Lake basin, and covered the Adirondack Mountains, terminate. This Loëss in the Mississippi valley teems (according to Sir C. Lyell, in *Antiquity of Man*, p. 237) with land-shells, and occasionally graduates near the rivers into a subjacent and more stratified deposit containing freshwater shells.

I have made these observations as brief as possible, both because my knowledge of the Loëss is limited in the way already mentioned, and because I desire to offer them as tentative only; but I incline very strongly to the belief, now less in fashion than it was, that the explanation of Geological phenomena is to be sought in causes still existing, rather than by resort either to causes wholly supposititious, or only now found to be in action to some very subordinate extent.

III.—THE LOESS—A REJOINDER.

By HENRY H. HOWORTH, F.S.A.

WHATEVER the result of this controversy, your readers will not be ungrateful to me for having been the means of drawing from such a distinguished explorer and geologist as Baron von Richthofen, the vigorous defence of his views which appears in the last Number of the *GEOLOGICAL MAGAZINE*. The admirable work he has done in China it would be an impertinence in me to praise; but I may be allowed to say that a large part of it will outlive any ephemeral discussions about the Loess—discussions in which there must be a considerable element of doubt, whatever theory is adopted. I cordially thank Baron von Richthofen for the kindly terms in which he refers to my writings on the history of the Mongols. Having done so, I would express a feeling, shared perhaps by others than myself, that, when discussing my views about the Loess, which were, I hope, stated in deferential language, it was hardly necessary to adopt expressions which are not quite judicial. The polemical vocabulary of Germany is proverbially somewhat tropical, and seems to some of us to be more adapted to religious and political strife, where feeling plays a large part, than to the dissection of cold scientific facts, where neither authority nor vigorous assertion avails much. Especially is this so in addressing a cultivated audience like that which your pages are likely to reach. If Baron von Richthofen's theory had been generally accepted in the scientific world, a little emphatic dogmatism might be overlooked; but as this is the very opposite of the fact, and the only difference between us is, that while he is a distinguished heretic, I am an obscure one, it seems to follow that the most seemly language for each party to use, especially for the one who has every claim to a wide reputation, is that of sobriety, if not of hesitation.

Turning from the manner to the matter of Baron Richthofen's paper, I confess to being greatly surprised. It is quite true that his work upon China is incomparable in its way, and will become a classic; but he is not the only writer who has dealt with the Chinese Loess after a long and familiar acquaintance with it. Father David, whose admirable papers in the *Nouvelles Archives du Museum*

d'Histoire Naturelle, are very widely known, has travelled over perhaps quite as extensive a tract in China, and has examined the problem of the Loess there with a special view to Baron Richthofen's theory, and has not only written about it, but has written very strongly, as Mr. Kingsmill has, against it; while if we turn to America, where the Loess deposits, the consideration of which I wished to postpone to a later paper, are so extensive, there has been a very wide discussion of the same problem, and with the exception, perhaps, of Mr. Clarence King and Professor Pumpelly, a general consensus of opinion completely adverse to Baron von Richthofen. If authority "*qua authority*" were of any avail in such a controversy, this would be a fair way of meeting the position he maintains; but for one heretic to quote authority against another is not very conclusive. Let us, therefore, brush aside these appeals on either side, and grapple more closely with the problem itself, which is admittedly a very difficult one.

With the first pages of Baron Richthofen's paper I have virtually no controversy. He merely condenses and states in clear tabulated form, what I urged in more diffuse, and perhaps not such clear language. About the twelve characteristic peculiarities of the Loess which he refers to there is no difference of opinion, save in regard to the first and the eleventh, to which I shall refer presently, and I mostly completely agree with the dictum that neither the sea, nor lakes, nor rivers, could deposit the Loess at altitudes of 8000 feet on hill-sides. This was the burden of three-fourths of my paper, which I reiterated more than once, and which seems to me to be incontrovertible.

