Art. XIII.—*Synthesis of the Paleogeography of North America;* by Edward Suess.*

It is only now, after more than four months of fatigue, that I can sit down to answer your kind letter of April 20 and thank you for the transmittal of your great paleogeographic memoir [The Paleogeography of North America] and the honour of having my name on the title page beside that of your illustrious Dana. I believe I cannot express my deep feeling of gratitude better, than by trying to enter into a candid comparison of the existing differences of views as they result on both sides of the Atlantic from differences of personal experience, and differences in nature; further, from those variances that are caused by different systems of classification or nomenclature, and which, as results from your memoir, are all governed by the indisputable fact, that great eustatic movements of the strand-line have taken place.

I intend first to write of tectonic influences on the distribution of seas, second to compare several great eustatic phenomena, and third to discuss the difficulties in finding an explaining hypothesis.

1. Tectonic movements influencing the distribution of seas.

First, I must confess myself a heretic in all regarding isostasy. I have in my last volume given the facts [IV, 1909: 608–] which cause me to doubt anything like a deficit in gravity beneath the mountains. Faye has always doubted it and, if I am not wrong, Professor Gilbert seems also to partake of this view. There is not sufficient space here to enter into this question and I only permit myself to doubt likewise whether any sinking can be caused by loading. All these loads seem trifles in comparison to the magnitude of the planet.

The ideas of Dana on mountain-making were the conception of a great genius. Experience tells us now, that caution is necessary in the use of terms like syncline, synclinorium, etc. I formerly used these terms for structures that are produced

* This very valuable contribution came in the form of a letter dated Marz (Márczfalva), Hungary, September 2, 1910, and addressed to the undersigned. The subject matter is of so much importance to geologists that it should have wider circulation than that of a personal letter, and it is here published with the author's consent. It will be seen that as yet geologists cannot explain several of the more fundamental characteristics on The Face of the Earth but that we are approaching a determined synthesis. This desideratum will come all the sooner through the life work of Professor Suess in making accessible the garnered geologic knowledge of all lands, printed in a multitude of languages, in his "Antlitz der Erde," or in the English translation by Sollas and Sollas, "The Face of the Earth."—Charles Schuchert.

by folding, of which both halves or sides form parts of the same tectonic unity. This is not the case with the great "synclinoria" in front of the mountain chains, the "Vortiefen" or "fore-depths" [see *ibid.*, IV; 626].

The great Pacific fore-depths of 7,000 to more than 9,000^{m} are evidently not caused by folding, as one side is formed by the foreland (mostly covered by the ocean here) while the other is the front of a folded chain. This is clearly visible wherever such a fore-depth enters the continent, as for example in the valley of the Ganges, where one side is the Peninsula (=Gondwana land), and the other the Himalaya, or the valley of Guadalquivir in Spain between the old foreland of the Meseta in the north and the young folded cordillera (Betic) in the south. It is clear that the Pacific Asiatic volcanoes have nothing to do with these depths; they always remain inside the more or less arched chain of mountains. The folds of the cordilleras advance towards the depths and seem to reach them. Sometimes the depths are filled with very thick masses of terrestrial sediments, coming from the new-born cordilleras, sometimes with deep-sea sediments having Radiolaria, as in front of the Carpathians and parts of the Eastern Alps. I regard the thousands of feet of Carboniferous sediments, partly marine and partly continental, which accompany the front of our pre-Permian chains extending from Silesia to southern England as the filling of such a fore-depth. Marcel Bertrand was right in drawing a line from the Bretagne to Newfoundland and in regarding the Appalachian coal fields as the continuation of those of southern England, Belgium, etc. All this is in harmony with the remarkable words of Dana on the existence of a greater trough or deeper channel on either side of the Azoic nucleus and perhaps also gives a hint as to the northern limit of your Poseidon ocean. But this trough is no synclinorium and no anticlinorium exists.*

It will be too great a digression for me to describe here the great Asiatic depths and I prefer to write briefly of the difficulty which arises from the fact, that a transgressing sea enters a fore-depth in one case and an extended river valley in another. Then, too, the contour may be very similar, but the thickness of the deposits is by far greater in the fore-depth. The Cretaceous Flysch with Fucoides and Inoceramus has

