Theory of the Earth, Volume 1 (of 4) eBook

Theory of the Earth, Volume 1 (of 4) by James Hutton

BOOKRAGS

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WITH THE

EXAMINATION

OF

Different opinions on that subject.

IN EIGHT CHAPTERS.

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SECTION I.

Prospect of the Subject to be treated of.

When we trace the parts of which this terrestrial system is composed, and when we view the general connection of those several parts, the whole presents a machine of a peculiar construction by which it is adapted to a certain end. We perceive a fabric, erected in wisdom, to obtain a purpose worthy of the power that is apparent in the production of it.

We know little of the earth's internal parts, or of the materials which compose it at any considerable depth below the surface. But upon the surface of this globe, the more inert matter is replenished with plants, and with animal and intellectual beings.

Where so many living creatures are to ply their respective powers, in pursuing the end for which they were intended, we are not to look for nature in a quiescent state; matter itself must be in motion, and the scenes of life a continued or repeated series of agitations and events.

This globe of the earth is a habitable world; and on its fitness for this purpose, our sense of wisdom in its formation must depend. To judge of this point, we must keep in view,

not only the end, but the means also by which that end is obtained. These are, the form of the whole, the materials of which it is composed, and the several powers which concur, counteract, or balance one another, in procuring the general result.

The form and constitution of the mass are not more evidently calculated for the purpose of this earth as a habitable world, than are the various substances of which that complicated body is composed. Soft and hard parts variously combine to form a medium consistence, adapted to the use of plants and animals; wet and dry are properly mixed for nutrition, or the support of those growing bodies; and hot and cold produce a temperature or climate no less required than a soil: Insomuch, that there is not any particular, respecting either the qualities of the materials, or the construction of the machine, more obvious to our perception, than are the presence and efficacy of design and intelligence in the power that conducts the work.

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In taking this view of things, where ends and means are made the object of attention, we may hope to find a principle upon which the comparative importance of parts in the system of nature may be estimated, and also a rule for selecting the object of our inquiries. Under this direction, science may find a fit subject of investigation in every particular, whether of *form*, *quality*, or *active power*, that presents itself in this system of motion and of life; and which, without a proper attention to this character of the system, might appear anomalous and incomprehensible.

It is not only by seeing those general operations of the globe which depend upon its peculiar construction as a machine, but also by perceiving how far the particulars, in the construction of that machine, depend upon the general operations of the globe, that we are enabled to understand the constitution of this earth as a thing formed by design. We shall thus also be led to acknowledge an order, not unworthy of Divine wisdom, in a subject which, in another view, has appeared as the work of chance, or as absolute disorder and confusion.

To acquire a general or comprehensive view of this mechanism of the globe, by which it is adapted to the purpose of being a habitable world, it is necessary to distinguish three different bodies which compose the whole. These are, a solid body of earth, an aqueous body of sea, and an elastic fluid of air.

It is the proper shape and disposition of these three bodies that form this globe into a habitable world; and it is the manner in which these constituent bodies are adjusted to each other, and the laws of action by which they are maintained in their proper qualities and respective departments, that form the Theory of the machine which we are now to examine.

Let us begin with some general sketch of the particulars now mentioned.

1st, There is a central body in the globe. This body supports those parts which come to be more immediately exposed to our view, or which may be examined by our sense and observation. This first part is commonly supposed to be solid and inert; but such a conclusion is only mere conjecture; and we shall afterwards find occasion, perhaps, to form another judgment in relation to this subject, after we have examined strictly, upon scientific principles, what appears upon the surface, and have formed conclusions concerning that which must have been transacted in some more central part.

2dly, We find a fluid body of water. This, by gravitation, is reduced to a spherical form, and by the centrifugal force of the earth's rotation, is become oblate. The purpose of this fluid body is essential in the constitution of the world; for, besides affording the means of life and motion to a multifarious race of animals, it is the source of growth and circulation to the organized bodies of this earth, in being the receptacle of the rivers, and the fountain of our vapours.

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3dly, We have an irregular body of land raised above the level of the ocean. This, no doubt, is the smallest portion of the globe; but it is the part to us by far most interesting. It is upon the surface of this part that plants are made to grow; consequently, it is by virtue of this land that animal life, as well as vegetation, is sustained in this world.

Lastly, We have a surrounding body of atmosphere, which completes the globe. This vital fluid is no less necessary, in the constitution of the world, than are the other parts; for there is hardly an operation upon the surface of the earth, that is not conducted or promoted by its means. It is a necessary condition for the sustenance of fire; it is the breath of life to animals; it is at least an instrument in vegetation; and, while it contributes to give fertility and health to things that grow, it is employed in preventing noxious effects from such as go into corruption. In short, it is the proper means of circulation for the matter of this world, by raising up the water of the ocean, and pouring it forth upon the surface of the earth.

Such is the mechanism of the globe: Let us now mention some of those powers by which motion is produced, and activity procured to the mere machine.

First, There is the progressive force, or moving power, by which this planetary body, if solely actuated, would depart continually from the path which it now pursues, and thus be for ever removed from its end, whether as a planetary body, or as a globe sustaining plants and animals, which may be termed a living world.

