



I. A reply to Mr. Playfair's reflections on Mr. Kirwan's refutation of the Huttonian theory of the earth

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To cite this article: Richard Kirwan Esq. LL.D. F.R.S. P.R.I.A. (1802) I. A reply to Mr. Playfair's reflections on Mr. Kirwan's refutation of the Huttonian theory of the earth , Philosophical Magazine Series 1, 14:53, 3-13, DOI: [10.1080/14786440208676152](https://doi.org/10.1080/14786440208676152)

To link to this article: <http://dx.doi.org/10.1080/14786440208676152>



Published online: 18 May 2009.



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THE
PHILOSOPHICAL MAGAZINE.

I. *A Reply to Mr. PLAYFAIR'S Reflections on Mr. KIRWAN'S Refutation of the Huttonian Theory of the Earth.*
By RICHARD KIRWAN, Esq. LL. D. F. R. S. and
P. R. I. A.

DEAR SIR,

Dublin, Aug. 24, 1802.

WHEN I first undertook to state some objections to Dr. Hutton's Theory, I little imagined I should call forth such indignation and such illiberal personalities as appeared in the second edition of that work; but however hurt the self-love of an author might have been by an attack on his favourite ideas, I had still less reason to foresee that the same style of hostility would have been persisted in by any person to whom I had given no offence whatsoever. Mr. Playfair's Illustrations of the Huttonian Theory, which I have seen only a few days ago, convince me I was mistaken; he not only attempts to justify the *asperities*, as he calls them, of Dr. Hutton, but aggravates them by new invectives. You need not fear, Sir, that I shall pollute the pages of your journal by the vulgar mode of retaliation; I shall make no reflections on his defence of Dr. Hutton's Theory: enough has been said on that subject. I shall content myself with repelling his unprovoked and unmerited attacks on myself.

To effect this, however, I shall be obliged to state his abusive paragraphs, and thus expose their indecency. It is the only vengeance I shall take; he, probably, will account it none.

Page 119. "To assert, that in the economy of the world we see no mark either of a beginning or an end, is very different from affirming, that the world had no beginning, and will have no end. The first is a conclusion justified by common sense as well as sound philosophy; while the second is a presumptuous and unwarrantable assertion." Here I must deny that the first assertion, namely, that we see no mark of a beginning, is a conclusion justified by common sense and sound philosophy.

No. LIII.

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Mr. De Luc, Saussure, and Dolomieu, were not deficient either in common sense or sound philosophy, yet they all assert that we may discover evident marks of the beginning of the world in its present state: nay, Dr. Hutton himself allows it, for he judged the actual world to have proceeded from a preceding, and asserts that it will have an end. What Mr. Playfair must then mean is, that we can see no trace of the beginning of this succession of worlds. Of such succession, it is true, we can trace no beginning, because it is merely fictitious: but while Dr. Hutton asserted the reality of such a succession without assigning any limit, it was natural for me, who was totally unacquainted with him, to infer that he really judged it to have no beginning: this conclusion was so natural, that it occurred to others long before I had written on this subject. Mr. Williams, his countryman, who was probably acquainted with him, says, "That Dr. Hutton aims at establishing the belief of the eternity of the world, is evident from the whole drift of his system, and from his own words; for he concludes his singular Theory with these singular expressions: 'Having in the natural history of the earth seen a succession of worlds, we may from this conclude that there is a system in nature—in like manner as, from seeing the revolutions of the planets, it is concluded that there is a system by which they are intended to continue these revolutions. But, if the succession of worlds is established in the system of nature, it is in vain to look for any thing higher in the origin of the earth. The result, therefore, of our present inquiry is, that we find no vestige of a beginning, no prospect of an end.'"—Williams's *Natural History of the Mineral Kingdom*, Preface, lx.