* "The region toward the Atlantic border, afterward raised into the Appalachians, was already then, even before the Lower Silurian era closed, the higher part of the land" (319). "We hence learn that in the evolution of the continental germ, after the appearance of the Azoic nucleus, there were two prominent lines of development, one along the Appalachian region, the other along the Rocky Mountain region—one, therefore, parallel with either ocean. Landward, beyond each of these developing areas, there was a great trough or channel of deeper ocean waters, separating either from the Azoic area" (344). Dana, this Journal, vol. xxii, 1856.

filled a great part of the fore-depth of the eastern Alps, Carpathians, and Apennines, and it is very curious that similar beds occur on the external (southern) side of the Alaskan (Aleutides) arch [described fully in "Face of the Earth," IV: 376-378]. Your Coloradoan Sea with the posthumous folding of the Laramide Range and the pressure from the Pacific agrees perfectly with European experience.

Other examples are not so definite. Take the Middle Jurassic. A transgression of this age appears on Franz Josef land and other islands of that part of the Atlantic, attains the northern coast of Russia, forms a broad strip on the west side of the Ural Mountains, attains the Caspian, mixes with Tethys, but lies in transgression beyond the borders of this sea in eastern Bavaria, and at the same time, with very similar fossils, appears in the Argentinian Andes, spreads farther than the southern borders of Tethys beyond Damascus, lies on Gondwana beds in German East Africa, also in transgression on old rocks in Khach (East India), as well as in western Australia, southern New Guinea, etc. This same transgression is met with in different parts of northern Siberia as a wide flat series of beds. It is the Enochkin and Naknek of the Alaskan peninsula and your Sundance transgression (Logan Sea).

In some places the middle Lias has left traces in the regions beyond Tethys, as in Madagascar; *Ammonites amaltheus* of the Lias has been found beneath this transgression in arctic Siberia (I believe on the lower Anabar but have no books here), but with these few exceptions the transgression of about Kelloway age everywhere rests on by far older rocks.

It is the strip along the west or front side of the Urals which connects the Arctic with Tethys, and eminent Russian geologists thought that a syncline was formed in front of the Urals. But curiously enough, transgression also proceeds from Tethys far to the south, and I am inclined to believe that the strip along the west side of the Urals was simply due to the sea entering a river system, let us say of a pre-Jurassic Volga.

I do not know enough about the relations of your Logan Sea to the Oregon Jurassics and of these to the Franciscan Jurassic Radiolaria to speak about them and only desire to point out the necessity of comparison and the difficulties. The evident entrance of fore-depths into the area of the existing continents, as for instance the one in front of the Carpathians and Alps extending through the middle of Europe and depositing Jurassic Radiolaria even in the suburbs of Vienna, always has prevented me from acceding to the opinion, that only "epi-continental seas" had entered the present continents. In Europe north of the Alps mountain-making ended before the upper Carboniferous or upper Permian, and coin-

cides very nearly with what you say about the fixing of the broader relations by the Appalachian Revolution of the Atlantic realm. Only sinking of regions seems to have occurred since that time in northern Europe.

2. Comparison of several great eustatic movements.

It has long been known that the stratigraphic series of the Alps is more complete by far than that of its foreland. Within this foreland the pre-Permian mountains (which I compare to your Appalachians) are to be separated from those regions in which no orogenetic process is known since the beginning of the Cambrian. Such I once named Archeboles, but the name is bad, because pre-Cambrian folds are well known in these same regions. The environs of Saint Petersburg and the rim of the Baltic shield are types of horizontal Cambrian.

This difference between the less extensive marine series of the foreland and the more complete series of the folded chains seems to exist all over the world with few exceptions (south-eastern Himalaya where the fore-depth cuts off part of the foreland, Mackenzie district and Argentina where the folds enter the less complete series of the foreland, the Jura Mountains which form a sort of complicated parma with a transitional series). In seeking to compare the marine series of the United States with that of Europe, I believe I ought to divide the immense array of facts offered by your maps into three groups or regions, as defined by Dana in 1859, viz., (1) the so called Azoic nucleus or Laurentia, (2) the Appalachians and (3) the western mountains. To these I add as a fourth area the United States Range of Ellesmere land which is thrust on Laurentia from the north and adds a new example of the completion of the marine series as soon as a folded region is entered.

Of these four regions Laurentia presents the imperfect marine series of an undisturbed nucleus or shield and has much in common with Gondwana land; Appalachia may be regarded as the continuation of the pre-Permian mountains of Eurasia (Altaides); the United States Range seems to be an Asiatic fragment; while the Mesozoic and upper Palæozoic of your western mountains has decided relations to Tethys. In this I believe I am not in contradiction with your results.