But this moving body is also actuated by gravitation, which inclines it directly to the central body of the sun. Thus it is made to revolve about that luminary, and to preserve its path.

It is also upon the same principles, that each particular part upon the surface of this globe, is alternately exposed to the influence of light and darkness, in the diurnal rotation of the earth, as well as in its annual revolution. In this manner are produced the vicissitudes of night and day, so variable in the different latitudes from the equator to the pole, and so beautifully calculated to equalise the benefits of light, so variously distributed in the different regions of the globe.

Gravitation, and the *vis infita* of matter, thus form the first two powers distinguishable in the operations of our system, and wisely adapted to the purpose for which they are employed.

We next observe the influence of light and heat, of cold and condensation. It is by means of these two powers that the various operations of this living world are more immediately transacted; although the other powers are no less required, in order to produce or modify these great agents in the economy of life, and system of our changing things.

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We do not now inquire into the nature of those powers, or investigate the laws of light and heat, of cold and condemnation, by which the various purposes of this world are accomplished; we are only to mention those effects which are made sensible to the common understanding of mankind, and which necessarily imply a power that is employed. Thus, it is by the operation of those powers that the varieties of season in spring and autumn are obtained, that we are blessed with the vicissitudes of summer's heat and winter's cold, and that we possess the benefit of artificial light and culinary fire.

We are thus bountifully provided with the necessaries of life; we are supplied with things conducive to the growth and preservation of our animal nature, and with fit subjects to employ and to nourish our intellectual powers.

There are other actuating powers employed in the operations of this globe, which we are little more than able to enumerate; such are those of electricity, magnetism, and subterraneous heat or mineral fire.

Powers of such magnitude or force, are not to be supposed useless in a machine contrived surely not without wisdom; but they are mentioned here chiefly on account of their general effect; and it is sufficient to have named powers, of which the actual existence is well known, but of which the proper use in the constitution of the world is still obscure. The laws of electricity and magnetism have been well examined by philosophers; but the purposes of those powers in the economy of the globe have not been discovered. Subterraneous fire, again, although the most conspicuous in the operations of this world, and often examined by philosophers, is a power which has been still less understood, whether with regard to its efficient or final cause. It has hitherto appeared more like the accident of natural things, than the inherent property of the mineral region. It is in this last light, however, that I wish to exhibit it, as a great power acting a material part in the operations of the globe, and as an essential part in the constitution of this world.

We have thus surveyed the machine in general, with those moving powers, by which its operations, diversified almost *ad infinitum*, are performed. Let us now confine our view, more particularly, to that part of the machine on which we dwell, that so we may consider the natural consequences of those operations which, being within our view, we are better qualified to examine.

This subject is important to the human race, to the possessor of this world, to the intelligent being Man, who foresees events to come, and who, in contemplating his future interest, is led to inquire concerning causes, in order that he may judge of events which otherwise he could not know.

If, in pursuing this object, we employ our skill in research, not in forming vain conjectures; and if *data* are to be found, on which Science may form just conclusions, we should not long remain in ignorance with respect to the natural history of this earth, a

subject on which hitherto opinion only, and not evidence, has decided: For in no subject, perhaps, is there naturally less defect of evidence, although philosophers, led by prejudice, or misguided by false theory, may have neglected to employ that light by which they should have seen the system of this world.

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But to proceed in pursuing a little farther our general or preparatory ideas. A solid body of land could not have answered the purpose of a habitable world; for, a soil is necessary to the growth of plants; and a soil is nothing but the materials collected from the destruction of the solid land. Therefore, the surface of this land, inhabited by man, and covered with plants and animals, is made by nature to decay, in dissolving from that hard and, compact state in which it is found below the soil; and this soil is necessarily washed away, by the continual circulation of the water, running from the summits of the mountains towards the general receptacle of that fluid. The heights of our land are thus levelled with the shores; our fertile plains are formed from the ruins of the mountains; and those travelling materials are still pursued by the moving water, and propelled along the inclined surface of the earth[1] These moveable materials, delivered into the sea, cannot, for a long continuance, rest upon the shore; for, by the agitation of the winds, the tides and currents, every moveable thing is carried farther and farther along the shelving bottom of the sea, towards the unfathomable regions of the ocean.

[Note 1: M. de Luc, in his second letter to me, published in the Monthly Review for 1790, says, “You ought to have proved that both gravel and sand are carried from our continents to the sea; which, on the contrary, I shall prove not to be the case.” He then endeavours to prove his assertion, by observing, that, in certain places where there is not either sufficient declivity in the surface, or force in the running water, gravel and sand are made to rest, and do not travel to the sea. This surely is a fact to which I most readily assent; but, on the other hand, I hope he will acknowledge, that, where there is sufficient declivity in the surface, or force in the running water, sand, gravel, and stones, are travelled upon the land, and are thus carried into the sea—at last. This is all that my theory requires, and this is what I believe will be admitted, without any farther proof on my part.]

