**Reply to some observations in Dr. Fleming's 'Remarks on the distribution of British animals' / [William Buckland].**

### Contributors

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# REPLY

TO

# SOME OBSERVATIONS

IN

## DR FLEMING'S REMARKS ON THE

# DISTRIBUTION OF BRITISH ANIMALS.

By Professor BUCKLAND.

*From the Edinburgh Philosophical Journal.*



ALLOW me, through the medium of your Journal, to express my obligations to Dr Fleming, for the handsome manner in which he has spoken of my *Reliquiæ Diluvianæ* in your last Number; and for the mild and gentlemanly tone he has maintained, whilst expressing his opinions on certain points whereon he differs from me.

I perfectly coincide with that eminent naturalist, as to the expediency and the necessity of illustrating the history of the Fossil World, by the analogies afforded by the structure and habits of living plants and animals, and the operations of nature now passing before us; but I see not how the charge of neglecting all these things can, with propriety, be advanced by him, against the present cultivators of the science of geology, whose foundation-stone (as far as relates to the history of fossil animals) is laid by Cuvier on the most accurate analysis of the structure of recent skeletons, from which he argues most rigidly, as to that of the fossil species*; nor

* J'ai donc dû me préparer à ces recherches, par des recherches bien plus longues sur les animaux existans; une revue presque générale de la creation actuelle pou-

am I aware, that the imputation of ignorance of modern botany, can be fairly laid to such illustrious names as those of Sternberg, Schlotheim, Brongniart and Rhöde, from whose labours the subject of fossil botany is now receiving illustrations; and with respect to the history of the formation of peat-bogs, sand, and marl-beds, which Dr Fleming specifies as points which have hitherto been too much neglected by geologists, I need only appeal to the accurate and able observations of Professor Jameson and Dr Macculloch, —to the copious, and, on these subjects, most judicious pages of Deluc,—to the voluminous folios of the Irish Bog Reports,— and to the numerous papers that occur in the Philosophical Transactions,—to shew, that the history of peat, sand, and marl, which Dr Fleming states "to have been neglected as too recent for inquiry or speculation," has received its due share of attention from the most eminent writers that have yet occupied themselves with the study of the physical structure of the Earth.

With respect to the matters at issue between Dr Fleming and myself, as it appears to me that his objections arise chiefly from a mistaken or imperfect view of the facts on which his arguments are founded, I beg to submit to his consideration, and that of the readers of your Journal, the following points, on which I consider his ideas to be erroneous; forbearing to enter into the arguments he has derived from them, since, if the facts are misconceived, his conclusions will, of course, follow the fate of the premises from which they are deduced.

1. Dr Fleming objects, that the distinctions I have drawn between Post-diluvian and Diluvian deposits,—or, in other words, between local deposits, which can be referred to existing causes, and those more extensive collections of water-worn detritus, which have resulted from some single, and transient, and universal inundation of the surface of our planet,—are not sufficiently established. And,

2. He thinks the remains of animals that occur in what I consider the deposits of this inundation, may be referred to genera and species that have gradually perished by local accidents, or been extirpated by man.

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voit seule donner un caractère de démonstration à mes résultats sur cette creation ancienne.—*Cuvier, Recherches sur les Ossemens Fossiles*, Discours Freliminaire, 1821, vol. i. p. 1.

To the first of these points I shall offer no other reply than to refer him to the distinctions between *alluvium* and *diluvium*, as stated by Cuvier in the Introduction to his History of the Pachydermata, tom. 1.; the slightest perusal of which cannot, I think, fail to convince the reader that it is utterly impossible to explain the phenomena which I have called Diluvial, by any causes at present in operation.

I would also refer to the luminous paper of Sir James Hall, in the Edinburgh Philosophical Transactions, 1813, on the evidences of an inundation afforded by the Corstorphine Hills, and other summits, in the neighbourhood of Edinburgh; and to the accurate and practical distinctions drawn by Mr Bald, in the third volume of the Wernerian Memoirs, p. 123, and fourth volume, p. 58., between the old and the new alluvial covers along the east coast, and in other parts of Scotland; and have only to add, that my own observations in that district, during the last summer, enable me to bear testimony to the fidelity of description both these gentlemen have maintained in the publications alluded to. It is needless here to repeat the evidences of diluvial action afforded by deposits of loam and gravel, in situations to which no river could have ever brought them, which I have collected in the chapter following p. 185. of the first edition of my Reliquiæ Diluvianæ. I shall deem it sufficient to subjoin the following extract from a foreign scientific journal, which shews that the distinctions I am contending for, are generally admitted by the best observers of the present time.

