Mexico through the Rocky Mountain region far toward the Arctic, with a land barrier reaching from the east at least to the boundary of Colorado and New Mexico. This barrier may have extended further. The other map shows the supposed early Eocene limits of the Gulf sea and the geographic position of the Cannonball area. What is needed is a paleogeographic map, or several of them, to express a reasonable hypothesis of the course of retreat of the sea as the land barrier rose and apparently cut off entirely a restricted northern ocean from the Gulf sea, perhaps before Fox Hills time. Somewhere there was an open sea, insisted on by Dr. Stanton, cut off from the Atlantic-Gulf ocean, in which the Fox Hills fauna was modified to that found in the Cannonball.

Unfortunately Dr. Stanton does not discuss the origin, the position, the extent, or the climatic and other conditions of the open sea in which this modification took place. He considers that the Fox Hills is the approximate equivalent of the upper part of the *Exogyra costata* zone, which is near the upper limit of the Cretaceous in the Atlantic-Gulf region. He nevertheless recognizes "considerable differences" in the faunas, which he attributes to lithologic facies, geographic separation, and possibly to climate.

It seems to a geologist necessary for the

invertebrate paleontologist to give some attention to the possibility that a northern isolated sea existed into early Eocene time and that its conditions produced a modification of the Cretaceous molluscan fauna naturally different from that arising during the same time in the Gulf region. Does not the Cannonball fauna show what modification had been reached at a time which, under the existing conditions, must be placed in the general time scale by utilizing, instead of ignoring, the other facts of the Lance and Fort Union formations, and also the concordant knowledge of Rocky Mountain history?

WHITMAN CROSS

WASHINGTON, D. C.,

TO THE EDITOR OF SCIENCE: In SCIENCE for January 14, 1921, Professor Schuchert, in reviewing Dr. Stanton's recent paper on "The fauna of the Cannonball marine member of the Lance formation," proceeds to answer this query in a most emphatic and unreserved affirmative. He assumes to speak with authority for geologists and vertebrate and invertebrate paleontologists, but he admits that the "floral brethren" will, of course, continue to dissent. The problem of establishing the line between Cretaceous and Tertiary time in the Rocky Mountain province has been more or less of a storm center for a number of years, but the question can only be settled when all the available lines of evidence have been evaluated and harmonized. Drawing this line at the top of the Fort Union will profoundly affect other areas and other problems, many of which Professor Schuchert appears to have underestimated if not indeed overlooked.

The faith that is in the "floral brethren" is strong! This evidence has been set forth at length on several occasions, but a brief recapitulation may not be without interest. Up to the present time, with one or two minor exceptions, the Fort Union has been everywhere accepted as of Eocene age. It has a very large flora of approximately 500 species. Aside from local stratigraphic and paleontologic considerations, the Eocene age of the Fort Union flora is attested by its affiliation with many European Eocene deposits of definite, acknowledged position, as Ardtun in Mull, Gelinden in Belgium, and Sezanne in the Paris Basin, as well as the Eocene in Greenland and Alaska. This affiliation amounts to many identical and closely related species, as well as identical and related genera. Several Fort Union species are believed to be still living, a condition not known for any earlier American deposit.

The flora of the Lance formation is also a rich one, comprising about 125 forms, some of which, however, are so fragmentary and obscure as to be incapable of more than generic determination. After eliminating the new forms and those that can not be specifically named there are 87 species that are positively identified, all but 15 of which (about 80 per cent.) are found in the Fort Union. It is unmistakably a Fort Union flora, and occurs through the whole vertical range of the Lance formation, some of the most characteristic Fort Union plants being found within four feet of the base of the beds. Of the entire known Lance-Fort Union flora less than 15 species have been reported from Cretaceous beds anywhere, and this number will be reduced instead of enlarged by revision of the floras involved.