If the vegetable soil is thus constantly removed from the surface of the land, and if its place is thus to be supplied from the dissolution of the solid earth, as here represented, we may perceive an end to this beautiful machine; an end, arising from no error in its constitution as a world, but from that destructibility of its land which is so necessary in the system of the globe, in the economy of life and vegetation.

The immense time necessarily required for this total destruction of the land, must not be opposed to that view of future events, which is indicated by the surest facts, and most approved principles. Time, which measures every thing in our idea, and is often deficient to our schemes, is to nature endless and as nothing; it cannot limit that by which alone it had existence; and, as the natural course of time, which to us seems

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infinite, cannot be bounded by any operation that may have an end, the progress of things upon this globe, that is, the course of nature, cannot be limited by time, which must proceed in a continual succession. We are, therefore, to consider as inevitable the deduction of our land, so far as effected by those operations which are necessary in the purpose of the globe, considered as a habitable world; and, so far as we have not examined any other part of the economy of nature, in which other operations and a different intention might appear.

We have now considered the globe of this earth as a machine, constructed upon chemical as well as mechanical principles, by which its different parts are all adapted, in form, in quality, and in quantity, to a certain end; an end attained with certainty or success; and an end from which we may perceive wisdom, in contemplating the means employed.

But is this world to be considered thus merely as a machine, to last no longer than its parts retain their present position, their proper forms and qualities? Or may it not be also considered as an organized body? such as has a constitution in which the necessary decay of the machine is naturally repaired, in the exertion of those productive powers by which it had been formed.

This is the view in which we are now to examine the globe; to see if there be, in the constitution of this world, a reproductive operation, by which a ruined constitution may be again repaired, and a duration or stability thus procured to the machine, considered as a world sustaining plants and animals.

If no such reproductive power, or reforming operation, after due inquiry, is to be found in the constitution of this world, we should have reason to conclude, that the system of this earth has either been intentionally made imperfect, or has not been the work of infinite power and wisdom.

Here is an important question, therefore, with regard to the constitution of this globe; a question which, perhaps, it is in the power of man’s sagacity to resolve; and a question which, if satisfactorily resolved, might add some lustre to science and the human intellect.

Animated with this great, this interesting view, let us strictly examine our principles, in order to avoid fallacy in our reasoning; and let us endeavour to support our attention, in developing a subject that is vast in its extent, as well as intricate in the relation of parts to be stated.

The globe of this earth is evidently made for man. He alone, of all the beings which have life upon this body, enjoys the whole and every part; he alone is capable of

knowing the nature of this world, which he thus possesses in virtue of his proper right; and he alone can make the knowledge of this system a source of pleasure, and the means of happiness.

Man alone, of all the animated beings which enjoy the benefits of this earth, employs the knowledge which he there receives, in leading him to judge of the intention of things, as well as of the means by which they are brought about; and he alone is thus made to enjoy, in contemplation as well as sensual pleasure, all the good that may be observed in the constitution of this world; he, therefore, should be made the first subject of inquiry.

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Now, if we are to take the written history of man for the rule by which we should judge of the time when the species first began, that period would be but little removed from the present state of things. The Mosaic history places this beginning of man at no great distance; and there has not been found, in natural history, any document by which a high antiquity might be attributed to the human race. But this is not the case with regard to the inferior species of animals, particularly those which inhabit the ocean and its shores. We find, in natural history, monuments which prove that those animals had long existed; and we thus procure a measure for the computation of a period of time extremely remote, though far from being precisely ascertained.

In examining things present, we have data from which to reason with regard to what has been; and, from what has actually been, we have data for concluding with regard to that which is to happen hereafter. Therefore, upon the supposition that the operations of nature are equable and steady, we find, in natural appearances, means for concluding a certain portion of time to have necessarily elapsed, in the production of those events of which we see the effects.

It is thus that, in finding the relics of sea-animals of every kind in the solid body of our earth, a natural history of those animals is formed, which includes a certain portion of time; and, for the ascertaining this portion of time, we must again have recourse to the regular operations of this world. We shall thus arrive at facts which indicate a period to which no other species of chronology is able to remount.

In what follows, therefore, we are to examine the construction of the present earth, in order to understand the natural operations of time past; to acquire principles, by which we may conclude with regard to the future course of things, or judge of those operations, by which a world, so wisely ordered, goes into decay; and to learn, by what means such a decayed world may be renovated, or the waste of habitable land upon the globe repaired.

This, therefore, is the object which we are to have in view during this physical investigation; this is the end to which are to be directed all the steps in our cosmological pursuit.

The solid parts of the globe are, in general, composed of sand, of gravel, of argillaceous and calcareous strata, or of the various compositions of these with some other substances, which it is not necessary now to mention. Sand is separated and sized by streams and currents; gravel is formed by the mutual attrition of stones agitated in water; and marly, or argillaceous strata, have been collected, by subsiding in water with which those earthy substances had been floated. Thus, so far as the earth is formed of these materials, that solid body would appear to have been the production of water, winds, and tides.

But that which renders the original of our land clear and evident, is the immense quantities of calcareous bodies which had belonged to animals, and the intimate connection of these masses of animal production with the other strata of the land. For it is to be proved, that all these calcareous bodies, from the collection of which the strata were formed, have belonged to the sea, and were produced in it.