"We believe the best geologists of the day agree in limiting the term *alluvial*, to those deposits which result from causes now in action, and in appropriating the term *diluvium*, to those universal deposits of gravel and loam, to the production of which no cause at present is adequate, and which can only be referred to the waters of a sudden and transient deluge. This gravel and loam are always confusedly mixed together, and are thus distinguished from the older deposits of sand and gravel which occur in regular alternating beds. The ablest writers in Europe now adopt these distinctions, and would no more think of confounding them, than to describe, under the same name, gypsum and limestone *."

* Review of Reliquiæ Diluvianæ in Silliman's American Journal of Science, vol. viii. No. 2. p. 326.

With regard to the second proposition, I think that, to a certain point, Dr Fleming's opinions and observations are correct. I fully coincide with him in believing, that the beaver and the wolf, like the roebuck, probably the bear, and the Irish elk, have gradually disappeared, together with the various species of birds, whose expulsion or extirpation he so ably describes, before the arrows of the hunter, and the snares of the agriculturist; but when he proceeds to apply the same explanation to the bones of quadrupeds, imbedded often many fathoms deep in masses of drifted clay and gravel, in situations at or near which no such deposits are at present taking place, or in caves and fissures which have been wholly closed, and whose interior has been often filled with the detritus of rocks introduced by the same sudden and transient inundation, to which alone the existence of the superficial deposits in question can be referred, I feel obliged to object to the application of the principle of gradual extirpation to this part of the subject, and appeal to the entire body of phenomena detailed in my Reliquiæ Diluvianæ, in support of my opinion.

But while I thus contend that there is evidence of a sudden and general destruction of animals by a transient inundation of the Earth's surface, I, at the same time, proceed most willingly with Dr Fleming to apply his method of illustration, to the animals inhabiting the earth antecedently to this great aqueous revolution; to explain the phenomena of the den at Kirkdale by the habits of living hyænas, and to argue on the probable history of fossil elephants and hippopotami, from the known habits of those which at present inhabit the banks of the Ganges or the Niger; and without this practice of illustrating the history of the fossil dead by the study of their living representatives, I could never have arrived at the conclusions I have founded on the evidence of the den at Kirkdale.

I proceed, therefore, to examine, in the order in which he has stated them, some of the propositions advanced by Dr Fleming.

### Case I.—*Peat-Bogs.*

I agree with Dr Fleming in considering the bones discovered in our peat-bogs; in mud and silt at the mouths of rivers,

or within the level of their floods; and in ponds or lakes, and other situations at or near which the formation of aqueous deposits is still going on, to prove that the horse, the ox, the boar, the beaver, and several species of deer, have existed as wild animals in this country since the formation of post-diluvian silt and peat began, and have been gradually extirpated, or domesticated, by man; and I admit this on evidence independent of the documents of history, or the voice of tradition, viz. the fact, that the bones of these animals occur imbedded in the deposits in question.

Case II.—*Fresh-Water Marl-Beds under Peat-Bogs.*

Deposits of this kind, formed at the bottom of shallow lakes and ponds, accumulate, until they arrive so near the surface of the waters, that the growth of peat commences, and often continues so far, that the hollow, which was once a shallow lake, becomes entirely filled up; the basis of the marl-bed, beneath this peat, is sometimes solid rock, and sometimes a bed of that ancient detritus of gravel, clay, or sand, which I have called Diluvium. The animal remains which occur in this fresh-water marl are of post-diluvian origin. Now, with respect to the Irish elk, if the common accounts should prove correct, that it is found in this shell-marl, immediately below the peat, or in the lower regions of the peat itself, it will only add another species to the list of animals that have repeopled this country, since the formation of the diluvium, and which, like our beavers and wild boars, have been extirpated by man; and the high state of preservation of its horns and bones from the bogs of Ireland, when compared with the usually decayed condition of bones from the diluvium, inclines me to favour this opinion. This animal, however, should it prove to be thus recent, will, like the ox and horse, and other species of deer, be common to our diluvial deposits, with those that are post-diluvial; it occurs with elephants and hippopotami in the diluvium, at Walton in Essex, and in the diluvial gravel of Germany, France, &c.* The evidence to prove its more recent existence, should, therefore, be carefully attended to, and forms an interesting subject of inquiry.

