ART. XVII.—*The Lœss of the Mississippi Valley, and the Æolian Hypothesis;* by E. W. HILGARD, University of California.

[Read before the National Academy of Sciences, April 16, 1879.]

IN a highly interesting paper read before this body a year ago, and published in the February number of this Journal, Prof. Pumpelly, speaking of the lœss formation, sums up his views on the subject of the origin of these peculiar deposits in the statement that, rejecting his own previously expressed explanation, he is led to "believe with Richthofen that the true lœss, *wherever it occurs*, is a subaërial deposit, formed in a dry central region, and that it owes its structure to the formative influence of a steppe vegetation."

It is not my intention, at this time, to discuss exhaustively the question of the origin of the lœss in general, but rather to formulate some of the more prominent objections lying against the application of the æolian hypothesis to some of the lœss regions with which a long study has made me familiar. The expression "true lœss" leaves some doubt as to how Pumpelly limits the meaning of the word; but assuming that (as seems logically necessary) he uses it in the lithological as well as in its systematic sense, I desire to call attention to some points in connection with the lœss of the Mississippi Valley, which seem to render the æolian hypothesis untenable so far as regards that, and similar deposits elsewhere, which are clearly related to the troughs of large river courses.

The latter fact scarcely needs more than a cursory reference, if only to the circumstance that western geologists have habitually brought the lœss under the general designation of "bluff formation." The lœss of the Rhine, and of the Danube, are familiar to us in the same connection. It can hardly be that Pumpelly is not willing to consider these prototypes as "true lœss."

Now, what are the points in which the typical lœss differs so far from other aqueous deposits, that in spite of this obvious correlation we hesitate to class it as such? Aside from Richthofen's objections based upon the hypsometrical relations of the Chinese deposit (concerning the cogency of which I am unable to judge, not having seen his original publication), there are two principal ones, viz:

1. Absence of stratification.
2. Absence of fossils of aqueous origin.

As to the absence of stratification, it is admitted on all hands that even the most typical lœss, everywhere, often shows "bedding planes;" which manifest themselves more or less by a tendency to terraces, or lines of more rapid erosion on the otherwise vertical walls. This occurs more rarely in the central regions of the lœss masses; but on the peripheric ones it is not only quite frequent, but amounts in some cases to the most unmistakable appearance of aqueous stratification. Such is the case in the lœss bluffs of the Ohio in Southern Indiana, where my attention was called to it by Dr. David Dale Owen. It occurs also, though not quite so strikingly defined, along the edge of the "American Bottom" in Illinois, opposite and above St. Louis. Generally speaking, indications of stratification in the lœss are more frequent as we advance from the axial and lower regions of a river valley toward the sides and heads.

In the Sixth Annual Report of the Geological Survey of Minnesota, Prof. N. H. Winchell pointedly refers to the obvious transition of the lœss deposits into those of the newer Glacial period. Similarly, as stated in my Mississippi report (pp. 195, 298) and in a memoir on the Geology of Lower Louisiana (Smithson. Contr., No. 248, pp. 4 and 5), the lœss of the Lower Mississippi passes laterally, and by imperceptible degrees, into a clayey loam undistinguishable, in its final landward form, from that which forms the general subsoil of the Southwestern States; and almost precisely the same change occurs to the southward, in Louisiana, the representative deposits appearing, in that case, quite distinctly stratified into beds of materials more or less differentiated from the typical lœss.

Now, if the lœss of the Mississippi trough is of aqueous origin at the heads, the sides, and the lower end of that trough, and is, moreover, covered, normally, by a deposit whose pebbles and sand streaks can leave no possible doubt of a general shallow submergence immediately following, and terminating, the lœss period; it is not easy to see how the central portion of the deposit can have been in a subaërial condition, as required by Richthofen's hypothesis.

Is, then, the deposit of the Mississippi trough not a "true lœss?"—I have compared it carefully, in every respect, with the descriptions given of the characteristics of the lœss elsewhere, geologically, palæontologically, structurally and chemically; and I despair of finding any material differences, or such as may not be found, as variations, in any lœss district. It is true that the drainage of the Mississippi "cane hills" has not, as a rule, cut cañons with vertical walls, but narrow V-shaped

valleys between sharp-backed ridges.* But wherever vertical cuts have been made, they stand like stone walls, unaffected by the weather; and even the subterranean villages of the Chinese lœss had their counterpart at the siege of Vicksburg. Where the deposit abuts against the older formations, we have a great abundance of land snails, as well as pebbles derived from the older drift, which usually appear in "strings" sloping away from the land. The "lœss-puppets," too, are there, varying in size, as the results of a mechanical analysis show, from the tenth of a millimeter to that of lumps and spindle-shaped masses weighing as much as ten pounds. The intimate vertical tubular structure I have not had occasion to verify; but long tubular concretions, apparently the casts of rootlets, occur frequently, not only in the older portions, but as a modern formation around decaying rootlets; which cannot be otherwise, since all the streams of the region are depositing calcareous tufa.