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We find the marks of marine animals in the most solid parts of the earth; consequently, those solid parts have been formed after the ocean was inhabited by those animals which are proper to that fluid medium. If, therefore, we knew the natural history of those solid parts, and could trace the operations of the globe, by which they had been formed, we would have some means for computing the time through which those species of animals have continued to live. But how shall we describe a process which nobody has seen performed, and of which no written history gives any account? This is only to be investigated, *first*, in examining the nature of those solid bodies, the history of which we want to know; and, 2_dly_, In examining the natural operations of the globe, in order to see if there now actually exist such operations, as, from the nature of the solid bodies, appear to have been necessary to their formation.

But, before entering more particularly into those points of discussion, by which the question is to be resolved, let us take a general view of the subject, in order to see what it is which science and observation must decide.

In all the regions of the globe, immense masses are found, which, though at present in the most solid state, appear to have been formed by the collection of the calcareous *exuviae* of marine animals. The question at present is not, in what manner those collections of calcareous relics have become a perfect solid body, and have been changed from an animal to a mineral substance; for this is a subject that will be afterwards considered; we are now only inquiring, if such is truly the origin of those mineral masses.

That all the masses of marble or limestone are composed of the calcareous matter of marine bodies, may be concluded from the following facts:

1_st_, There are few beds of marble or limestone, in which may not be found some of those objects which indicate the marine origin of the mass. If, for example, in a mass of marble, taken from a quarry upon the top of the Alps or Andes[2], there shall be found one cockle-shell, or piece of coral, it must be concluded, that this bed of stone had been originally formed at the bottom of the sea, as much as another bed which is evidently composed almost altogether of cockle-shells and coral. If one bed of limestone is thus found to have been of a marine origin, every concomitant bed of the same kind must be also concluded to have been formed in the same Manner.

[Note 2: “Cette sommite elevee de 984 toises au dessus de notre lac, et par consequent de 1172 au dessus de la mer, est remarquable en ce que l’on y voit des fragmens d’huitres petrifies.—Cette montagne est dominee par un rocher escarpe, qui s’il n’est pas inaccessible, est du moins d’un bien difficile acces; il paroit presqu’entierement compose de coquillages petrifies, renfermes dans un roc calcaire, ou marbre grossier noiratre. Les fragmens qui s’en detachent, et que l’on rencontre en montant a la Croix de fer, sont remplis de *turbinites* de differentes especes.” M. DE SAUSSURE, *Voyage dans les Alpes*, p. 394.]

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We thus shall find the greatest part of the calcareous masses upon this globe to have originated from marine calcareous bodies; for whether we examine marbles, limestones, or such solid masses as are perfectly changed from the state of earth, and are become compact and hard, or whether we examine the soft, earthy, chalky or marly strata, of which so much of this earth is composed, we still find evident proofs, that those beds had their origin from materials deposited at the bottom of the sea; and that they have the calcareous substance which they contain, from the same source as the marbles or the limestones.

2_dly_, In those calcareous strata, which are evidently of marine origin, there are many parts that are of a sparry structure, that is to say, the original texture of those beds, in such places, has been dissolved, and a new structure has been assumed, which is peculiar to a certain state of the calcareous earth. This change is produced by crystallisation, in consequence of a previous state of fluidity, which has so disposed the concreting parts, as to allow them to assume a regular shape and structure proper to that substance. A body, whose external form has been modified by this process, is called a *crystal*; one whose internal arrangement of parts is determined by it, is said to be of a *sparry structure*; and this is known from its fracture.

3_dly_, There are, in all the regions of the earth, huge masses of calcareous matter, in that crystalline form of sparry state, in which perhaps no vestige can be found of any organised body, nor any indication that such calcareous matter had belonged to animals; but as, in other masses, this sparry structure, or crystalline state, is evidently assumed by the marine calcareous substances, in operations which are natural to the globe, and which are necessary to the consolidation of the strata, it does not appear, that the sparry masses, in which no figured body is formed, have been originally different from other masses, which, being only crystallised in part, and in part still retaining their original form, leave ample evidence of their marine origin[3].

[Note 3: M. de Saussure, describing the marble of Aigle, says, “Les tables polies de ce marbre presentent frequemment des coquillages, dont la plupart sont des peignes stries, et de tres-beaux madrepores. Tous ces corps marins on pris entierement la nature et le grain meme du marbre, on n’y voit presque jamais la coquille sous sa forme originaire.”]

We are led, in this manner, to conclude, that all the strata of the earth, not only those consisting of such calcareous masses, but others superincumbent upon these, have had their origin at the bottom of the sea, by the collection of sand and gravel, of shells, of coralline and crustaceous bodies, and of earths and clays, variously mixed, or separated and accumulated. Here is a general conclusion, well authenticated in the appearances of nature, and highly important in the natural history of the earth.