Europe cannot boast of a Palæozoic series comparable in completeness with that of the United States. I have read with great interest what you write regarding the interrelation of Atlantic biota such as that of the Paradoxides fauna, but I fear I am not able to say more about the older palæozoics than I have already said in my "Face of the Earth;" perhaps

other information will be found in the last volume, which was not at your disposal. I will restrict my remarks to the undisturbed region extending to Texas and the western mountains.

These undisturbed regions (those in which Cambrian is not folded) not only show the clearest marks of the negative periods, but also the slow creeping upon them of the transgressions. The negative marks of the strand-line are found more rarely and with difficulty in the folded regions with their rich marine series.

A great negative phase appears at the limit of Jurassic and Cretaceous (Comanchic). This is the Wealden, extending certainly from Poland across Hanover, southern England, Spain, Portland, to the Potomac beds of Maryland, Texas, and Colorado to Alberta, etc. If no other case were known, this one would be sufficient to prove the wide extension of similar movements. The contrast is given by the Spiti beds of the Himalayas, as described a short time ago. In England the Jurassic ends with oscillations in the Purbeck, then follows the Wealden as the time-equivalent of lower Neocomian (Berriasien) and then the marine Cretaceous series. In Himalaya all is marine and difficulties exist in separating the latest Jurassic (upper Tithonian) from the Berriasien. At this time climatic differences seem to have existed in the seas (Knoxville?).

I do not think that lower Neocomian exists in Texas; the oldest forms from Trinity seem to me to be Gault, according to Kilian's determination, and although it may seem daring on my part, I venture to state that the equivalents of the European Cenomanian (upper Greensand) begin within the Fredericksburg. This is the introduction to the great transgression known to me (your Coloradoan Sea is a part of it). The full series exists with lower Neocomian in the southern Andes as well as in the Alps, but in leaving these one sees the Gault creeping over older rocks in northern France while the Cenomanian transgression spreads from the United States through Europe, covering the Sahara from the Atlantic to the Nile, then passing southern Russia and attaining even the desert near Kashgar. Then there seems to appear a pause or even a small regression during the Turonian, perhaps coinciding with your remarks about Pierre, and after this the maximum of the transgression is attained in the Senonian with outliers in the Arctic (central western Greenland) as well as in the Antarctic (Scott).

Next appears the great and probably rapid negative movement of the strand-line, which forms the limit between the Mesozoic and Cenozoic. North America indeed possesses extremely little lower Eocene. This absence seems to occur

all over the world in the undisturbed regions except parts of the Sahara.

I will try to point out but two peculiarities of the upper Cretaceous transgression:

a. The great inundation does not, as far as I know, attain the northernmost coasts and islands of Eurasia. Here, on the contrary, transgressions appear in undisturbed (only faulted) regions, a condition which we are not accustomed to see in these areas. Such are the upper Carboniferous and Triassic marine beds of Bear Island, etc.; the Kelloway, etc., which has been described; then Lias at one locality in northeastern Siberia; the Volga beds of lower Cretaceous age in northern Russia; the coast of Siberia, also spreading to the center of Russia; and finally the circumpolar late Champlain transgression. It seems almost as if in certain Mesozoic phases the transgressions in undisturbed regions were complementary to those in lower latitudes. The upper Cretaceous inundation is traceable to Scotland, attains Scania and Moscow, but is not known farther in the north. Angara land (eastern Siberia) and China have as yet not given a sign of this transgression. You know by far better than I that North America shows the contrary. The upper Cretaceous transgression clearly extends to Yesso and Sakhalin, in northern Alaska along the arctic coasts about Colville River to the delta of the Mackenzie and thence southward into the United States. The undisturbed arctic and subarctic coasts therefore show quite a different marine series in Eurasia and in America.

b. The wide inundation of so many continents and the succeeding probably rather rapid retreat of the marine waters also dissipated the land waters, resulting in the destruction of the large dinosaurs, the inhabitants of swamps, rivers and lowlands, and retaining only those types of Reptilia which exist unto the present day. The freshwater fauna was driven into the upper reaches of the rivers. The Pyrgulifera from Bear River have a very remarkable affinity with the forms from the upper Cretaceous beds of the Gosau of Hungary and similar forms from southern France. The Pyrgulifera living in Tanganyika lake resemble them so much that I am willing to believe that this African freshwater fauna is not a relict of the Cretaceous transgression which seems not to have attained to that part of Africa, but the descendants of the habitants of higher parts of an upper Cretaceous river from the time of the transgression, exactly as Baikal preserves some species of Levantine age.