* A detailed description of the remarkable Skeleton of the Fossil Elk, discovered in the Isle of Man, and now preserved in the Museum at Edinburgh, will be given, by Professor Jameson, in a future Number of this Journal.

Case III.—*Horns of Rhinoceros in Scotland.*

Dr Fleming states, p. 297, "that a specimen of the horn of the fossil rhinoceros, found in one of the marl-pits at the Loch of Forfar *, exists at present in the Edinburgh Museum, and we have been informed by Professor Jameson, that two other examples have occurred in Blair-Drummond moss on the banks of the Forth. It is to be hoped, he adds, that the skulls will yet be procured."

Could the above cases be established, they would be decisive in favour of the theory maintained by Dr Fleming. In my Reliquiæ Diluvianæ, p. 33, I have expressed an opposite opinion, that the horns of the rhinoceros neither have nor are likely to be ever found in a fossil state, unless when preserved in ice. I made it my business, therefore, whilst at Edinburgh, carefully to investigate the cases here alleged to have existed in that neighbourhood, and the following are the results.

Mr David Don informs me that the horn in the Edinburgh Museum was presented by himself and his brother, on the death of their father in the year 1814, to a museum then existing at Dundee, which was shortly after broken up and the contents sold by auction; and that the story of its having been found in the Loch at Forfar, must have been invented either by the auctioneer or the person who bought it of him, and sold it again to the Museum at Edinburgh, at a price proportionate to the increased value that would justly have been attached to it, if it had really been a Scotch fossil †. Mr Don, however, affixed no such history to it when he gave it to the museum at Dundee. In his letter to me on this subject, he says, "It had a long time been in possession of my father, who, I am inclined to believe, obtained it from some friend, whilst he was Superintendant of the Royal Botanic Garden at Edinburgh." He adds, "Had it been found in the lake at Forfar, it is not probable that so remarkable a circumstance should have remained un-

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* Vide Wern. Mem. vol. iv. p. 582.

† In the recent sale at Fonthill, the public were, in a similar manner, informed, that a vase of rock crystal, submitted to the hammer, was a real topaz; and the authority of Professor Buckland, who had never seen or heard of the gem in question, was advanced in confirmation of the alleged fact.

known to me, or any of my family." The story of the other two horns of the rhinoceros, said to have been found in the moss of Blair-Drummond, and of which Dr Fleming is anxious, as he well may be, to discover the skulls from whence they were derived, I found to originate in another mistake of a similar kind. Professor Jameson had been informed of the supposed discovery of these horns by a gentleman of Stirling, who is factor to the estate of Blair-Drummond. I proceeded to Stirling, and found this gentleman to be a man of accurate observation as to the geological structure of this district, and particularly of the peat and alluvial deposits in which these horns were said to have been discovered. But he informed me that he had no personal knowledge of the finding of them; that the discovery was made many years ago by some of his father's workmen, who, together with his father, are now dead; but that he believed the horns were still existing in the House of Blair-Drummond. I applied forthwith to Mr Drummond for further information, and learnt from him that there were some years ago two horns of a rhinoceros somewhere about his house, and that they have since been removed to that of his mother in Edinburgh *. He further adds, "I know nothing of their history but what my factor tells me, and he seems uncertain whether his father had seen them dug up, or had only been told by some person that they were found at some former time. As to the question about their having been found at Blair-Drummond, I can only answer with safety, in the terms of a verdict peculiar to our criminal courts, "Not Proven."

The evidence then before us amounts to nothing more than this,—that there exist two horns of a rhinoceros, which at some unknown former time were found in some unknown place, by some unknown person, and preserved in some unknown room in the mansion of Blair-Drummond, from which they have since

* Professor Jameson has lately examined these horns, and informs me, "that they differ not in shape from those of the living two-horned rhinoceros; that the fibres at their base exhibit the usual transparency of recent horns, and that the base of one of them is perforated with round sinuous holes, like those made in timber by the Teredo, but smaller." Holes of this kind are not uncommon in recent horns of this animal; they occur in a specimen in the Oxford Museum, and still retain within them the husk or sheath of some parasitic worms resembling maggots, by which they were produced.

been removed to Edinburgh: Until, therefore, the bones or teeth of these animals shall be found in the moss of Blair-Drummond, or the loch of Forfar, or the skulls procured, which Dr Fleming hopes to find, we remain without the slightest evidence of the rhinoceros having been a postdiluvial inhabitant of Scotland.