Now, I ask what can be the meaning of these last words, *it is in vain to look for any thing higher in the origin of the earth*. Higher than what? Is it not plain that the meaning is, *higher* than that established succession, of which succession we can trace no beginning? And what is a succession of which we can trace, and to which we assign, no beginning? Now Dr. Hutton, in his first edition, nowhere mentioned that it had a beginning, though such beginning were not apparent on bare inspection of the actual world: was not then his meaning at least ambiguous?

So also Mr. Howard, in his learned work on the Structure of the Earth, p. 549, says: "Dr. Hutton rejects all time, the operations of his living renovating nature scorn all limits: time (says he), which measures every thing, is to nature endless and nothing." But to return to Mr. Playfair: "Mr. Kirwan, in bringing forward this rash and ill-founded
censure,

censure, was neither animated by the spirit nor guided by the maxims of true philosophy. By the spirit of philosophy he must have been induced to reflect, that such *poisoned* weapons as he was preparing to use are hardly ever allowable in scientific contest, as having a less direct tendency to overthrow the system than to hurt the person of an adversary, and to wound, perhaps incurably, his mind, his reputation, or his peace." This severe censure appears to me unmerited : of its liberality I leave others to judge. The mention of preparation of *poisoned* weapons is perfectly risible, when it is considered that the whole argument is comprehended in ten or twelve lines. If Dr. Hutton had lived either in Spain or Portugal, some hurt to his person might indeed be apprehended ; but in Britain, where Mr. Hume, with impunity, trespassed much more on the received religious principles, no danger could rationally be suspected ; and it were idle to think that the reputation of an author could any more be wounded by an inference obviously deducible from his principles, than by his own statement of those principles.

Mr. Playfair continues : " By the maxims of philosophy he (Mr. Kirwan) must have been reminded, that in no part of the history of nature has any mark been discovered either of the beginning or end of the present *order*." This I deny, in common with those eminent geologists already mentioned : clear traces of a *beginning* are found. " By attending to these considerations Mr. Kirwan would have avoided a very illiberal and ungenerous proceeding ; and, however he might have differed from Dr. Hutton as to the *truth* of his opinions, he would not have censured their tendency with such rash and unjustifiable severity." I never once considered the tendency of his opinions, but merely their direct consequences ; I had nothing to do with their tendency in a mere geological treatise.

Page 143. " It has been asserted that Dr. Hutton maintained all calcareous matter to be originally of animal formation : this position, however, is so far from being laid down by Dr. Hutton, that it belongs to an inquiry which he carefully avoided to enter on."

Page 147. " It is nevertheless true, that Dr. Hutton sometimes expressed himself as if he thought that the present calcareous rocks are *all composed of animal remains* : this conclusion, however, is more general than the facts warrant, and, from some incorrectness or ambiguity of language, is certainly more general than he intended." Yet, p. 156, treating of my account of the origin of coal mines, he says,

"It is indeed worth while to compare what is said concerning the degradation of mountains in the above quotations (from my Geological Essays) with what is advanced concerning their indestructibility in another passage of the same volume, namely, all mountains are not subject to decay; for instance, scarce any of those that consist of red granite, &c. One can be at no loss about estimating the value of a system in which such gross inconsistencies make a necessary part." Mr. Playfair then finds a gross inconsistency in maintaining that *some* mountains are subject to decay, though *all* mountains are not; facts proved beyond the reach of contradiction; but he can see none in his own two paragraphs.

Page 157. "The quantity of hornblende and filiceous schistus necessary to be decomposed in order to produce the coal strata presently existing, is enormous. It is true that Mr. Kirwan, never at all embarrassed about preserving a similitude between nature as she now is, and as she was heretofore, lays it down, that the part of the primeval mountains, which is worn away, contained much more carbon than the part which is left behind: this, however, is an arbitrary supposition." Not quite arbitrary neither. Mountains before the flood must have been in many respects differently circumstanced from the present; and if at present, after attaining their utmost state of consolidation, many of them are in a state of decay, much more liable to it must they have been then. Hornblende and filiceous schisti are not the only stones that contain carbon, nor are the mountains that present these rocks the only mountains that contain veins of it: many granite mountains also present them. Mr. Playfair, indeed, pays little regard to the authorities I adduce to prove the facts I allege; more impartial readers may possibly pay more.