Again, when the term subaerial is used by Baron Richthofen as covering his position, there must be no mistake about my own meaning. I have no doubt whatever that the Mammoth and his companions, that the land shells and the debris of vegetables found in the tufas, are as clear proofs that the conditions in which they lived were subaerial, as the same series of remains is in the case of the brick-earths and the loams. In all these cases the remains prove the existence of subaerial conditions, and not subaqueous ones, where the animals and plants lived. I believe, further—and the view is fully held both by French and American as well as by our own geologists—that the animal and vegetable contents of the Loess, as well as its stratigraphical position, prove that it belongs to precisely the same horizon geologically as the loams, brick-earths, and the so-called diluvium. All this I have never hesitated to urge. The ground I have taken is, that at the epoch when Palæolithic man lived, there was a tolerably uniform deposit over central and southern Europe, representing the old land surfaces on which the Mammoth and his companions passed their lives, represented as to texture, ingredients, and contents in its most unsophisticated form by our lower brick-earths and the diluvium gris of the French writers. The whole being of subaerial origin, this homogeneous deposit was presently sophisticated in two ways. When the catastrophe came—which I have adduced a very con-

siderable array of evidence to show closed the Mammoth period and formed that notable gap which is universally acknowledged by archæologists between Palæolithic man from Neolithic man, the loams were largely redistributed and rearranged, remaniés as the French call it, while at the same time there was an outpouring of a considerable amount of subterranean mud in certain districts, which was mixed with the subaerial surface deposits, and with the debris of animals and plants then living on the surface, and thus constituted the Loess. The Loess, therefore, in this view, is a subaerial deposit sophisticated and altered by a flood of calcareous mud or water charged with mud, and distributed as we find it by an impelling wave of water. It will be seen, therefore, that the question between us is not as to whether the deposit is subaqueous or subaerial, but whether the particular subaerial theory of Baron Richthofen and the peculiar atmospheric agencies appealed to by him are competent to explain the constitution of the Loess and its distribution; and if not, whether the subterranean origin of a large portion of it is possible, and whether its distribution by a flood of waters is not an hypothesis which more completely explains the facts.

Baron Richthofen complains that I did not quote directly from his book. That most valuable book, which I looked through some time ago with mingled admiration and envy, was certainly not by my side. I have not a copy of it, nor is there, unfortunately, a copy of it available for reference in these parts; but I appeal with every confidence to your readers, who may examine the Baron's restatement of his case, whether I either mis-stated or misunderstood it. It will be conceded by those who do so examine it that every point of any importance he now urges was perfectly present to me when I wrote, and was, as I think, fairly met. On the other hand, it seems very clear that Baron Richthofen has only read one paper out of a considerable series which I have contributed to your pages, all of them aiming at one conclusion, all of them supporting one thesis, and whose force, if they have any, depends on their cumulative character. Perhaps, if he does me the great favour of replying to this communication, he will first look over the papers which preceded and have succeeded that dealing with the Loess.

The first point to which I would call attention is that we are completely at issue about the kind of surroundings which the debris of the Loess fauna show must have existed when that fauna was living. Baron Richthofen says, "The genera and mostly the species of mammals found in the Loess, or their next relatives, are known to abound at present in steppes and on grassy plains." Is this so? The Mammoth, it has been well said, would starve in a few days on the richest Craven pasture. The Elephant and his nearest relatives cannot browse upon the herbage of steppes or grassy plains. Its natural habitat is the forest, its natural food—the succulent branches of trees, and we actually know, as is most familiar to Baron Richthofen, that both the Mammoth and the *Rhinoceros tichorhinus* did live upon the softer portions of trees, for remains of their food have been preserved and examined. These are the characteristic

quadrupeds of the Loess, and with them occur other forest animals. If we turn from the mammals to the molluscs in the Loess, the very large proportion of which are land shells, the same conclusion is inevitable. Those conchologists who are best able to decide such a question agree that the *Helices* and other shells of the Loess lived in the recesses of damp woods, and their abundance proves the conditions to have been singularly favourable to them, namely, those of a humid atmosphere and of deep shade.

I have quoted European authorities for this view whose experience is overwhelming. Let me add to them that of my correspondent, Prof. Todd. Speaking of the abundance of land and the paucity of aquatic shells in the American Loess, he says: "However, some semi-aquatic species, as *Succineas* and *Helicinas*, are very abundant from top to bottom of the formation, and the decidedly aquatic *Limnea humilis* is quite abundant in the Upper Loess of Western Iowa. This is sufficient to show that the formation, so far as this region is concerned, could not have been so dry as is called for by Richthofen's theory. Even the land shells observed need a much moister region than our upland prairies in their present conditions. Some of them are not found at all, away from moist banks or groves." Again, "I think no one can study the life of the Missouri Loess without seeing that it bears, throughout, evidence of a moist climate, the very opposite of the Baron's ideal."—(Richthofen's Theory of the Loess, by J. E. Todd, p. 6.)

The evidence of the mammals and shells is supported also by the debris of vegetation in the tufas, which prove the former existence there of luxuriant forests. All this is entirely at issue with the Baron's postulate, and in fact cuts the ground from beneath it. I referred to it in my paper, but he does not notice it in his reply.