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The general amount of our reasoning is this, that nine-tenths, perhaps, or ninety-nine hundredths of this earth, so far as we see, have been formed by natural operations of the globe, in collecting loose materials, and depositing them at the bottom of the sea; consolidating those collections in various degrees, and either elevating those consolidated masses above the level on which they were formed, or lowering the level of that sea.

There is a part of the solid earth which we may at present neglect, not as being persuaded that this part may not also be found to come under the general rule of formation with the rest, but as considering this part to be of no consequence in forming a general rule, which shall comprehend almost the whole, without doing it absolutely. This excluded part consists of certain mountains and masses of granite. These are thought to be still older in their formation, and are said never to be found superincumbent on strata which must be acknowledged as the productions of the sea.

Having thus found the greater part, if not the whole, of the solid land to have been originally composed at the bottom of the sea, we may now, in order to form a proper idea of these operations, suppose the whole of this seaborn land to be again dispersed along the bottom of the ocean, the surface of which would rise proportionally over the globe. We would thus have a spheroid of water, with granite rocks and islands scattered here and there. But this would not be the world which we inhabit; therefore, the question now is, how such continents, as we actually have upon the globe, could be erected above the level of the sea.

It must be evident, that no motion of the sea, caused by this earth revolving in the solar system, could bring about that end; for let us suppose the axis of the earth to be changed from the present poles, and placed in the equinoctial line, the consequence of this might, indeed, be the formation of a continent of land about each new pole, from whence the sea would run towards the new equator; but all the rest of the globe would remain an ocean. Some new points might be discovered, and others, which before appeared above the surface of the sea, would be sunk by the rising of the water; but, on the whole, land could only be gained substantially at the poles. Such a supposition, as this, if applied to the present state of things, would be destitute of every support, as being incapable of explaining what appears.

But even allowing that, by the changed axis of the earth, or any other operation of the globe, as a planetary body revolving in the solar system, great continents of land could have been erected from the place of their formation, the bottom of the sea, and placed in a higher elevation, compared with the surface of that water, yet such a continent as this could not have continued stationary for many thousand years; nor could a continent of this kind have presented to us, every where within its body, masses of consolidated marble, and other mineral substances, in a state as different as possible from that in which they were, when originally collected together in the sea.

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Consequently, besides an operation, by which the earth at the bottom of the sea should be converted into an elevated land, or placed high above the level of the ocean, there is required, in the operations of the globe, a consolidating power, by which the loose materials that had subsided from water, should be formed into masses of the most perfect solidity, having neither water nor vacuity between their various constituent parts, nor in the pores of those constituent parts themselves.

Here is an operation of the globe, whether chemical or mechanical, which is necessarily connected with the formation of our present continents: Therefore, had we a proper understanding of this secret operation, we might thereby be enabled to form an opinion, with regard to the nature of that unknown power, by which the continents have been placed above the surface of that water wherein they had their birth.

If this consolidating operation be performed at the bottom of the ocean, or under great depths of the earth, of which our continents are composed, we cannot be witnesses to this mineral process, or acquire the knowledge of natural causes, by immediately observing the changes which they produce; but though we have not this immediate observation of those changes of bodies, we have, in science, the means of reasoning from distant events; consequently, of discovering, in the general powers of nature, causes for those events of which we see the effects.

That the consolidating operation, in general, lies out of the reach of our immediate observation, will appear from the following truth: All the consolidated masses, of which we now inquire into the cause, are, upon the surface of the earth, in a state of general decay, although the various natures of those bodies admit of that dissolution in very different degrees[4]

From every view of the subject, therefore, we are directed to look into those consolidated masses themselves, in order to find principles from whence to judge of those operations by which they had attained their hardness or consolidated state.

It must be evident, that nothing but the most general acquaintance with the laws of acting substances, and with those of bodies changing by the powers of nature, can enable us to set about this undertaking with any reasonable prospect of success; and here the science of Chemistry must be brought particularly to our aid; for this science, having for its object the changes produced upon the sensible qualities, as they are called, of bodies, by its means we may be enabled to judge of that which is possible according to the laws of nature, and of that which, in like manner, we must consider as impossible.

[Note 4: Stalactical and certain ferruginous concretions may seem to form an exception to the generality of this proposition. But an objection of this kind could only arise from a partial view of things; for the concretion here is only temporary; it is in consequence of a

solution, and it is to be followed by a dissolution, which will be treated of in its proper place.]

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Whatever conclusions, therefore, by means of this science, shall be attained, in just reasoning from natural appearances, this must be held as evidence, where more immediate proof cannot be obtained; and, in a physical subject, where things actual are concerned, and not the imaginations of the human mind, this proof will be considered as amounting to a demonstration.

SECTION II.

An Investigation of the Natural Operations employed in consolidating the Strata of the Globe.

We are now about to investigate those mineral operations of the globe by which the qualities of hardness and solidity, consequently of strength and durability, are procured to great bodies of this earth.

That those qualities are not original to such bodies, but actually superinduced in the natural operations of the earth, will appear from the examination of some of the hardest and most solid of those mineral bodies. In such masses, (for example of flint and agate,) we find included shells and coralline bodies. Consequently, there must be a natural operation in the globe for consolidating and hardening its soft and loose materials. It is concerning the nature of this consolidating operation that we are now to inquire.