But if it must be admitted that the lœss of the Lower Mississippi is a true, typical lœss, exhibiting all the lithological and structural characteristics by which that deposit is recognized elsewhere; and that hypsometrical and stratigraphical data compel us to assume that it has here been formed under water: then the mainstay of the æolian hypothesis falls at once, for what has happened here can have happened elsewhere. Nor should it be forgotten that, if the lœss does not exhibit the usual features by which we are accustomed to recognize aque ous deposition, it is, on the other hand, equally devoid of the evidences of wind-drift structure, which ought to be very obvious at least in the windward portion of the lœss regions. On the contrary, it seems that all observers testify to its dead uniformity over the entire areas; so that with the exception of such differences as would be expected to occur from the rolling down of debris, and the local action of streams, there is no constant mechanical or chemical difference between the windward and the leeward regions. At least no such differences are reported in the United States; nor are they mentioned in the *resumés* of Richthofen's views that have been published.

As to the absence of almost all but terrestrial fossils, save locally where the material generally is more clayey, I cannot help suspecting some connection between this fact and the solution and re-deposition of carbonate of lime, so constantly and rapidly going on in these deposits. The adherents of the æolian hypothesis find no difficulty in accounting for the absence of every vestige of the vegetation which they consider as a more or less essential agent of its formation. According to them, this vegetation has left no mark but the tubes originally coat-

* See Rep. on the Geology and Agriculture of Mississippi, 1860, pp. 194, 313, ff.

ing the rootlets. Now it is not easy to see, how under such circumstances any shell consisting of calcic carbonate can remain undissolved. I here recall to mind my observations on the deposits of the later (Grand Gulf) Tertiary of the Southwestern States; where in a deposit evidently formed on the shores of the Gulf, consisting of fine-grained sandstones, claystones, and in some cases silts scarcely distinguishable from that of the lœss period, we have such an absolute dearth of fossils that my most elaborate search in hundreds of localities, over an area nearly a hundred miles in width by two hundred and fifty in length, covered with rocks admirably adapted to the preservation of fossils and of an aggregate thickness of about 150 feet, has nevertheless failed to give any more definite clue to its fauna and flora than a few unrecognizable leaves, and a barely identifiable fragment of a turtle shell. The latter, however, was found in a stratum of clay containing an abundance of calcic carbonate in the shape of veins and irregular concretions, which in one locality at least showed plainly the general outlines of molluscan shells. The same features are repeated in the overlying strata of the "Port Hudson Group," which immediately underlie the lœss and are intimately connected with it. Here also there is an inexplicable scarcity of fossils; but calcareous concretions prevail everywhere, and fortunately, in some of the more clayey portions the maceration has only partially destroyed the forms of the shells; so that after tracing a stratum filled with concretions for miles, one will occasionally see these nodules taking shape, and finally, for a few feet, exhibit a very copious fauna, which, farther on, is again represented only by calcareous concretions.*

Cases in point are, of course, far from rare, and have probably come under the observation of most geologists in the field; but I think they have too often remained without mention, in the usual eagerness to find and describe well-characterized fossils. In my view, the wonder in the case of the lœss is, not that there should be so few vestiges of animal and plant life, but rather that any such should have escaped disintegration, under the oxidizing and dissolving processes that have been going on so long in so porous a material.

But why the exceptions in favor of the terrestrial animals? It seems to me that their localities of occurrence give a possible clue to the distinction. The destructive processes are essentially dependent upon the presence and percolation of water; and this should be least in the marginal portion, where as a matter of fact most of the terrestrial fossils are found. Whether in addition, there is a difference as to destructibility

* See my Memoir on the Geology of Lower Louisiana, and the Rock-salt Deposit of Petite Anse; Smithson. Contr., No. 248, 1872.

of land- as compared with fresh-water shells, may be a question deserving investigation. That the phosphatic bones should not have dissolved as easily as the mere carbonate shells, is readily intelligible; and as regards their mode of occurrence, I remark that in the lœss of the Lower Mississippi they are always very much scattered, many bones belonging to the same individual being rarely found together, but seeming to have drifted widely apart. It is not easy to see how the cumbrous bones of the Mammoth could have been widely separated in a subaërial deposit.