The existence of thick moist woods is assuredly inconsistent with the dry winds and dry grass pastures which he requires for his theory, inconsistent also with the prevalence on any scale of those dust-storms which he postulates, and perhaps the best proof of this is that while the Loess is such an exceedingly fertile soil, in North China, where local dust-storms prevail largely, nothing is more remarkable than the virtual absence of trees over many degrees of latitude—a circumstance which the Père David has made much comment upon.

We have not yet done with the fossil contents of the Loesses. In certain places, as we have urged over and over again, the remains of the animals are found intact, showing that they could not have been transported very far. In other cases they are found buried in hecatombs. In others again, and most frequently, the bones are found sporadically and much scattered. In all cases, as we have tried at very considerable length to show, they present evidence of having been largely disturbed and transported. How could such agencies as the Baron invokes explain the presence of great hecatombs of animals buried pell-mell together, or the presence in some cases of whole skeletons of Mammoths and Rhinoceros, or explain in others the dispersal of their bones. All these factors are inconsistent with continuous gentle atmospheric agencies only and with

the accumulations of vast depths of Loess through which the bones are distributed irregularly, and I must repeat that I cannot conceive how the wind could transport them or the dust bury them intact, unless the meteorological conditions were entirely different from those now in existence, and quite transcendental.

Let us now turn to the structure of the Loess. Baron Richthofen accepts the theory which makes the ramifying calcareous tubes found in the Loess to be the root impressions of grasses, and bases a good deal of deduction upon it. The theory that they are so, and are not due to calcareous filtration, is at present, however, very much in need of evidence, and, to my mind, has utterly collapsed. I would add to what I said on this subject in a previous paper, that if these tubes were mere casts of roots, we ought assuredly, in regard to a *forest* flora, to find any number of casts of *tree* roots, and roots of *herbaceous* plants, which are so easily discriminated; but we find none of these—only the ramifying fine tubes which have been called grass roots, but which, to my mind, have nothing whatever to do with grass roots, but are due to the percolation of water charged with calcareous matter. This is further supported by the fact that they do not occur where the Loess has a dense hard structure—only where it is porous; that they occur chiefly near the surface, and diminish in size as we go down, which is the exact result we should expect from percolating threads of water acting on a deposit highly charged with carbonate of lime, and inconsistent with a deposit gradually growing higher, while the plants, whose roots the tubes are supposed to be, were continuously being buried by additions of wind-borne dust. In regard to the features just named, Mr. Todd (who, by the way, does not question their being root-marks) says: "They may be said to vary inversely as their distance below the surface. Near the surface, besides being most abundant, larger ones are found. At the depth of 30 or 40 feet, they are very minute and rare. None have been found lower than about 45 feet, although several favourable localities have been examined" (Todd, *op. cit.* p. 8).

The concretions that occur in the Loess add very considerably to the weight of evidence which makes these tubes to be the results merely of percolating water, for these concretions are clearly the result of the same agency acting slightly differently. In regard to these concretions, I shall be pardoned for quoting a graphic description by my correspondent, Prof. Ellsworth Call. He says: "They assume all possible shapes from the spherical through the spheroidal to the oblong; in all cases they are more or less numerous studded with roughened projections. No one shape seems to obtain more than another, and not unfrequently several are found cemented together, forming an eccentric single mass. They are certainly characteristic of the Loess, for that formation nowhere occurs without their presence. *They are decidedly hydraulic, as would be naturally inferred from their constitution.* In no case have I ever observed fossils—either mollusks or vegetable matters—acting as a nucleus. On one occasion 2803 of these bodies were crushed with that especial point in view. In nearly every instance, 2789, they were found to contain

loose fragments broken by some means from their inner walls, but no foreign substance whatever could be detected. In the remaining 14 specimens, while the concretions were hollow, they yet contained loose particles of no substance whatever. Not a single specimen was solid throughout. *That they were originally solid, or of a pasty consistency, is not to be doubted, as a study of the inner surface reveals. They all present a deeply fissured interior consequent on the evaporation of water and subsequent contraction.* In the vast majority of cases the pyramidal masses of the interior showed distinct irregularly concentric lines of growth, or rather of accretion. . . . Professor J. D. Whitney says of them that they '*have been formed in the Loess by infiltration along the lines of cleavage and resultant chemical action on calcareous matter occurring in large quantity along certain planes*'" The Loess of North America (American Naturalist, May, 1882, pp. 373 and 374). To return to the tubes. I see no evidence whatever in them to support the wind theory of Professor Richthofen, nor do I see any either in the peculiar quality of the Loess, by which it cleaves in perpendicular faces, which is no doubt due to the presence of calcareous matter in excess, and to the presence of these very tubes. Wherever we can trace current and unmistakable wind formations, such as dunes, etc., etc., we have no such cleavage properties, and I cannot see how Baron Richthofen proposes to connect them with his predicate.