Case IV.—*Fossil Hippopotami.*

A hippopotamus is recorded by Lee, in his Natural History of Lancashire, as having been found *under* a peat-bog in that county.

This evidence is unfortunately too imperfect to be of use in a disputed point. We simply learn from it that the bones were not in the peat but under it; but whether the foundation on which this peat-bog lay was a bed of postdiluvian shell marl, or of diluvial clay or gravel, we are not informed. The analogy of the other localities in which the hippopotamus has been found in England, leads to the latter hypothesis.

Dr Fleming concludes, "These animals, formerly inhabitants of this country, have their remains preserved, not only in peat-bogs and marl-beds, but likewise in the silt of our great rivers; in the valley of the Thames, for example, they occur in the regular stratified clay, sand, gravel, and peat." As this conclusion is founded chiefly on the cases I have already discussed, it must stand or fall with them. He proceeds to support it further, by stating, on the misinterpreted authority of Mr Trimmer's paper in the Philosophical Transactions, that the hippopotamus and elephant occur in the valley of the Thames, in the regular stratified clay, sand, gravel, and peat. In reply to this, I venture to assert, that no remains of this kind have ever been found in the peat-bogs of any part of the valley of the Thames, and still less in the regular stratified clay, that is, the London Clay. The case described by Mr Trimmer is that of the brick-earth-pits at Brentford, which I visited last week, and where there is not the smallest trace of any kind of peat-bog to be seen. The patches of peat mentioned by Mr Trimmer in his paper, as being only two or three inches thick, and of small extent, were portions of drifted peat, or other vegetable matter that became lodged and entangled in the sand and gravel, at the same time

with the bones in question. Their extent must have been very small, for not a particle of peat is now visible, although a larger section is open than existed at the time when Mr Trimmer made his observations.

But even admitting all the facts which Dr Fleming contends for as to this point, and supposing it proved that the elephant, rhinoceros, hippopotamus, hyæna, and other lost animals, have existed recently in Europe, and been extirpated by man, and that we found their remains in postdiluvian deposits of peat, and silt, and fresh-water marl; they would only be in the same predicament with the horse, the ox, the fox, the wolf, the boar, the beaver, and others, whose bones are common to these postdiluvial formations, as well as to antediluvian caves and fissures, and to beds of diluvial gravel: still every atom of the evidence contained in my *Reliquiæ Diluvianæ* would remain unaffected by this discovery, and the great and universal phenomena of diluvial deposits would still be equally inexplicable, without appealing to the agency of a transient and general inundation of the Earth *.

Dr Fleming, however, at page 299, speaking of what Mr Bald denominates the "old alluvial cover," and many English mineralogists "diluvium," concludes, that "The partial oc-

* Dr Fleming, speaking of the gradual extirpation of certain well known animals in this country and on the continent, says, "These changes have all taken place in the course of the last six or eight centuries; in ages that have preceded, the same causes must have been in more or less active operation," &c. In short, by the theory of gradual extirpation, he would explain the extinction of the lost species of elephant, rhinoceros, hippopotamus, hyæna, &c. over Europe. Is it not incumbent on him first, to show at what period such animals as these, much too formidable to be overlooked, were ever known to have existed? Can he give any reason why hyænas should have been extirpated at a more early period than wolves, had they ever existed in postdiluvian Britain? Or is it probable, that the savage hordes which inhabited Germany before its occupation by the Romans, should have utterly destroyed such powerful animals as the elephant and rhinoceros, as well as the hyæna, from the impenetrable fastnesses of the great Hercynian forest, when animals of the same kind have not yet ceased to abound in the woods of India and the wilds of Africa, in spite of a farther persecution of nearly two thousand years? Surely the theory of their extinction by the savage natives, preceding the Roman invasion of these countries, is a matter of the highest improbability; their existence at that time, and subsequent extirpation, is, in the utter silence of Cæsar and Tacitus, and all later historians, and even of tradition, a moral impossibility.

currence of these strata,—their limited extent,—great difference of character in neighbouring districts,—the presence of remains of terrestrial animals,—and the absence of marine exuviæ,—demonstrate that a universal flood, possessing the velocity some have assigned to it, had no share in this formation." Here again we are obliged to differ from Dr Fleming as to matters of fact.

