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ART. I.—*Present Tendencies in Paleontology*;^{*} by
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When a few days ago your president asked me to take part in the meeting to-night I felt bound to accede for friendship's sake, rather than because of any message I had to deliver. On the eve of my departure from the country I have had no time to formulate and marshal what few ideas I have on the subject. Your president in his wisdom must have had a motive, otherwise why call in an Antony when Washington is full of Brutuses. Recognizing as I do certain iconoclastic temperamental trends in myself, I suspect that he may expect that I will lay about me lustily in an endeavor to crack a few heads, and in the words of the poet "stir up the animals." I am resolved, however, to overflow with the milk of human kindness, and to shed sweetness if not light upon the subject.

One is embarrassed to decide whether to attempt the difficult role of historian of present tendencies where there is such grave danger of not seeing the forest because of the trees; to take the allotted time in criticism of past and present accomplishments in paleontology, or in an endeavor to sketch the things hoped for in the golden era of the future.

The normal course of events has been so muddled by world conditions during the past few years that it is difficult, nay impossible, to discern with any clarity the present trend in paleontologic research. If I were asked to state the tendencies as they appeared to me prior to 1914, I could do no better than sketch certain trends that

^{*} Address read before the April meeting of the Geological Society of Washington.

were essentially nationalistic, although such generalizations inevitably do great injustice to individual genius in all countries, and like all generalizations are only entertaining half-truths.

Paleontologic work in France, particularly in the vertebrate and plant fields, was characterized by breadth of view and philosophy of interpretation such as it has always exhibited and men like Douvillé and Kilian in the invertebrate field were fully sustaining the national tradition. Perhaps nowhere have the problems of faunal facies and their lateral variations been as conclusively solved as in that country. German paleontology ran true to the racial temperament. The quantity of detailed descriptive work was probably greater than in any other country, the quality was not especially high and there was no correlation between facts and fancies. The scramble for self-advertisement and professorial advancement may be illustrated by Steinmann's wild book on evolutions, or Jaekel's speculations on the classification of trilobites, or Arldt's book on paleogeography, or to go back to an earlier day by the factory for the manufacture of subjective phylogenies which Haeckel operated at Jena for so many years. Crossing the channel you will, I think, agree that British paleontology had the solid qualities characteristic of things British. Oftener than not the work was absolutely solid. The point of view was still that of the founders. Whatever had been good enough for Sedgwick or Murchison should be conserved to the bitter end. The younger generation was busily engaged in trying to make over and patch the outworn garments of paleontology, not daring to suppose that what had been insoluble to the grand old men of British geology was capable of solution. (I quote substantially from a letter from an English friend.)

One cannot probably get into a sufficiently detached frame of mind to visualize correctly the true position of the United States in the present status of paleontology. I think we undoubtedly exhibit a provincialism and radicalism that goes with young nations as with young individuals. Of one thing I feel reasonably sure, namely that the future belongs to us if we keep our ideals high enough. Our scientific, like our economic, opportunities, are very great.

A French paleontologist borrowing the metaphor from

the diurnal rotation of the earth writes me that the paleontologic sun is setting in Europe while the dawn is just breaking in America. After making the proper deduction for the felicity of Gallic politeness there is a grain of truth in the figure.

The two Americas stretching from the abundant Arctic lands to the north of our present continent, southward far into the southern zone, and with a very obvious former connection with Antarctica; possessing a fairly typical representation of all the great systems of rocks except for the weakness of our known Permian, Triassic and Jurassic history,—no region on the earth is as strategically located for the solution of problems of earth history or the former distribution of life—both marine and terrestrial.

We are, then, “called to a high calling” and have a mission to fulfill beside which the imperialistic dreams of reactionary political prophets are but as ships that pass in the night.