Again, the Loess for the most part is completely unstratified. Occasionally, especially in America, there are local areas 'where a kind of stratification occurs, but these are very local, and I shall return to them presently. This absence of stratification I quoted myself as a proof that the Loess is neither of marine, lacustrine, nor fluviatile origin. It is assuredly equally a proof that it is not due to gradual accumulation by the wind. Dunes accumulated by the wind are so easy to study that we have no difficulty in finding materials, and assuredly they present quite a different structure to Loess. Deposits made by wind, especially when made as Baron Richthofen suggests, in dry seasons alternating with wet ones, have a laminar structure corresponding to the series of layers deposited, just as deposits made by water have. Nor should we find homogeneous masses several hundred feet thick with the same structure and the same contents as the results of such a series of seasonal deposits. These masses, to my mind, bear, on the contrary, unmistakable evidence in their very structure of having been deposited by one great effort, and under one set of conditions. Again, wind acting upon dust or sand, deposits it with a very well-marked contour in dunes and sand hills, especially when the sand is arrested by grasses (*a fortiori* by forests of trees), and forms great masses of rolling sand hills on the weather-side of any area subject to sand drifts. Where are such phenomena to be traced in the structure of the Loess deposits? No doubt, in certain localities where the wind has recently and is even now disturbing the surface layers of Loess, we get such wind structure; but this is purely local, and to be explained as just mentioned.

Professor Aughey speaks of this wind-structure in some of the Loess hills on the Logan, Elkhorn, Loup, and Republican rivers. He says, "This structure is often found there as distinct as among the shifting sands of our sea-coast. In every case, however, where I examined this structure in the Loess, I found it to be superficial. Out of nineteen such hills none of them possessed this structure over ten feet deep, and few of them over five feet, and many of them only from two to three feet deep. In the deep cañons, where the Loess is exposed vertically from one hundred feet, I have never found this wind-structure over ten feet deep. It occurs, therefore, only in the Loess that has been recently modified by winds, and long after it was first deposited" (*Sketches of the Physical Geography, etc., of Nebraska*, p. 274). It would be difficult to find a more striking proof that the Loess was not originally distributed by the wind than the fact that it so readily assumes a wind-structure, and yet that this peculiarity is only traceable in the superficial layers where the current winds have acted upon it. Again, wind driving dust and sand in a definite direction, as, for example, from the Mongolian steppes towards China, would, in crossing such a broken district as the long tract of mountains bordering the valley of the Yellow River on the north, leave unmistakable traces of its passage. It would strip the high ground completely and choke up the valleys, especially those parts of the valleys under the lee of the mountains; but nothing of this sort, if we are to follow the careful observations of Père David and others, occurs; but the Loess is generally deposited in a mantle, as one American writer says like a blanket, washed evenly over the surface, and not piled up in drifts in those places where the force of the wind could not move it. Again, such a wind would assuredly sift the materials, dropping the heavier ones first, and carrying the lighter ones further away; but this is not what we find, the texture is the same throughout, and throughout, heavy grains of quartz and particles of mica occur confusedly among the finer siliceous dust, as Baron Richthofen himself says. Again, the process described by the Baron would surely rub down the particles of dust by trituration against one another, and make them rounded and weathered, whereas, as he has remarked in China, and other observers in America, a remarkable feature of the Loess is the sharp-edged and angular particles out of which it is formed.

Baron Richthofen explains the Loess as composed partially of dust brought in by winds and partially as the result of the decay of grasses and other vegetable matter. The proof of the existence of these grasses he finds in the calcareous tubes we have mentioned. But apart from the reasons we have urged against the capillary tubes being treated as casts of roots, upon which a good deal of the reasoning in regard to the vegetable debris in the Loess depends, we must remember that the various analyses that have been made, especially of the American Loess, show that there is hardly a trace of carbonaceous matter in it—assuredly a very strong proof that the amount of material due to decayed vegetation in the Loess is hardly appreciable. This is true also of the black earth of Russia, and is in very marked

contrast with the beds of real humus intercalated with the Loess in certain places (see Aughey, *op. cit.* p. 176).