1. With respect to the asserted "partial occurrence of these strata, and their limited extent, I know not what may have been his opportunities of locomotion and observation; but I dare assert, that, in the whole course of my own geological travels, from Cornwall to Caithness, from Calais to the Carpathians, in Ireland or in Italy, I have scarcely ever gone a mile, without finding a perpetual succession of deposits of gravel, sand, or loam, in situations that cannot be referred to the action of modern torrents, rivers, or lakes, or any other existing causes; and with respect to the still more striking diluvial phenomena of drifted masses of rocks; the greater part of the northern hemisphere, from Moscow to the Missisippi, is described by various geological travellers as strewed, on its hills as well as valleys, with blocks of granite and other rocks of enormous magnitude, which have been drifted (mostly in a direction from north to south) a distance sometimes of many hundred miles from their native bed, across mountains and valleys, lakes and seas, by a force of water, which must have possessed a velocity to which nothing that occurs in the actual state of the globe affords the slightest parallel. I must therefore deny, that the occurrence of these deposits is partial, or their extent limited.

2. That their character is different in neighbouring districts I readily concede; for it often differs in the same field, and even in the same pit or quarry; as well it may do, considering the turbulent condition induced on the Earth, by the inundation of which it is the wreck and rubbish.

3. The presence of the remains of terrestrial animals, simply shews that they had perished; but whether they were drifted from other countries to those in which we now find them, or how far they may have been floated backwards and forwards, by the flux and reflux of the mighty currents then in motion, before the carcasses became putrid, and the bones fell piecemeal into the gravel, as the agitation was subsiding, we have no means

to judge; and, without the evidence afforded us by the interior of caves and fissures, we should have been unable to prove that the elephant, rhinoceros, hippopotamus, and hyæna, had ever inhabited Europe; as it might have been argued, that these animals were all drifted from the tropical regions now occupied by such genera, by the waters of the same inundation that produced the superficial deposits of gravel, loam, and sand, in which alone their bones had been discovered before the investigations that have been made into the contents of caves and fissures.

4. The absence of "marine exuviæ" is another case of misstated facts. Had Dr Fleming ever examined the diluvial clay which forms the cliffs more than sixty feet high at the brick-kilns on the south of Peterhead, he might have found (as I did last summer), marine shells imbedded in it, similar to those which now live in the adjacent seas; and had he further examined the shells found in diluvium not many years ago in the bed of the Paisley Canal, three miles from Glasgow, and of which a list was published by Captain Laskey*, whilst a very perfect collection of them is preserved in the cabinet of Dr Browne of Glasgow; or, had he ever seen or heard of the thousands of acres of marine shells of existing species, which cover more than one-fourth part of the counties of Norfolk and Suffolk, so as to form an integral part of their gravel pits, and to be mixed in every possible proportion with ordinary diluvial gravel, sand, and clay, and with the bones of elephants and other land animals;—he would never have advanced such arguments as these to "demonstrate," that a *universal* flood had no share in the formation of what many English mineralogists have called *diluvium*.

I considered it needless when I was at Edinburgh, to investigate the fact asserted by Dr Fleming at page 298, that a copper battle-axe was found in digging the Union Canal at Bonnington, in the same kind of clay or till with the tusk of an elephant, as its accuracy is questioned by Professor Jameson himself, in a note subjoined to the passage in question; but I here adduce it as another of the mistakes that cannot but arise from neglecting or denying the distinction between Alluvium and Diluvium.

* See Annals of Philosophy, Feb. 1814, vol. iii. p. 150; and Wern. Soc. Mem. vol. iv. p. 568.

It remains only to notice a few more errors into which Dr Fleming has fallen, in his Observations on my History of Bones discovered in Caves and Fissures.