I shall therefore quote one entitled to the highest credit. Citizen Haüy, in the third volume of his Mineralogy, p. 308 and 309, tells us that anthracite (native mineral carbon loaded with stony matter) belongs exclusively to primitive countries, and that the observations of Mr. Dolomieu prove the existence of carbon independently of animals and vegetables; and that anthracite was, he presumed, nothing more than pure carbon, associated, by accidental causes, with a certain quantity of iron and flux. And Mr. Duhamel has shown in a memoir, approved by the Academy of Sciences, that the argillaceous *strata* that intercede beds of coal are formed of the *detrites* of the neighbouring mountains: *Journal des Mines*, viii. p. 40; and Haüy, iii. p. 319. My system is not then quite

quite *original*, in the farcastic sense in which Mr. Playfair applies this word; but it is evident irritation was his sole purpose.

Mr. Playfair adds, "We may also object to Mr. Kirwan, that the siliceous part of the mountain has not been chemically dissolved; it has only been abraded and worn away. Mechanical action has reduced the quartz to gravel and sand, but has not produced on it any chemical change; the carbon, therefore, could not be set loose." Mr. Kirwan has not assumed, that carbon was set loose from quartz, though it might have been from siliceous schists and other compound stones and rocks: disintegration is often effected by decomposition; thus felspar is converted into argil and siliceous particles in many instances; but this more frequently happens to stones that contain iron.

Page 158. Mr. Playfair, objecting to strata formed by *transfudation*, asks what occupied the space of the coal bed before the transfudation from the upper part of the mountains. The question is unfair, and very similar to the numerous difficulties objected to Dr. Black's discovery of fixed air, before its truth was generally acknowledged. A fact is often discovered, though the mode of its production be unknown; yet in this case the question is easily answered: the carbonic part of the coal in the mountain of St. George's here alluded to was formed before the upper part of the mountain was formed; it is only the bituminous ingredient that subsequently transfused from the supervenient superior strata; and I suppose it will be admitted that petrol might penetrate and coalesce with the carbonaceous part, without floating the upper part of the mountain, as Mr. Playfair ludicrously supposes. This account is so much the more probable, as this mountain is formed of a mixed calcareous stone abounding in argil; and this species of calcareous stone is of secondary formation, as Mr. Hassenfraz, the author of this memoir, truly remarks, p. 266.

Mr. Playfair concludes by remarking, "that such reasoning is so great a trespass on every principle of common sense, that to bestow any time on the refutation of it, is, in some measure, to fall under the same censure."

Page 171. "If any one asserts, as Mr. De Luc has done, that sand is a chemical deposit, a certain mode of crystallization which quartz sometimes assumes, let him draw the line which separates sand from gravel; and let him explain why quartz in the form of sand is not found in mineral veins, in granite, nor in basalt; that is, in none of the situations where the appearances of crystallization are most general and best ascertained." What is meant by a *chemical deposit*, I do not

understand; but that sand is sometimes found regularly crystallized is evident, for a whole stratum of such sand has been found at Neuilly: to expect that a substance exposed to endless friction should always be found in regular crystals would be extravagant; that of Neuilly was therefore formed on the spot in which it was found.—A vein filled with sand has been found among the mines of Peregruba in Siberia. Renovantz, Preface, xiii. Gravel and sand differ only in size, and to expect either in granite rocks would be inconsistent; but on and beside granitic mountains gravel very frequently occurs. The crystallization of basalts is so far from being ascertained, that, on the contrary, it has been demonstrably proved that basaltic prisms are not crystallized, by those that have paid most attention to this subject.—Romé de Lisle, i. p. 439; Haüy Miner. iv. 476.