In paleontology, as in all branches of human endeavor, there is nothing more obstructive of progress than a reverence for old ideas and systems which have outlived their usefulness. This is especially to be guarded against in an organization where rules and standards have to be formulated for the guidance of field parties and where there is an obvious necessity for laying down certain classifications for the presentation of results in reports and upon maps. There is a tendency, well illustrated by the Geological Survey of the United Kingdom, for official sanction to lag about a generation behind the advance of knowledge. On the other hand there is the great danger that we in America, in a scientific isolation paralleling our former political isolation, filled with pride at the size of our country and the number of pages of geological contributions printed annually, may neglect not only the past but the present state of our science in other lands. I believe that a knowledge of the historic development of paleontology and the details of the European succession is of the utmost importance, for Europe is after all historically the type continent despite the untypical development in a world sense of so many of its geological horizons.

Undoubtedly the geological history of North America is much fuller and more normal than that of Europe, as

has been frequently pointed out, and if civilization had flowered first in the Western instead of the Eastern hemisphere, we should have to-day a much more logical geological column. But it was otherwise ordained, and if, adopting the insular motto that North America is good enough for us, we make our scientific horizon coincide with our political horizon, we lose that breadth of view and perspective that is such a necessary part of our philosophy. We exchange for the vocational state of mind of a State University that intangible leaven indicated by the much abused word culture, which depends on point of view or perspective. The average European paleontologist has almost invariably a more cosmopolitan viewpoint than the average American paleontologist—an outcome of his training and the fact that the world is his field. It seems to me that proposals such as the elimination of the Permian as a system, or the lumping of the Triassic and Jurassic into a single system, are examples of our provincial point of view, entirely ignoring, as they do, the great development of marine series of these ages in other parts of the world.

The inertia of old ideas and the vitality of traditions, even in radical minds, is astonishing. Witness the slow death of the notion, inherited from Brongniart, that the formation of secondary wood in stems, such as *Sigillaria*, stamped their possessors as exogenous seed-plants. Witness the implications, still alive and vigorous, that march in the train of the notion that an Age of Reptiles is a chronologic and geologic unit. Perhaps the most striking instance of what I am seeking to illustrate is furnished by the survival of Cuvier's conceptions in stratigraphic paleontology. I doubt if there lives a paleontologist who would defend the theorem that faunas or floras were repeatedly exterminated by cataclysmic revolutions and renewed by special creations, and yet when you see the average paleontologist in action, his logic is inevitably colored by the assumption that a floral or faunal unit had an objective reality and is not merely a cross section of the tree of life at a particular time. Nothing it seems to me is more pernicious than the idea that, perhaps poorly determined, formational boundaries are circuit breakers in the continuous life stream that has flowed down to us from the immeasurable past. This is especially illustrated in the discussions of the

more important boundaries. Where a well-marked time-interval intervenes between two normal marine units exposed to our investigation, it is easy sailing, but when the hiatus is small or is partially bridged by marine formations elsewhere, or by preserved continental sediments—disputation is endless. I need only cite in support of this contention the Hercynian, Rhaetic, Wealden and Laramie questions. When terrestrial sediments and life replace marine sediments and life in a single section or vice versa we insist that the particular marine fauna vanished or appeared with the particular retreat or advance of the sea in that region and that the terrestrial fauna and flora appeared or vanished with the deposits in which it is found. That the terrestrial and marine organisms were contemporaneous over a much longer interval than is represented by the particular associated deposits is apparently never considered. This might be illustrated by a discussion of the age of some of our Cretaceous formations, but I pass on to the broader question, often lost to view, that our systematic units in so far as their contained floras and faunas are concerned are purely subjective academic pigeon-holes and if we had the whole record we probably could not differentiate Silurian from Devonian, Jurassic from Cretaceous or Oligocene from Miocene. We have hoped for much from the so-called method of diastrophism, and it has undoubtedly immeasurably widened our stratigraphic horizon and rationalized many outstanding problems. As an absolute criterion for the determination of larger units or as affording the basis for the rhythmic timetable, it was foreordained to failure. I can see no more reason for assuming that two successive cycles of sedimentation correspond in relative duration, than there would be for assuming that because a man, a turtle and an elephant are born, live and die, that all endure for the same number of years. Any succession of changes is in a sense rhythmic, but the elaboration depends on the location of a particular section with respect to the direction and distance of the basin from which the transgression emanated. We would make a sorry mess of it did the segregation of Permian, Triassic or Jurassic rocks depend on their visible development in North America. The mid-Tertiary section in the northern Paris basin represents nearly continuous sedimentation while farther

south there are many breaks. Neither section could be correctly interpreted in the absence of the other.