No doubt in Mongolia we have a high plateau covered with thin grasses, and no doubt thin grasses will arrest moving dust. This is a mere parallel to the office performed by the various siliceous grasses in forming the dunes and sand hills of Holland; but the sand itself in Holland comes ready made, and, as I understand Baron Richthofen, the surface of the Gobi is formed of Loess ready made, the grass merely arrests it in certain places when it is moving, it does not help to make it. What we want to know is, how and where it was or is being made either formerly or now. Let us now turn to the European deposits of Loess. Whence did the wind bring them? They were clearly not made *in situ* by the wind acting on the drift deposits and gravels underlying the Loess. Whence then did they come?

If we turn east, west, north, or south of the great area of European Loess, whence are we to derive the dust which formed its basis according to Baron Richthofen? Wind blowing over grass pastures takes no dust—it must be over bare ground; but where are we to find such anywhere? Again, if all the grass of the Russian or French plains was taken off, Baron Richthofen will not argue that wind blowing over the ordinary loam known as diluvium or over the chernozom would carry off dust which, when deposited, would be Loess. All this surely demands some answer, as does the cardinal problem, that the Loess is a limited deposit in Europe with sharp boundaries, especially towards the north. I can nowhere find an answer to this critical question in Baron Richthofen's paper. Suppose we could find such a source for the dust, still our difficulties would not be ended; for the problem, as solved by Baron Richthofen, requires that we should have quite exceptional conditions besides. Take, for example, the demands upon our credulity involved in the following sentence in his own paper: "The Loess-covered portions of Europe extend, as is well known, from the Pyrenees, the Alps, and the Balkan in the south, to Belgium, the North German plains and Poland in the north, and from southern France in the west to beyond the limits of the continent in the east. Every portion of this entire region must have had the character of a steppe during a sufficient length of time to allow the deposit to be formed in at least such thickness as we observe at present" (*op. cit.* 301). Where have we any evidence to support such an extraordinary postulate? Where is the evidence that this large portion of Europe was a dry grassy steppe when the Helices and other damp-loving molluscs were living here? These and the debris of plants in the tufas are very much better tests of the climatic conditions than hypothetical appeals to a continental climate induced by Europe being prolonged as far as the hundred fathom line, as if the presence of sea shells mingled with those of the land, and with Mammoth remains, at the mouth of the Somme, and at several points on our own coasts, do not prove such a postulate to be out of the question at this particular epoch. In Europe, therefore, so far as we know, there is no area

whence the dust could come, and there is abundant evidence that the dry steppe-climate of Baron Richthofen is virtually out of the regions of possibility. But he makes still greater demands on our faith when, not content with speaking of a steppe climate, he speaks of such steppes as if they were the equivalents of or had any analogy with the Siberian tundras, and treats the two as if they were the same thing. The tundras are as different to steppes as anything can be; they are covered with thick moss, and can neither be denuded by winds nor have their substance increased by them. They are essentially exceedingly humid, and quite different to the dry areas he otherwise speaks of. Surely we require some explanations of these extraordinary statements. Here let me say parenthetically, that Baron Richthofen cannot be serious in urging that the proof of the identity of the Pleistocene fauna of Europe, including that of the Loess, with that buried under the tundras, was reserved for Dr. Nehring. Has he forgotten the name of Cuvier, to select only a very big name from a large crowd, who proved this elementary position long before Dr. Nehring was born? Nor assuredly can he be serious in supposing that when the *Fig*, the *Cyrena fluminalis*, and the Hippopotamus lived in Europe, the climate here was like that now current on the lower Obi and Yenissei. If he really urge this, in view of all the facts and the matured opinions of the Russian naturalists which I have brought together in the earlier papers of this series, then I have nothing more to say.

Baron Richthofen postulates two climatic conditions as having succeeded one another in regions where Loess prevails, one marked by extreme dryness, the other by great damp. How is this shown, when both the fauna and flora point to a damp climate having existed at the time when Baron Richthofen demands a very dry one? But apart from this, we are reasonable in asking for some foundation upon which to rest such meteorological revolutions in such areas as China, the valley of the Mississippi, and Central Europe. It is easy to postulate a humid climate following a continental one, in which the conditions were almost the reverse of each other, so long as we limit ourselves to possibilities of *thought*; but we, who are morbidly anxious for some reasonable explanation of our difficulties, must have something more than transcendental predicates. We must have inductive ones. I know that local dust storms in North China do prevail, as they prevail in Mongolia and in North America, where the Loess exists; but, as Professor Call says, the evidences of such action are purely local, and dust storms merely tend to rearrange the surface, denuding the windward, and covering more deeply the leeward, bases and sides of the hills; but these winds do not help us to explain either the origin of the Loess or its general distribution.