Page 180. "The Neptunist, who has provided the means of dissolving the materials of the strata, has only performed half his work, and must find it a task of equal difficulty to force this powerful menstruum to part with its solution. Mr. Kirwan, aware, in some degree, of this difficulty, has attempted to obviate it in a very singular way. First, he ascribes the solution of all substances in water, or in what he calls the *chaotic fluid*, to their being created in a state of the most minute division. Next, as to the deposition, the solvent being, as he acknowledges, very insufficient in quantity, the precipitation took place (he says) on that account the more rapidly. If he means by this to say, that a precipitation without solution would take place the sooner, the more inadequate the menstruum was to dissolve the whole, the proposition may be true; but it will be of no use to explain the crystallization of minerals (the very object he had in view), because to crystallization it is not a bare subsidence of particles suspended in a fluid, but it is a passage from chemical solution to non-solution or insolubility that is required."

My meaning is clearly stated in pages 10 and 11 of my Geological Essays, that the solids contained in the chaotic fluid were not dissolved by that fluid, but were contained in that state of minute division to which, if the fluid could of itself dissolve them, they would be reduced; and that, if the quantity of that fluid were insufficient to hold them in solution, this circumstance would hasten their crystallization, precipitation, and deposition, respectively. I did not assert that the solution was effected by the menstruum, but, on the contrary, denied it. Mr. Playfair's assertion, that crystallization is a passage from chemical solution to non-solution or insolubility, is denied by Bergman: "*Sed non tantum vere soluta*
in

in aqua determinatas acquirunt formas, verum etiam ni fallor satis attenuata." Bergman, ii. p. 15. It is sufficient that the particles suspended and sufficiently attenuated have an affinity to each other.

Page 22. "Barytic earth is well known to have a stronger attraction to fixed air than common calcareous earth has, so that the carbonate of barytes is able to endure a great degree of heat before its fixed air is expelled: accordingly, when exposed to an increasing heat at a certain temperature, it is brought into fusion, the fixed air still remaining united to it: if the heat be further increased the air is driven off, and the earth loses its fluidity." And, p. 185, "Carrara marble may require a heat of 6300° of Wedgwood to melt it in the open air; but under such a pressure as would retain this gas it cannot be inferred that it might not melt with the heat of a glasshouse furnace. In like manner it may be true that 280 cubic inches of air acting on charcoal cannot effect the fusion of one grain of this marble after its fixed air is driven off from it; but we cannot from thence draw any inference applicable to a case where the carbonic acid is retained, and where the action of heat is independent of atmospheric air." Now, in no experiment with which I am acquainted has native aerated barytes been fused without the expulsion of its air, or union with the earth of the crucible. Dr. Hope, indeed, fused it in a black lead crucible, but found it lost 23 per cent. nearly of its weight; which is the whole, or nearly so, of the air contained, and accordingly it made but a very slight effervescence with marine acid. Hence the position laid down, p. 22, that barolite or native carbonate of barytes may be fused and still retain its fixed air, is founded on no experiment; but from all known experiments the contrary inference is fairly deducible with respect to it, as well as with respect to Carrara marble; nor is there any reason to think that a lower heat, independent of the atmosphere, could have any other effect.

Page 201—203. The fact alluded to, namely, that shells are found incorporated in the body of a rock at a great height near Guancavelica, I have fully stated and explained in a dissertation long since printed, and which accompanies this letter; to which I shall therefore beg leave to refer*.

Page 242. "Mr. Kirwan, in order to account for the magnitude of masses of iron found in Siberia and Peru, supposes that small pieces of native iron have been originally agglutinated by petrol: this is, no doubt, the most singular of all the opinions advanced on the subject; and, as it bor-

* See the next article in the present Number.—EDIT.

rows nothing from analogy, it admits of no proof and requires no refutation." I was, however, led into this opinion by analogy with Mr. Gadd's experiments in the 32d volume of the Memoirs of Stockholm; for he tells us, that if clay and calces of iron be plentifully mixed with oil, they will form a mass which will harden even under water. If Mr. Playfair were acquainted with Mr. Chladni's opinion, that these masses were fragments of a broken planet that fell within the sphere of attraction of our globe, he might possibly think it the most *singular*; yet even so, it is probable he would not sacrifice the pleasure of bestowing that distinguished epithet on mine. However, as he judged my conjecture, I know not on what foundation, inconsistent with the principles of chemical science, I have mixed rust of iron with petrol, and afterwards with petrol to which sulphur was added, and found it disposed to coalesce in a few days.