Progress in paleontology can only result from the action and reaction of the two parallel lines of human endeavor, namely, the accumulation of facts through exploration, research and discovery, and the elucidation of the accumulated facts through advances in philosophic interpretation. The temple of science remains merely a pile of bricks and stone until each brick and stone is fitted into its proper niche. These two lines of endeavor rarely develop proportionately. The accumulation of fact usually far outruns their adequate interpretation, for example, paleontology made rapid progress in the early years of the 19th century through the discoveries of Cuvier 1769-1832 and because of his genius as a comparative anatomist. It was checked by his conspicuous failure as a natural philosopher, exemplified in the invertebrate field by d'Orbigny's (1802-57) 27 distinct creations. His successor Owen (1804-1892) was similarly endowed with gifts of descriptive industry and was a still greater failure as a philosopher. The slowing of the wheels of progress by false philosophy is well illustrated by the historic influence of the dogmatic doctrines of the so-called Neptunists emanating from Werner and his students (1750-1817). One might mention many similar instances nearer our own day if more were necessary.

It would be a fine thing if paleontologists could imitate the practice of business concerns in periodically taking an inventory and making up a balance sheet, writing off the moribund theories and discarding obsolete methods, and determining if there was sufficient gold in the treasury as a reserve for the paper in circulation. For years, invertebrate, vertebrate and plant paleontologists have seemingly been largely actuated by a desire to merely multiply the diversity of the organic record. Zittel was the first to bring into prominence the truism that fossils are not primarily "things dug" and to be studied like minerals, but as belonging to the dynamic world of once living things—a part of a biota and something multifariously interacting with the particular organic and physical environment. Vertebrate paleontology has probably been foremost in stressing the biologic aspect of the subject, while the others and particularly the plant

side have lagged behind. Stratigraphic paleontology cannot, however, be divorced from biological paleontology without becoming sterile. Historical geology which is the ideal we strive for is a vast synthesis woven of many diverse strands—the warp is stratigraphy but the vari-colored woof is furnished by a multitude of criteria and we cannot ignore a single leaf lobe or venation pattern or tooth cusp or bone facet or loop pattern or hinge plate without a knot or break in the fabric.

Fossils are not to be looked upon merely as medals of creation to be transmitted to the paleontologist for report, resulting usually in a hasty and ill considered list of “sp”-’s, “cf”-’s and question marks, to be used as padding for some printed report. Neither is there room in our science for the closet naturalist who cannot see a contract nor tell the bottom from the top of a section in the field. Paleontology is equally crippled whether divorced from biology or stratigraphy.

Bird’s-eye methods that cannot discriminate between mid-Cretaceous and mid-Tertiary Foraminifera are of no service to geology. Loosely drawn genera and species are no longer useful. Witness the transformation of the genus *Olenellus* into the wonderful family *Mesonacidae* in the skillful hands of a Walcott. The precise systematic methods introduced by Waagen (1869) and so largely exemplified in the work of Ulrich and David White, which seek the recognition of the most minute mutations—often somewhat contemptuously referred to as the splitting of hairs—is the only method by which paleontology can contribute to stratigraphy. In paleobotany the older bird’s-eye obscurantist method has no living champions and the time is not far distant when all loose generic aggregates like *Spirifer* and *Venus* or *Zamites* will join the limbo where now dwell *Ammonites*, *Goniatites* and *Ceratites*, and only emerge as useful descriptive terms purged of generic significance. The same is true of broadly conceived specific aggregates. The poorer the diagnosis and illustration of a species at the hands of the paleontologist, the greater the variety of diverse things that come to be called by the same name, and I could give you many illustrations proving that the more common names in lists drawn up from different regions, particularly if they are the work of the earlier workers, are absolutely worthless. This is espe-

cially true of the work of Ettingshausen, Geinitz, Lesquereux, and their contemporaries in Carboniferous paleobotany. Even where identity seems assured as in dealing with cortical remains of forms like *Sigillaria* and *Lepidodendron*, assumed cosmopolitanism is vitiated by the discovery that identical surface form between specimens from Europe and America was accompanied by slight differences in anatomy or specific differences in cone structure.