There is no evidence that the dust from the Mongolian steppes is now adding to the Loess which occurs in China, which is separated from Mongolia by the well-known chain of mountains that runs north of the Yellow River. But suppose it were, we should be no nearer solving our problem; for Baron Richthofen urges that the

Loess is not being stripped from Mongolia, but is growing there too, as it is in China. How, again, can we understand a patch of steppe, in the centre of a vast area of Loess, supplying all the country round with dust, while it is being stripped itself by all the winds of heaven? for we must remember that the Russians have shown that in the province of Irkutsk, north of the Gobi Desert, Loess occurs as it does in China. The problem of deriving the main body of the American Loess from the small area known as the American Desert would be equally great, even if it were proved that the ingredients of the Loess were ready there for these winds to act upon. Again, I must urge what seems to have escaped Baron Richthofen, that it is quite clear from every consideration that the Loess belongs to the same geological horizon as the diluvium of the French and Russian writers. The contents, animal and vegetable, and the primitive works of man which it contains, as well as its stratigraphical position, all prove this. Again, although the Loess differs from the loams and brick-earths in its texture, and in the abundance of carbonate which it contains, yet its mode of distribution, as we have shown, is essentially the same, both being spread over high ground and low, irrespective of the drainage, and being otherwise similarly distributed. Any theory that accounts for the distribution of the Loess must also therefore explain the idiosyncrasies of the loamy deposits. Assuredly the wind theory of Baron Richthofen would even by himself be deemed incompetent to explain the difficulties of the brick-earths and upland loams of Western Europe. Prof. Aughey urges an objection which is closely connected with this. He says very properly, that "a fact often overlooked is the transition character of some beds of sand as they shade into the Loess. As beds of Loess and stratified sands at the bottom of Loess sections often alternate, and even sometimes with strata of clay, it is not easily conceivable how subaqueous agency should have formed the one, and æolian agency the other" (*op. cit.* p. 280). Lastly, Baron Richthofen argues as if all the debris found in the Loess were sub-aerial. This is not so, however. In America, in China, and in Europe it is quite true that the land shells prevail largely over the water shells; but the latter are certainly found in appreciable numbers, especially in the Loess of Iowa, showing that considerable lakes or rivers must have existed, which is again proved by the traces of stratification in certain areas. I think the American geologists exaggerate the extent of these lakes from overlooking the fact that the Loess has been largely transported and rearranged, and thus its contents have been swept over a wide area remote from their original site; but it is nevertheless absolutely clear that evidence of such lakes exists, and such lakes in fact exist still.

In Clay, Fillmore, York, and other counties, says Professor Aughey, "there are considerable numbers of ponds, covering from a few acres to half a section of land, grown up around the border with reeds and coarse grasses and sedges, and, where the water is deeper, with arrow leaves, pond-lilies, and other water plants. In every instance where I had an opportunity to examine them, there

was a thin bed of clayey matter mixed with organic materials, from a few inches to a foot or more in thickness, lying on the bottom, and on the top of the Loess deposit. This clayey matter was probably deposited there before the waters finally retired from the old lake bed in which this soil originated" (*id.* pp. 270 and 271).

How is it possible to account for such sheets of water under the arid conditions absolutely required by Baron Richthofen's fierce winds and steppe climate? The fact is, whichever way we approach the problem, it seems to me that Baron Richthofen's theory not only fails to explain the facts, but is completely at issue with them. His great name may give the theory a certain ephemeral importance, but it will not bear the test of close criticism when we leave the realms of general hypotheses, and come down to the grim, awkward, tyrannical region of facts, and when we have turned it over in every way with one result, we confess to feeling that there is some temerity in its author speaking of the *exposé* he has given in your pages as refuting without any further discussion arguments which are not even touched.

Having said so much about the Baron's theory, I must now turn to the justification of my own, which he characterizes with scant courtesy as "views which could be pronounced at an early and rather low stage of geological science, but are long since abandoned," "suppositions which bear the character of the infancy of that science," and of which he says again that he does not believe that any geologist will seriously take the trouble to argue against. I must at once disabuse him of this view. If he had seen the letters on my table from those whose reputation is not less than his own, he would not have used these phrases, which are rather characteristic of the amenities of scientific controversy of a former day. I can assure Baron Richthofen that, whatever the value of the views, they have certainly secured the attention and received the approval of those whose judgment I respect, and that they will need to be met by some stronger arguments than those he has so far used.