Sedimentation was undoubtedly continuous through the Lance and Fort Union formations; in fact, it is impossible to draw any satisfactory line between them. The highest point at which dinosaurs occur is taken as the top of the Lance, but where these remains are absent it has no recognized or recognizable top. If the Cannonball marine member of the Lance formation is Cretaceous then both Lance and Fort Union are Cretaceous, for there is no stopping point short of the top of the Fort Union. Professor Schuchert even holds that there "is here a continuous and unbroken series of deposits from the Pierre and Fox Hills into the top of the Fort Union, and that the reported erosion contacts between the several formations are due to nothing more than changes from marine to brackish and fresh-water deposition, or to irregularities

characteristic of continental sediments, the local breaks not representing a loss of geologic time of any marked historical value."

The plants certainly do not uphold this contention, but they do indicate a very considerable hiatus between the top of the acknowledged marine Cretaceous section and the inauguration of the Lance. The Laramie is not known within this area, but can it be doubted that it was the interval during which in other areas beds of Laramie age were laid down and subsequently removed in whole or in part? That there was an important interval of some kind is also shown by the fact that it was sufficiently long for over 60 per cent. of the marine Cannonball fauna to be derived through modification of the typical Fox Hills fauna.

F. H. Knowlton

PROOF OF NON-DISJUNCTION FOR THE FOURTH CHROMOSOME OF DROSOPHILA MELANOGASTER

During the spring and summer of 1920 I secured genetic evidence that strains of *D. melanogaster* haploid for the fourth chromosome had been produced by non-disjunction, and in November cytological verification was obtained. The fact that non-disjunction of the fourth chromosome is known to occur is perhaps the strongest reason for believeing that the aberrations observed by Dr. Little[1] may be the consequences of non-disjunction. The direct evidence presented by Dr. Little by no means proves such to be the case, which is unfortunate, considering the ample means in *D. melanogaster* for checking up this hypothesis by means of other fourth-chromosome mutants (bent, shaven) and especially by direct cytological examination. Probably Dr. Little will include such evidence in his forthcoming detailed report. For the present, his published evidence is in better conformity with the assumption of a less extreme eyeless allelomorph, or of a dominant fourth-chromosome "minus" modifier. On the non-disjunctional view selective reduction of the three fourth chromosomes present is required, but there is no obvious reason why E and e should always go together in the manner assumed. A simple explanation is supplied on the weak-allelomorph view, for $\overset{\smile}{Ee}$ is the weak allelomorph and the selective reduction $\overset{\smile}{Ee} - e$ is simply *segregation* in the $e^{w} - e$ compound. *Linkage* supplies the explanation on the modifier view, for the E is then a dominant minus modifier in the fourth chromosome, and $\overset{\smile}{Ee} - e$ is simply $\overset{\smile}{M^{e}e} - e$. As far as can be judged from the short account given, all the observed ratios are in conformity with either of these views. Thus, Dr. Little has not proved by direct and available means that the case is actually one of non-disjunction, nor has he proved it negatively by excluding well-recognized alternative hypotheses which are equally valid and even more in harmony with the facts of the case as stated.

C. B. Bridges

[1] Science, 53: 167.

SURVEYING FROM THE AIR

The article on "Surveying from the Air," December 17, 1920, is a summary of the work of the Coast and Geodetic Survey along the lines of aerial photography, and of necessity does not go into the requisite detail regarding the reasons for making the following statement:

> These experiments proved very conclusively that photographs from the air, using present-day equipment, are of little practical value to the hydrographer.

This statement has been noted by Mr. Willis T. Lee, of the U. S. Geological Survey in Science, February 18, 1921, who cites *Comptes Rendus* Tome 169, October 27, 1919, in which mention is made of experiments near Brest where successful photographs were obtained of the bottom at a maximum depth of 17 meters.

During the experiments at Key West, the results of which were the only ones then known to me, occasional successful photographs of the bottom were obtained in depths of 35 feet and less. No attempt was made to photograph at greater depths. When the conclusion regarding the "practical value" of the photographs was arrived at, all factors re-