From Moses' account of the spread of the passengers of Noah's ark to Matthew's recently published *Climate and Evolution*, many attempts have been made to explain the origin and migration of organisms. It has taken a long time for naturalists to realize that modern distribution has its key in ancestral distribution, or to discriminate the fluctuation of life zones from such very different seasonal phenomena as are displayed by the migratory birds. It would perhaps be better to eliminate the word migration altogether and use the term dispersal, since the criteria of voluntary and involuntary action are of extremely doubtful validity.

The time of origin of an organic type or assemblage, the place of origin, the area once occupied and the time of extinction or the area now occupied, are among the most important questions with which we have to deal. Obviously without the correct chronology such questions are insoluble, hence the importance of far flung correlations and the need for the most critical criteria for correlation.

Similar successions of fossil-bearing sediments in different areas naturally resulted in this correspondence being considered indicative of synchronicity. Huxley in his anniversary address to the Geological Society of London in 1862 was the first serious critic of this conception. He, as you know, proposed the term homotaxis for the alternative idea due to the necessity of taking into account the time consumed in the dispersal of organisms. Those who adopt the latter and apparently reasonable assumption sometimes take the position (E. Forbes, N. S. Shaler) that similarity of organic content, instead of being indicative of chronologic synchronicity, proves that the compared deposits could not have been contemporaneous. Conceding that this view grossly exaggerates the importance of the time element, it is to be noted that of

late there has been a tendency to deny altogether the validity of the homotactic viewpoint.

The question is vital in a consideration of past evolution, distribution, climatic conditions and paleogeography. It is also almost infinitely complex, and there are various underlying conceptions such as the rate of spread of different classes of organisms and the degree of cosmopolitanism reached by marine organisms during times of land emergence and of terrestrial organisms during times of submergence when the obverse records are largely wanting, that have a very important bearing. If the conditions, both geographic and topographic, which are predicted for the various Appalachian troughs or basins during Paleozoic time, are correctly interpreted, as there seems to be no reason for doubting, we are introduced to an environment which is special in the sense that it is not duplicated at the present time anywhere on the earth's surface so far as I can see. This being true the generalizations derived from the study of the overlaps and the rapid floodings of these Appalachian basins must be applied with great caution to other sets of conditions such as determined the broad seas of Jurassic, Upper Cretaceous or Eocene times.

If our present Coastal Plain margin were to take another dip beneath the ocean, would it be possible for the paleontologist of a million years hence to establish the synchronicity of deposits formed at the same time along our middle Atlantic and Gulf coasts or to differentiate these chronologically from such late Pleistocene shell marls as those at Wailes Bluff at the mouth of the Potomac or Simmons Bluff in South Carolina? I think it would be feasible, and am inclined to disagree with the universality of the statement (Ulrich) that for stratigraphic purposes the coarseness of the distinguishable chronologic units obviates the necessity of attempting to deal with the theoretically true time involved in dispersal. This may be perfectly true, however, of some of the Paleozoic transgressions over the base-levelled Appalachian troughs and also when dealing with marginal invasions around the borders of a single oceanic basin where the faunas have had time to become generally distributed.

Our conclusions usually do not rest upon irrefutable logic, however, and it is most important to determine by closer analysis the interlacing waves and ripples of dis-

persal of animals and plants that have been going on since the beginning of life—as well as the rapidity of radiation of different types of organisms. Paleogeography will be on a far less speculative footing when it rests on proof and not on authority.