P. 422. "One of Mr. Kirwan's objections to the deposition of materials at the bottom of the sea is thus stated:— 'Frisk has remarked, in his mathematical discourses, that if any considerable mass of matter were accumulated in the interior of the ocean, the diurnal motion of the globe would be disturbed, and consequently it would be perceptible.' The appeal made here to Frisk is singularly unfortunate, as that philosopher demonstrated the contrary to Mr. Kirwan's position. The instance just given may serve as one of many to show what confidence is to be placed in that undigested mass of facts and quotations which Mr. Kirwan, without discrimination and without discussion, has brought together from all quarters." Mr. Playfair might, however, easily infer, from the loose manner in which Frisk is quoted, (namely, in *some* of his mathematical treatises, so different from my usual manner, in which the page I take from is mentioned,) that I had not that author before me; and, in fact, I took it from Mitterpacher's Physical Description of the Earth, p. 25, who says, that according to Frisk, in the case above mentioned, the velocity with which the centre of the globe would move would be increased; omitting the calculation, and the mention that the increased velocity would not be *sensible* for a long period of years. I hope therefore it is the only one out of the many quotations I have made in which any mistake can be found: if Mr. Playfair could find any other, he doubtless would have mentioned it. The attempt to weaken the force of the numerous facts I collected, adverse to the Huttonian theory, by calling them an *undigested* mass, is curious, and, if allowable, would furnish a very convenient and expeditious method of getting rid of

of them, but will not, I presume, appear perfectly satisfactory to an impartial public.

Page 427. The fact relative to the deltas formed in the mouth of the Bourampouër Mr. Playfair thinks I have misapprehended, because major Renneil does not assert that rivers employ *all* the materials which they carry with them, and deliver none into the sea. On the contrary, I think, they carry many of them into the sea, but not to any great distance, much less into its unfathomable depths, as Dr. Hutton asserts. The fact relative to the extension of coasts is now so well known to all modern geologists, that it were time lost to dwell longer on it: that the deltas themselves are diminished by particles detached from them, and carried into the recesses of the deep, is a remark which I do not recollect to have met with; but in some instances, where they consist merely of sand, they are often diminished by the winds: that even the argillaceous particles are not carried far into the sea, may be seen in Moris's American Geography, p. 49, Irish edition.

I decline entering into any further discussion of Mr. Playfair's replies to some other objection made by me to the Huttonian theory. The intelligent reader will meet with many confident and arrogant assertions of which he probably will require some proof, as in p. 481 and 482; many that are perfectly unsatisfactory or even contradictory, as where he allows that the impulsive motion of the waves against the shores is greater at a *small* distance from them, and yet asserts that the *detrites* of the shores are conveyed to a *great* distance. He also affirms with great confidence, that as the flowing of the tide requires just six hours, and the ebbing of it also six hours, the quantity of matter moved, and its velocity, must be just the same: p. 432. In the abstract this is certainly true, and in the middle of the ocean; but in most harbours the contrary happens: thus La Lande tells us that in the harbours of Brest, Dunkirk, Bordeaux, and Rouen, the ebb tide is about a quarter and often half of an hour slower than the flow: vol. iv. p. 117. These exceptions, I am persuaded, he is well acquainted with; but his *hurry and impatience* * (if I may be permitted to use his own expressions) to combat my assertions led him to overlook them. He is much at a loss to account for the remains of elephants and of the rhinoceros found in Siberia, and thinks it most probable that they belonged to ancient species of those ani-

* These are the causes to which he ascribes the many mistakes he supposes me to have committed in combating the Huttonian theory; to reason he thinks me incapable.

imals that could endure the severity of a Siberian winter: p. 475. If I had advanced such an opinion, it is probable he would be equally at a loss to find an epithet sufficiently severe to stigmatize it.