When it seemed clear to me that the Loess could not be explained as a marine, lacustrine or fluvial deposit, and that Baron Richthofen's view was equally inadmissible, it was necessary to frame some new theory which would meet the facts. These were an explanation of the origin of the Loess, and secondly of its distribution. In regard to its origin, I suggested that it seemed to have great analogies with the Moya or volcanic mud that is thrown out in certain volcanic districts, and that its calcareous ingredients seemed to point to a subterranean origin. Baron Richthofen says dogmatically that the Loess differs completely in composition and structure from all known kinds of volcanic mud, ancient or modern. This is a strong assertion, and if it be indisputable, it goes some way to discount what I have said. Now it is very strange that a few weeks after my papers on the Loess were printed, I received a letter from Professor Aughey, of Nebraska, calling my attention to the important fact that in Nebraska, where the Loess is developed on the largest scale of any district in America, it is found in close

connection with the so-called geyser deposits of the American geologists. Speaking of the Pliocene beds of Nebraska, he says, "Near or in many of these beds all over the Pliocene region of the plains are found many extinct geyser tubes, and sometimes old geyser basins. Of these I observed at least thirty between Arapahoe and the west line of the State; I have also found them in the Loup region, and on the Niobrara. As some of these geyser tubes had their exit in the Fort Pierre group on the Upper Republican, it is probable that they commenced their work in the Cretaceous period, and were in operation all through the long centuries of the Eocene, Miocene and Pliocene periods, *and far into the Quaternary*; a similar bed exists on Oak Creek, which was deposited in interglacial times. Nebraska and Northern Kansas, in fact, was a great geyser region all through the Tertiary period. It far exceeded in the number and magnitude of its geysers the Upper Yellowstone region and Iceland at the present day. Few memorials of these old extinct geysers are visible at the present time, owing to their being covered up by the superincumbent Quaternary deposits, but enough remains to show that a prodigious number must have existed at least in Pliocene times" (Sketches of the Physical Geography, etc., of Nebraska, pp. 239 and 240). If we examine the analyses that are available of the Loess from different districts of the silico-alkaline earths that characterize the Nebraska Pliocene beds and of geyserite proper, we shall be constrained to assign them all three to the same ultimate cause. This evidence is surely very important, and it will, I trust, in some measure affect Baron Richthofen's emphatic statement which I have quoted. I showed in my paper that in the European area of the Loess, calcareous springs, fumes of carbonic acid, and other traces of still living volcanic action exist, while the proofs that they existed much more forcibly during the deposition of the Loess are very familiar, the intercalation of Loess and volcanic ashes in the Rhine volcanoes being the most forcible. This I have already shown at some length in a previous paper. In regard to the mountains of North China, which are the focus of the Loess deposits there, I might quote many additional facts from Père David's papers already cited, in which he has shown how in all directions these mountains are strewn with evidences of volcanic forces actively at work down to the most recent geological period, and apparently showing special signs of activity in the Quaternary period.

It will be remembered that in postulating a subterranean origin for certain ingredients of the Loess, I did so in a very hesitating way, and merely as a tentative hypothesis. Tentative hypothesis only it still remains, as does every opinion I hold upon every subject; to be surrendered immediately, and without regret, directly it is shown to be inconsistent with the facts. At present, however, so far as my judgment goes, it is the only hypothesis available, for the only process that I know by which siliceous mud consisting of very comminuted angular particles entirely free from structure, and from the presence of foraminifera, etc., charged with carbonates, and like the Loess in every respect, can be explained, is the theory I have