I do not believe that we can safely generalize with our present stock of accumulated knowledge. Take a theoretical case of a transgression and assume that the rate of change of level amounted to a foot a century, which I suppose would be considered fairly rapid, and that the submergence amounted to 500 feet, the time involved would be 50,000 years. The Upper Cretaceous transgression represented by the Dakota sandstone and Benton involved perhaps twice as great a change of level, and disregarding any halts or oscillations of the strand it would still mean that 100,000 years were involved in the operation. Inevitably there would be changes in salinity and climate which must be reflected in the faunas. It seems to me that we must either admit a certain measure of validity of homotaxis in all except special cases or assume that the breaks between faunally distinct formations represent very great lapses of time. On the other hand changes in faunal facies in passing from a formation like the Onondaga to the Hamilton mean merely a change in local environment such as is, I imagine, responsible for most examples of recurrent faunas, so-called.

The question is also influenced by what the term fauna denotes. Does it mean the whole biota or only certain forms considered as typical. Certainly I should expect *Belemnitella* to spread more rapidly than the contemporaneous *Exogyra*, or an Echinoid more rapidly than a *Pentremite*. The varying vitality of organisms under adverse conditions, either as mature animals or in the larval state, is also a factor of great importance. Larval oysters are very intolerant under adversity while other sedentary molluscs have a much more hardy progeny. Another factor in distribution is the relative length of the free swimming larval stage in sedentary forms. There are wide limits of usage as to what characterizes a fauna and what are its critical members. Shall we rely on its more abundant dominant species, on the percentage of species common to another fauna of known age, to the first or the last appearance of certain forms, or shall we place the greatest weight upon the rarer short lived

types? It seems to me that no single rule of general application can be laid down. There should be no dogmatism! In general the broadly conceived species which are abundant, are long ranging and of less value than the perhaps rarer more restricted types. One type of organism may be much more valuable than another. I should regard the active Zeugledon of the open sea as a much more critical indication of Upper Eocene age than a dozen species of Mollusca. Similarly I should regard the sea lizards of the Upper Cretaceous or fishes like Pycnodus as of much more diagnostic value than species of Exogyra or Inoceramus. The wider removed the areas to be correlated, the more important are the geographically wide ranging and geologically restricted forms and the greater the importance to be attached to their initial appearance.

Progress depends on research, as even an outcrop chaser in Oklahoma would probably admit, but research is about as much abused a term as culture. Research to the neophyte at the university, particularly in current biologic and psychologic investigation, consists in "having a problem" and I often wish that the Board of Health classed "having a problem" along with other communicable diseases and would quarantine its victims.

True research does not depend on subject matter *but on method* and the invidious distinction and discussions of pure and applied science would have no point were it not for the pragmatic individuals, false and mercenary ideals and superficial Burbank methods that characterize so much of applied science.

I should wish to depreciate the tendency, rampant throughout the world, and accelerated by war conditions, to seek a justification for research as a means toward some economic end. If the elucidation of earth history and the origin and evolution of life on the globe are not of prime importance as ends in themselves; if the whence and the why and the whither are not supreme, then indeed has our lot fallen among evil days.

Research is, I suspect, a dangerous subject for discussion before a body of men the majority of whom are connected with a great Federal Bureau. There are so many very necessary and commendable public services crying for accomplishment, and there is so much justification for the pious wish to give the people what they think they

want, that it is not to be expected that the pragmatist and the idealist will contentedly lie down together like the proverbial lion and lamb, or that the Survey will ever lack for critics or defenders. Without posing as either may I not venture to hope that research will constantly increase in both quality and amount, and that the day will speedily arrive when a first rate paleontologist can command as large an income in the successful practice of his profession as he can in an administrative position.

I have, I fully realize, inflicted upon you to-night a few rather poorly articulated and in some cases trite illustrations. A large subject hastily presented always leads to half truths, unless elaborated in much greater detail, and I can only hope that those who follow me in the series will display a greater competence than I have done.

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