But I think that apart from its geological and other relations, there is intrinsic evidence in the nature of the material, contradictory of its æolian origin. In a paper lately published, I have drawn attention to a general distinctive feature of fine detrital aqueous deposits, viz: the necessary state of "flocculation" in which they are deposited, so long as the water is not absolutely quiescent. Excepting only under conditions of such moisture as would preclude the possibility of conceiving the wind as an adequate cause, dust deposits cannot be in a flocculated condition, but in the very nature of the case must consist of single grains closely packed. It is true that this axiom does not seem to accord with our every-day experience; for the dust of our roads, as well as that which we take off our furniture once a week, lies very loosely and is evidently not of a "single-grain" structure. But the organic "fluff," and hygroscopic and glutinous impurities causing this looseness of texture, could form no important part of the material carried by the supposed secular wind-storms of the lœss era. Now as the lœss is naturally one of the most porous and pervious of deposits, so as hardly to require tillage; and since moreover, it is shown by mechanical analysis to consist largely of minute spheroidal concretions—in other words, of floccules permanently fixed by the calcareous incrustation, precisely as should be the case if it *were* an aqueous deposit; while if a wind-deposit we should expect it to be cemented bodily into a continuous, rock-like mass: I submit that this structural peculiarity renders the aqueous origin of the lœss extremely probable. It may be possible to corroborate this argument, however, by direct experiment, in cases where the formation of calcareous concretions has not so far fixed the floccules as to render the production of slaty cleavage by pressure impracticable; and on the other hand, I intend to ascertain by direct trial, in what manner the lœss material will be deposited by an artificial wind, after freeing it from the calcareous cement by digestion in weak acid; and also, what will be the effect of pressure upon the material so treated, in the tamped condition on the one hand, and in the flocculated on the other.

Again, I am unable to see how the æolian hypothesis can account for the chemical peculiarities of the lœss. It seems to me to be very far from having the composition of the average rock dust of the earth's surface; nor does it present the chemical condition in which we would expect to find such dust. What has become of the feldspars, or of their products of decomposition? Admit that their dust would, by this time, all have been kaolinized, how can an average of two and a half or three per cent of alumina, shown by analysis, account for it? The previous decomposition products of the rocks of the great Western Plateau, as shown us by the Tertiary deposits, tell a very different tale, in the large prevalence of clayey strata; and it is from these very strata, moreover, that the largest part of the lœss material must, in one way or another, have been derived. It is easy to understand that the clay could be carried to the Gulf in suspension in *water;* but where did it finally lodge on the æolian hypothesis? If it be objected that, as stated by Prof. Brewer, "some of the lœss material is so fine as to remain in suspension for a year," I reply that although it will not stay so for one minute where any considerable amount of lime or other neutral salt is present in solution; yet, should the water be alkaline, the clay will remain suspended indefinitely. As there is a superabundance of sources of carbonated alkalies in the region in question, no objection can be raised to the aqueous origin of the lœss, on the score of its containing too much lime to permit the clay to escape to seaward. But I do consider that large average percentage of calcic carbonate, which so uniformly characterizes the lœss everywhere, as another serious objection to its æolian origin; it being out of all proportion to the alumina, not only on the score of general averages, but also on that of the slow, and almost exclusively aqueous mode of disintegration of limestone proper. If on the other hand, the lime was chiefly carried in solution, and from this assimilated by testacea whose shells were afterwards mostly destroyed by maceration, its presence is readily accounted for.

I do not, of course, pretend that the considerations I have here presented should be accepted as conclusive against the æolian hypothesis. But it seems to me that the points mooted require a fuller consideration than they have heretofore, so far as I am aware, received at the hands of the advocates of that hypothesis; and that they suggest a line of investigation not adequately pursued as yet. We are not yet cognizant of the precise nature of the varying conditions under which thick aqueous deposits apparently devoid of stratification may be formed; as they are even in flowing water, and may be seen in the deposits of the Mississippi River after any great overflow; as also in the stratified drift of the Southern States, where

the "flow and plunge" structure is the rule. If it can be said (as I do *not* think it can) that no deposits similar to the loess are now in process of formation by lakes: neither have we at this time, any example of the accumulation of such deposits through the agency invoked by Richthofen, on any considerable scale; although the postulated conditions exist in not a few regions. All dunes and drifted desert sands show wind-drift structure, as a necessary consequence of the varying velocity of the wind; and it seems to me that even in the presence of the supposed steppe vegetation, a condition of things under which that structure should nowhere appear, or should have been destroyed afterwards, is much more difficult to imagine than that, under the anomalous conditions of the "Champlain" period of depression, such conditions of aqueous deposition as we now find only exceptionally, should have prevailed more generally and for a longer time; a time, however, immeasurably shorter than that to which we must stretch our imagination for the formation of a thousand feet of dust deposit, brought by a wind so uniform in its direction and velocity as to leave no trace of the proverbial variability of that agent. And when we find ourselves driven to the supposition that this extraordinary wind did, moreover, drop its uniformly fine dust into the trough of the Lower Mississippi, leaving all the adjoining upland without a vestige for hundreds of miles on either side: the sum-total of anomalous conditions required to sustain the æolian hypothesis partakes strongly of the marvellous.