With respect to the habits of modern hyænas, I have to offer him my thanks for the manner in which he has disposed of the evidence of Dr Knox, in the fourth volume of the Wernerian Memoirs, p. 385, as inapplicable, on the ground of difference of species between the Fossil and Cape Hyæna. But I am surprised he should characterise as *valuable* the notices of any writer who argues, that, because lions and tigers do not devour bones, therefore hyænas also do not eat them; or that, because he himself has never seen an hyæna in the act of dragging off its prey, therefore they never do so. Is the positive evidence, then, which I have quoted from Brown, Sparman, and Busbequius, who assert the fact, that hyænas do drag off their prey, to be set aside by the negative fact that Dr Knox has never seen them in the act of doing it? Since the publication of my last edition, I have seen an officer from India, Captain Sykes, who has often hunted hyænas in the vicinity of Bombay; and from him I learn that he has not only seen heaps of bones accumulated at the mouths of their dens, but that, in digging one from its hole, he observed large quantities of bones flung out with the dirt and rubbish from the interior of the den.

Dr Fleming, at page 301, gives his opinion, that the "bones at Plymouth were washed by some *land*-flood from an open fissure, and deposited in confusion in the neighbouring caverns." Is he then ready to maintain, that the bones in the caves and fissures, and the gravel that occurs on the summit of the rock at Gibraltar, were deposited also by what he calls a land-flood? —and that many hundred other caves and fissures along the coast and islands of the Mediterranean, and the Adriatic, in Dalmatia, and in Germany, at various elevations from 2 to 2000 feet above the level of the sea, were all so similarly affected by partial land-floods, that there is not a tittle of difference in the effects produced on each and every one of them by such numerous and independent inundations? Or is he prepared to shew, how a land-flood could cover the summit of the insulated rock of Gibraltar, at the height of 1439 feet above the sea, without inundating at the same time at least nine-tenths of Europe, and

to point out the source from whence the waters of such a land-flood could be derived? Unless he is so, it is in vain to say a partial land-flood may have risen a hundred feet at Plymouth, and have moved the bones, and carried the mud and pebbles into the caves at Oreston; for this is but one of many hundred analogous cases of such bones imbedded in similar mud and pebbles that must be accounted for, and for which the only sufficient cause that has ever been proposed, is an universal and transient deluge.

Again, at page 301. Dr Fleming contends, that these bones found thus universally in fissures, and caverns connected with them, cannot have been drifted into their present position by the waters of a general flood, and there surrounded by them with mud; because, in the solitary instance of the large cavern of Wokey Hole, there is a river now running through it, and depositing mud and sand upon human bones or urns, or any thing else that may now, or at any modern period of time, have been deposited within the level of its winter floods. Surely it is at least incumbent on him to shew that there is, or may have been, a river running through every cave, and every fissure in the world, in which bones and bony breccias are found imbedded in mud, before he can establish a conclusion like this. His present argument is stated thus: "A subterranean river runs through the cave at Wokey, which may have deposited mud during its highest floods. But why may not the mud in the Kirkdale cave have been deposited by a similar agent?" The answer is, because there is no river there to deposit it; and, because it is impossible that now, or at any past period of time, any river should flow, or ever have flowed there. The cave at Wokey is a connected series of large and lofty vaults, with two apertures near its floor, by which a subterranean river at this moment runs in and out continually; whilst that at Kirkdale is a small hole, (seldom larger than a large gutter hole), not five feet square at its mouth, and branching internally into smaller ramifications, which finally terminate in a close end, or *cul de sac*; so that by no possibility could any river now, or at any past time, have found a passage through it.

I fully agree with the observation quoted from Mr Young, that the cave at Kirkdale is not a fissure in the rock, and that it

has a number of rounded hollows or depressions in the sides and roofs, (and possibly on the floor, though I have never seen them there), resembling such water-worn hollows as we see in rocks, in the beds of rivers, or on the shores of the ocean; but I must add, that the appearance of such hollows is a feature common to the cave of Kirkdale, with every other cave in limestone rocks that I have ever examined; and in every case there is decided evidence that the hollows have not been produced by friction from moving water, in the fact, that, though not unlike in shape, they are never smooth and polished like the holes worn in the beds of rivers, and on the sea-shores, but are constantly rough and studded over internally, like a corroded preparation, with thousands of small and delicate points, projecting in high relief over these surfaces, and which would inevitably have been destroyed, had friction or any kind of violence been employed in producing the cavities in question.