There are just two ways in which porous or spongy bodies can be consolidated, and by which substances may be formed into masses of a natural shape and regular structure; the one of these is simple *congelation* from a fluid state, by means of cold; the other is *accretion*; and this includes a separatory operation, as well as that by which the solid body is to be produced. But in whichever of these ways solidity shall be procured, it must be brought about by first inducing fluidity, either immediately by the action of heat, or mediately with the assistance of a solvent, that is, by the operation of solution. Therefore, fire and water may be considered as the general agents in this operation, which we would explore.

Heat has been already mentioned as a general power, and as acting in all the different parts of the globe; I would now wish more particularly to call the attention of the reader to subterraneous fire, or heat, as a powerful agent in the mineral regions, and as a cause necessarily belonging to the internal constitution of this earth.

It is not our purpose at present to inquire into the particular nature of this power of subterraneous heat, or to trace the proper connection and analogy of the internal fire with that which is so necessary to our life, and which acts so great a part upon the surface of the earth, this being reserved for the last part. Our intention in here mentioning it, is only to dispose the mind to look for active powers or efficient causes, in

that part of the earth which has been commonly considered as passive and inert, but which will be found extremely active, and the source of mighty revolutions in the fate of land.

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There may, indeed, be some difficulty in conceiving all the modifications of this mineral power; but as, on the one hand, we are not arbitrarily to assume an agent, for the purpose of explaining events, or certain appearances which are not understood; so, on the other, we must not refuse to admit the action of a known power, when this is properly suggested in the appearances of things; and, though we may not understand all the modifications, or the whole capacity and regulation of this power in bodies, we are not to neglect the appropriating to it, as a cause, those effects which are natural to it, and which, so far as we know, cannot belong to any other. On all occasions, we are to judge from what we know; and, we are only to avoid concluding from our suppositions, in cases where evidence or real information is necessarily required. The subject now considered, subterraneous fire, will afford an example of that truth; and, a general view of this great natural power will here find a proper place, before the application of it for the explanation of natural appearances.

No event is more the object of our notice, or more interesting as a subject for our study, than is the burning of a fire: But, the more that philosophers have studied this subject, the more they seem to differ as to the manner in which that conspicuous event is to be explained. Therefore, being so ignorant with regard to that fire of which we see the origin as well as the more immediate effects, how cautious should we be in judging the nature of subterraneous fire from the burning of bodies, a subject which we so little understand.

But, though the cause of fire in general, or the operations of that power in its extreme degrees, be for us a subject involved in much obscurity, this is not the case with regard to the more common effects of heat; and, tho' the actual existence of subterraneous fire, as the cause of light and heat, might be a thing altogether problematical in our opinion; yet, as to other effects, there are some of these from which the action of that liquefying power may be certainly concluded as having taken place within the mineral region, although the cause should be in every other respect a thing to us unknown. In that case, where the operation or effect is evident, and cannot be disputed, to refuse to admit the power in question, merely because we had not seen it act, or because we know not every rule which it may observe in acting, would be only to found an argument upon our ignorance; it would be to misunderstand the nature of investigating physical truths, which must proceed by reasoning from effect to cause.

Our knowledge is extremely limited with regard to the effects of heat in bodies, while acting under different conditions, and in various degrees. But though our knowledge in these respects is limited, our judgment with regard to the efficacy of this power of heat is in its nature positive, and contains not any thing that is doubtful or uncertain. All mankind, who have the opportunity, know that the hard substance of ice is by heat converted into water, wherein no hardness remains; and the profound philosophy of Dr Black, in relation to the subject of *latent heat*, as that of Sir Isaac Newton, in relation to the weight of bodies, is not necessary to convince the world that in the one case ice will melt, and in the other, that heavy bodies will move when unsupported.

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But though, in the abstract doctrine of *latent heat*, the ingenuity of man has discovered a certain measure for the quantity of those commutable effects which are perceived; and though this be a progress of science far above the apprehension of the vulgar, yet still, that solid bodies are changed into fluids, by the power of heat, is the same unalterable judgment, which the savage forms as well as the philosopher. Here, therefore, are evident effects, which mankind in general attribute to the power of heat; and it is from those known effects that we are to investigate subterraneous fire, or to generalise the power of heat, as acting in the interior parts, as well as on the surface of this earth.

If, indeed, there were any other cause for fluidity besides the operation of fire or the power of heat, in that case the most evident proof, with regard to the flowing, or former fluidity, of mineral bodies, would draw to no conclusion in proving the existence of mineral fire; but when we have not the smallest reason for conjecturing any other cause, or the least doubt with regard to that which, in the doctrine of latent heat, has been properly investigated, the proofs which we shall bring, of fusion in all the minerals of this earth, must be held as proofs of mineral fire, in like manner as the proof of subterraneous fire would necessarily imply mineral fusion as its natural effect.