offered. Nay, more, as I have shown or tried to show, the only method by which deposits very like the Loess are at the present moment being made is by the very process I postulate, of which Baron Richthofen writes so lightly. So much for the origin of the Loess. In regard to its distribution I do not speak with the same hesitation. Here the evidence, as I have presented it to your readers, is so consistent, cumulative and overwhelming, that it will require a very great many *facts* on the other side to disturb it. The evidence that the Loess was spread over the country independently of the drainage—spread like a sheet over hill and dale by a mass of moving waters—is so great that it seems to me conclusive. I have tried to focus a portion of it, as well as the evidence of the correlated deposits and their contents, from many quarters. The proofs are at present ample, that the big mammals were actually drowned, that they perished not singly but in many cases in hecatombs, that they were immediately covered over and protected from the weather in some cases with their skeletons intact, in others with their bones scattered hither and thither, that the loamy deposits wherever we meet with them and however we sift them bear unmistakable traces of this great wave of waters, which is amply supported again by the ethnographic evidence and by human tradition. This and much more *ejusdem generis* I have partially tried to show in the papers I have printed in the *MAGAZINE*, and which are not yet concluded. The nature and extent of this diluvial movement, which Baron Richthofen produces as a scarecrow, I cannot discuss at present nor until a good deal more of my evidence is published. At present it will suffice to say that I believe it was very widespread in the Northern Hemisphere, and that one of its notable effects was the origin of the Loess by the mixing of the products of a subterranean flood of calcareous mud with the loams forming the land surface where the Mammoth lived in the areas now covered by Loess in Europe, China and America.

I hope Baron Richthofen will forgive any expressions which he may disapprove of in this paper. It is not to be expected that phrases wanting in courtesy can be used by a distinguished writer of one very much his inferior without some resentment. He would not respect me if I consented to such treatment. But I can assure him, if I may be guilty of the presumption, that no one from Cathay to Peru rates more highly than I do his reputation, nor values more his contributions to Eastern Geography and Geology, and no one awaits more impatiently the time when they may be published at a cost that will bring them within the reach of poor men—poor men living in Philistia—overloaded with books and babies, and only very moderately endowed by nature with those qualities that secure success in the struggle for existence.

I wish to add a postscript on another matter on which Baron Richthofen could perhaps help me; or perhaps my appeal might reach some other German geologist. In the fourth volume of Mid-dendorf's *Travels*, p. 1081, there is a very extraordinary account of the discovery of a Mammoth which, if reliable, is valuable. The notice has been partially translated in one of Professor Dawkins'

papers, and again in his *Early Man in Europe*, p. 106. This notice has always seemed to me to be most suspicious. It describes how a young German engineer in the Russian service called Benkendorf, being one day in a steam launch in the estuary of the Lena, came across a Mammoth which had been detached from the frozen banks of the river, and was careering about in the flood. The notice describes how the animal was secured by a rope, how it was examined, and the contents of its stomach observed, but that presently a sudden rush of water carried it away. The whole story is told in a vigorous style, and with a very suspicious resemblance to a romance concocted out of what was already known about the Mammoth, and in fact adds little of actual fact to our previous knowledge. I recently looked up the notice in Middendorf, who quotes the account as trustworthy, but I confess my suspicions were not allayed when I found whence he had obtained it. He quotes the story from a boy's book entitled "*Kosmos für die Jugend. Blicke in die Schöpfung der Welt und in die Kulturgeschichte der Menschheit vom anfang bis zur Gegenwart*," published at Nurnberg in 1862, and written by Philipp Körber, the author of many other works. This book I have not been able to consult, as, unfortunately, like many others, it is not in the British Museum Library. The account professes to be taken from the letter of the young Russian naval officer Benkendorf, who was born in the Island of Oesel in the Baltic, February 23rd, 1821, and joined a topographical expedition to Siberia, whence he wrote several letters to Körber describing his adventures, the one we are interested in being in the year 1846. It is very strange that if genuine no accounts of this discovery should have reached the ears of Baer or Brandt, Schmidt or Schrenck, who none of them mention it, and that it should be first heard of in a popular book for boys in 1862. It would be interesting if the story could be sifted by some one who could get access to Mr. Körber, or even to his book, and the information would be especially useful to students like myself.

IV.—SUPPLEMENT TO A CHAPTER IN THE HISTORY OF METEORITES.

By WALTER FLIGHT, D.Sc., F.G.S.

(Continued from page 316.)

Found 1879, July 19.—Lick Creek, Davison Co.¹

In this paper is given an engraving, actual size, and a short account of a small metallic mass, weighing rather more than two pounds, and found at the above date in Davison county. When found it was covered with a thick scaly crust of oxide. It weighs 1.24 kilogrammes or $23\frac{3}{4}$ ounces avoirdupois. It is one of the rare class that do not show the Widmanstätten figures. It contains iron, nickel, cobalt, and phosphorus. A complete analysis of the meteorite is being prepared. It is the property of Professor W. E. Hidden, of the New York Academy of Sciences. Mr. Hidden has in his cabinet three other undescribed meteorites from the

¹ *Illustrated Scientific News*, New York, March 15, 1880, iii. No. 6, pp. 62 and 66. *Amer. Journ. Sc.* xx, 1880, 324.