In reply to the note at page 300, in which the authority of Professor Goldfuss, is quoted by the editor to support an opinion, that the elk and hyæna are the animals intended by the terms *schelch* and *halb-wolf* in the romance of the Niebelungen written in the 13th century, and enumerated among the beasts slain in a hunt a few hundred years before that time, in Germany; I have only to observe, that the authority of the same romance, would equally establish the actual existence of giants, dwarfs, and pigmies, of magic tarn caps, the using of which would make the wearer become invisible; and of fire-dragons, whose blood rendered the skin of him who bathed in it of a horny consistence, which no sword or other weapon could penetrate *.

Dr Fleming will, I am sure, excuse me if I suggest to him, that the tone of levity in which he speaks of the facts established by the evidence of the den at Kirkdale, as a parallel case to the fables of travellers who have pretended to discover the decayed timbers of the Ark, is not the most appropriate to a discussion of the nature now before us.

*Appendix.*—Since this article was sent to the editor, Dr Fleming has published a second paper, in No. XXIII. of this Jour-

* Vide Weber's Northern Romances, p. 172.

nal, in which he proposes to explain the universal dispersion of diluvial deposits by the bursting, at different periods, of an almost universal series of lakes. Had such lakes ever existed, it may fairly be asked, Where are the traces of their ancient locality? It is evident from the terraces, or parallel roads, in the valleys of Glen Roy, Glen Gloy, and Glen Spean, and some three or four more, which are all that have hitherto been noticed on the surface of the whole earth, that wherever such lakes have burst their ancient barriers, they have left behind them, in these terraces, evidence that shews the amount of their former extent and successive depressions. Even river-floods, of any magnitude, produce a similar effect, and form terraces in the adjacent gravel-beds that mark the line of their highest inundations, as I have stated, in a note at page 217, first edition, of my *Reliquiæ Diluvianæ.* Is it not, then, utterly impossible that such an universal system of lakes as Dr Fleming's hypothesis assumes, could ever have existed, without leaving on their banks similar terraces to those of Glen Roy, Glen Gloy, and Glen Spean? Not one example, however, of such a terrace occurs in England, a country that is half covered with diluvial gravel. Neither, I believe, are there any other lacustrine terraces in Scotland, but those just mentioned, although river terraces are very common. Until, therefore, such lacustrine terraces are found to be of nearly universal occurrence on the sides of upland valleys, we remain without a particle of evidence that such lakes have either existed or become extinct, and must consider the assumption of Dr Fleming respecting them to be altogether gratuitous.

In this same second memoir, p. 294. and 295, I find Dr Fleming advances facts, in direct contradiction to the *demonstration* in his former paper, that a universal flood had no share in the formation of our diluvium, which, in part at least, he founded, on the asserted "*absence of marine exuviæ*" in diluvial deposits, viz. those I have stated, that marine shells of existing species occur in the diluvium of the neighbourhood of Peterhead, and in the Paisley Canal, near Glasgow. He also quotes other cases of the same kind, *e. g.* that of recent marine shells, discovered by Mr Adamson on the banks of Loch Lomond, &c.; but he makes no allusion to his denial, in his former paper, of the existence of such deposits, and I presume could not have

been aware of facts which so materially affect his argument, at the time of his writing the paper in question. At any rate, it would have been more candid to acknowledge his error, than to leave to me the task of pointing it out, and applying it to my advantage in the matter at issue between us.

I forbear at present to offer any farther remarks on Dr Fleming's second memoir, as I should be drawn to greater length than the patience of your readers would tolerate, or the limits of a Journal, destined to be the vehicle of original communications, rather than of controversial discussions, could with propriety admit.

*Postscript.*—I have just been informed by Mr Weaver, that he has established, beyond all doubt, the fact of the elk having existed as a postdiluvial animal in Ireland. Its bones and horns, he says, occur in the Bog of Kilmegan near Dundrum, in the county of Down; they lie at the bottom of the peat between it and a bed of shell-marl, resting upon, or being merely impressed in the marl, which is composed of a bed of fresh-water shells, from one to five feet thick, and must have been formed while the bog was a shallow lake. In this and other similar lakes and swamps, Mr Weaver imagines these animals fled for refuge from their enemies, and were drowned in the waters, or swallowed up in the morass.