Thus we have, in our physical investigation, several points in view. First, from the present state of things, to infer a former state of fusion among mineral bodies. Secondly, from that former fusion, to infer the actual existence of mineral fire in the system of the earth. And, lastly, from the acknowledged fact of subterraneous fire as a cause, to reason with regard to the effects of that power in mineral bodies.

But besides the power or effect of subterraneous heat in bodies which are unorganised, and without system, in the construction of their different parts, we have to investigate the proper purpose of this great agent in the system of this world, which may be considered as a species of organised body. Here, therefore, final causes are to be brought into view, as well as those which are efficient. Now, in a subject involved with so much obscurity, as must be for us the internal regions of the globe, the consideration of efficient and final causes may contribute mutually to each others evidence, when separately the investigation of either might be thought unsatisfactory or insufficient.

So far it seemed necessary to premise with regard to the great mineral power which we are to employ as an agent in the system of this earth; and it may be now observed, that it is in the proper relation of this power of heat and the fluidity or softness of bodies, as cause and effect, that we are to find a physical principle or argument for detecting those false theories of the earth that have been only imagined, and not properly founded on fact or observation. It is also by means of this principle, that we shall be enabled to form a true theory of the mineral region, in generalising particular effects to a common cause.

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Let us now proceed in endeavouring to decide this important question, *viz*. By what active principle is it, that the present state of things, which we observe in the strata of the earth, a state so very different from that in which those bodies had been formed originally, has been brought about?

Two causes have been now proposed for the consolidating of loose materials which had been in an incoherent state; these are, on the one hand, fire; or, on the other, water, as the means of bringing about that event. We are, therefore, to consider well, what may be the consequences of consolidation by the one or other of those agents; and what may be the respective powers of those agents with respect to this operation.

If we are not informed in this branch of science, we may gaze without instruction upon the most convincing proofs of what we want to attain. If our knowledge is imperfect, we may form erroneous principles, and deceive ourselves in reasoning with regard to those works of nature, which are wisely calculated for our instruction.

The strata, formed at the bottom of the sea, are to be considered as having been consolidated, either by aqueous solution and crystallization, or by the effect of heat and fusion. If it is in the first of these two ways that the solid strata of the globe have attained to their present state, there will be a certain uniformity observable in the effects; and there will be general laws, by which this operation must have been conducted. Therefore, knowing those general laws, and making just observations with regard to the natural appearances of those consolidated masses, a philosopher, in his closet, should be able to determine, what may, and what may not have been transacted in the bowels of the earth, or below the bottom of the ocean.

Let us now endeavour to ascertain what may have been the power of water, acting under fixed circumstances, operating upon known substances, and conducting to a certain end.

The action of water upon all different substances is an operation with which we are familiar. We have it in our power to apply water in different degrees of heat for the solution of bodies, and under various degrees of compression; consequently, there is no reason to conclude any thing mysterious in the operations of the globe, which are to be performed by means of water, unless an immense compressing power should alter the nature of those operations. But compression alters the relation of evaporation only with regard to heat, or it changes the degree of heat which water may be made to sustain; consequently, we are to look for no occult quality in water acting upon bodies at the bottom of the deepest ocean, more than what can be observed in experiments which we have it in our power to try.

With regard again to the effect of time: Though the continuance of time may do much in those operations which are extremely slow, where no change, to our observation, had appeared to take place, yet, where it is not in the nature of things to produce the change

in question, the unlimited course of time would be no more effectual, than the moment by which we measure events in our observations.

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Water being the general medium in which bodies collected at the bottom of the sea are always contained, if those masses of collected matter are to be consolidated by solution, it must be by the dissolution of those bodies in that water as a menstruum, and by the concretion or crystallization of this dissolved matter, that the spaces, first occupied by water in those masses, are afterwards to be filled with a hard and solid substance; but without some other power, by which the water contained in those cavities and endless labyrinths of the strata, should be separated in proportion as it had performed its task, it is inconceivable how those masses, however changed from the state of their first subsidence, should be absolutely consolidated, without any visible or fluid water in their composition.

Besides this difficulty of having the water separated from the porous masses which are to be consolidated, there is another with which, upon this supposition, we have to struggle. This is, From whence should come the matter with which the numberless cavities in those masses are to be filled?

The water in the cavities and interstices of those bodies composing strata, must be in a stagnating state; consequently, it can only act upon the surfaces of those cavities which are to be filled up. But with what are they to be filled? Not with water; they are full of that already: Not with the substance of the bodies which contain that water; this would be only to make one cavity in order to fill up another. If, therefore, the cavities of the strata are to be filled with solid matter, by means of water, there must be made to pass through those porous masses, water impregnated with some other substances in a dissolved state; and the aqueous menstruum must be made to separate from the dissolved substance, and to deposit the same in those cavities through which the solution moves.

By such a supposition as this, we might perhaps explain a partial consolidation of those strata; but this is a supposition, of which the case under consideration does not admit; for in the present case, which is that of materials accumulated at the bottom of the ocean, there is not proper means for separating the dissolved matter from the water included in those enormous masses; nor are there any means by which a circulation in those masses may be formed. In this case, therefore, where the means are not naturally in the supposition, a philosopher, who is to explain the phenomenon by the natural operation of water in this situation, must not have recourse to another agent, still more powerful, to assist his supposition which cannot be admitted.