I will dwell a little longer on the most important question of fluviatile faunæ about which more is said in the last chapter of my book. The development of lungs preceded by gills teaches

that life has proceeded from the ocean to fresh water and land. In other cases of animal life no considerable change is visible; examples are the Medusa of Tanganyika, Victoria and the lower Niger. In a like way the marine pelecypods Mysidæ in the upper Volga, now separated by twelve degrees of latitude from the Caspian, are probably older than the separation of the Caspian from the Mediterranean. Another example is the sirenian *Phoca baikalensis.* In Pyrgulifera a Cretaceous freshwater gastropod has been preserved and I regard this as a relict from the head of a Cretaceous river, because the marine Cretaceous trangression and indeed every later marine inundation seems in the center of Africa not to have extended far beyond the southern limit of Sahara. In this way only can we understand that Nile, Niger, Gambia, Senegal, Kongo, Zambesi and lake Tchad (Boulanger's Megapontic sub-region) possess a very uniform fluviatile fauna. Further, the oldest types of fluviatile fishes exist in the oldest continents, Amia and Lepidosteus in Laurentia, Lepidosiren in Brazil, Polypterus and Protopterus in Africa, Ceratodus in Australia.

3. Difficulties in finding a satisfactory hypothesis as to the causes of transgressions.

When I wrote of eustatic movements in 1883 ["Face of the Earth," vol. I] I confessed that I did not understand the transgressions. I thought that variations in rotation might somehow have influence. I also believed and still think that the accumulation of sediment was a *vera causa*, but hardly sufficient. Now, after twenty-seven years I cannot offer you more than a loose heap of doubts regarding the explanation. I have learnt more and know less about it.

Regarding rotation, we must ask: Where was the pole? and has it always been fixed? Many years ago Oswald Heer said that its position was variable, as plants of a warmer climate are known from the Devonian or lower Carboniferous through the whole succeeding stratigraphic series and that signs of refrigeration begin to appear for the first time in the middle Tertiary. Now, the Jurassic ferns from the Antarctic teach a similar lesson and all these plants demand not only a warmer climate but more light than the polar nights afford. Further, the repeated glacial periods in different latitudes seem to hint of great displacements of the poles; several theories have been proposed but none is adequate.

It is quite true, as you remark, that the sinking of part of an ocean's base or part of a continent must increase the rapidity of rotation. The question remains, however, whether the phenomenon is not accompanied by a displacement of the planetary center of gravity.

All measurements of the polar applanation of the globe are executed on the base of the actual strand-line. The result of these measurements therefore, does not represent the applanation of the lithosphere but of the hydrosphere, and the high terraces or strand-lines in high latitudes prove the variability of the hydrosphere's shape. It is very improbable that the quantity of water has greatly increased, and this increase was probably not more than the volume of juvenile waters issued by volcanoes.

The terraces of the north are very distinct, as well as those of a great number of islands in low latitudes of the Pacific; but I cannot with certainty see whether these two sets of phenomena are synchronous and continuous or complementary. I believe in the formation of negative eustatic strand-lines through the sinking of ocean bottoms; therefore I suppose them to be synchronous. Rotation would give complementary lines (better complementary phases, as plus in polar regions and minus at equator), but synchronous negative lines might interrupt them.

What I wrote in 1883 about the considerable attraction of the continents and islands on the adjacent waters was then regarded as fully ascertained by our first authorities. Later on doubts arose and the question seems not yet fully settled.

Nature is parsimonious on occasions in allowing us to follow the actual facts in arctic, antarctic and in tropical regions. What we know is principally the northern temperate zone. In Mesozoic times the American and the Euro-Asiatic-Arctic transgression seem to be different. Of real peri-arctic transgression, that is, actual heaping of water about the north pole, the last inundation (Champlain) offers most proof and still holds as the best evidence for a rotatory hypothesis. On the other hand, the sharpness of all negative lines speaks decidedly against their formation by rotatory phenomena. Therefore I accepted the formation of the elevated strand-lines as due to the making of new depths, and left the cause of transgressions in doubt. Even now I cannot go farther.

I must close. Writing to a fellow geologist from whom I have learnt so much is such a treat to me that I must beg you to forgive the great length of this letter. What I offer you is little more than a number of questions; but questions are the buds on the tree of knowledge.