P. 481. It is evident, he says, that my geological writings are the work of a man who has not seen nature with his own eyes. This, I suppose, he infers from the total absence of observations made by myself. It is however pretty notorious here, that I have visited, traversed, and examined, most of the numerous mountains in this country; but I thought that in a controversy of this nature, the testimonies of persons who had taken no part in it, and who had seen much more than I have, would be more effectual and convincing: it is much easier to vilify my writings than to answer them.

No absurdity appears to Mr. Playfair so great as the attempt to connect the Mosaic history of the creation of the earth with any philosophical inquiry concerning it: this attempt he thinks injurious both to the freedom of philosophical investigation and to the dignity of religion: p. 477. The text of Moses he thinks covered with a veil which cannot be torn off, and must be considered as if it never existed: p. 478. Yet in other parts of his work he seems himself sensible of the extravagance of this assertion taken in the most extensive sense, p. 126; he seems to limit it to the *age, figure, and motion* of the *earth*; which no geologist ever pretended to infer from the Mosaic text, in which no mention of them is to be found, no more than they have the explanation of the act of creation itself; a notion which he also unjustly ascribes to them: but the series of events which took place after the creation of the earth, are too plainly mentioned to be overlooked or misunderstood: to say that this account is so obscure as not to be intelligible, is tantamount to saying that it is useless. It was never pretended that Moses intended to write a treatise of geology, any more than that Greek or Latin historians intended to give a treatise of astronomy from their occasional mention of eclipses or comets; nor is the genuine philosophical investigation of those phenomena any more impeded in one case than in the other: on the contrary, due notice is taken of their accounts by all astronomers. But there is a species of investigation, abusively called *philosophical*, which abstracts from historical accounts either of the creation or of the flood, as if the accounts given of both by Moses were unworthy of credit: to this objection the Huttonian theory is certainly liable, though I
never

never charged it upon it: so far was I from wishing to make use of what Mr. Playfair called poisoned weapons, as he unjustly accuses me of having done.

It is in vain Mr. Playfair seeks to compare geology with astronomy and zoology; neither of these sciences requires any notice to be taken of the original constitution of their objects, their actual state being very nearly the same as their primordial state: but, where hybrid species occur, their origin never fails of being attended to.

In geology, the case is very different: here we meet with objects whose original state must have been very different from their actual state. Rocks or stones presenting regular forms must of necessity have been originally in a state from which such forms could arise. Masses now hardened into stone, but presenting the impressions either of vegetables or of other stones, must have been originally in a soft state. Calcareous stones filled with shells must have been originally in a state fitted for the admission of those shells; hence geologists necessarily recur to a state of inanimate nature prior to the present. This is admitted by Dr. Hutton as well as by Neptunists; but he thinks the present state to have originated from a gradual destruction of a former, as that did from a still more antient: still the most antient of these worlds either resembled the present, or it did not: if not, we can say nothing of it; if it did, the same difficulties must occur, and consequently its primordial state must have been different from its subsequent state, as this state is supposed to have been similar to the present. The crystallized or soft state of our present rocks Dr. Hutton thinks proceeded from an igneous fusion of the materials of a prior world: but he cannot suppose this of the first of these worlds; its similarity with ours must therefore be otherwise accounted for.

I shall here conclude my observations on Mr. Playfair's observations. Controversies managed as this has been by him and Dr. Hutton, whose favourite method of answering objections consists in depreting or sneering at the understanding, and undermining the credit of their author, are a disgrace to philosophy, and sufficiently expose the weakness of the cause that obliges to have recourse to such expedients.

I am, &c. &c.

Mr. Tilloch.

RICHARD KIRWAN.