Thus, it will appear, that, to consolidate strata formed at the bottom of the sea, in the manner now considered, operations are required unnatural to this place; consequently, not to be supposed, in order to support a hypothesis.

But now, instead of inquiring how far water may be supposed instrumental in the consolidation of strata which were originally of a loose texture, we are to consider how far there may be appearances in those consolidated bodies, by which it might be

concluded, whether or not the present state of their consolidation has been actually brought about by means of that agent.

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If water had been the menstruum by which the consolidating matter was introduced into the interstices of strata, masses of those bodies could only be found consolidated with such substances as water is capable of dissolving; and these substances would be found only in such a state as the simple separation of the solvent water might produce.

In this case, the consolidation of strata would be extremely limited; for we cannot allow more power to water than we find it has in nature; nor are we to imagine to ourselves unlimited powers in bodies, on purpose to explain those appearances by which we should be made to know the powers of nature. Let us, therefore, attend, with every possible circumspection, to the appearances of those bodies, by means of which we are to investigate the principles of mineralogy, and know the laws of nature.

The question now before us concerns the consolidating substances of strata. Are these such as will correspond to the dissolving power of water, and to the state in which these substances might be left by the separation of their menstruum? No; far, far from this supposition is the conclusion that necessarily follows from natural appearances.

We have strata consolidated by calcareous spar, a thing perfectly distinguishable from the stalactical concretion of calcareous earth, in consequence of aqueous solution. We have strata made solid by the formation of fluor, a substance not soluble, so far as we know, by water. We have strata consolidated with sulphureous and bituminous substances, which do not correspond to the solution of water. We have strata consolidated with siliceous matter, in a state different from that under which it has been observed, on certain occasions, to be deposited by water. We have strata consolidated by feld-spar, a substance insoluble in water. We have strata consolidated by almost all the various metallic substances, with their almost endless mixtures and sulphureous compositions; that is to say, we find, perhaps, every different substance introduced into the interstices of strata which had been formed by subsidence at the bottom of the sea.

If it is by means of water that those interstices have been filled with those materials, water must be, like fire, an universal solvent, or cause of fluidity, and we must change entirely our opinion of water in relation to its chemical character. But there is no necessity thus to violate our chemical principles, in order to explain certain natural appearances; more especially if those appearances may be explained in another manner, consistently with the known laws of nature.

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If, again, it is by means of heat and fusion that the loose and porous structure of strata shall be supposed to have been consolidated, then every difficulty which had occurred in reasoning upon the power or agency of water is at once removed. The loose and discontinuous body of a stratum may be closed by means of softness and compression; the porous structure of the materials may be consolidated, in a similar manner, by the fusion of their substance; and foreign matter may be introduced into the open structure of strata, in form of steam or exhalation, as well as in the fluid state of fusion; consequently, heat is an agent competent for the consolidation of strata, which water alone is not. If, therefore, such an agent could be found acting in the natural place of strata, we must pronounce it proper to bring about that end.

The examination of nature gives countenance to this supposition, so far as strata are found consolidated by every species of substance, and almost every possible mixture of those different substances; consequently, however difficult it may appear to have this application of heat, for the purpose of consolidating strata formed at the bottom of the ocean, we cannot, from natural appearances, suppose any other cause, as having actually produced the effects which are now examined.

This question, with regard to the means of consolidating the strata of the globe, is, to natural history, of the greatest importance; and it is essential in the theory now proposed to be given of the mineral system. It would, therefore, require to be discussed with some degree of precision in examining the particulars; but of these, there is so great a field, and the subject is so complicated in its nature, that volumes might be written upon particular branches only, without exhausting what might be laid upon the subject; because the evidence, though strong in many particulars, is chiefly to be enforced by a multitude of facts, conspiring, in a diversity of ways, to point out one truth, and by the impossibility of reconciling all these facts, except by means of one supposition.

But, as it is necessary to give some proof of that which is to be a principle in our reasoning afterwards, I shall now endeavour to generalise the subject as much as possible, in order to answer that end, and, at the same time, to point out the particular method of inquiry.

There are to be found, among the various strata of the globe, bodies formed of two different kinds of substances, *siliceous* bodies, and those which may be termed *sulphureous* or *phlogistic*. With one or other, or both of those we substances, every different consolidated stratum of the globe will be found so intimately mixed, or closely connected, that it must be concluded, by whatever cause those bodies of siliceous and sulphureous matter had been changed from a fluid to a concreted state, the strata must have been similarly affected by the same cause.

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These two species of bodies, therefore, the siliceous and the sulphureous, may now be examined, in relation to the causes of their concretion, with a view to determine, what has been the general concreting or consolidating power, which has operated universally in the globe; and particularly to show, it has not been by means of any fluid solution, that strata in general have been consolidated, or that those particular substances have been crystallized and concreted.

Siliceous matter, physically speaking, is not soluble in water; that is to say, in no manner of way have we been enabled to learn, that water has the power of dissolving this matter.

Many other substances, which are so little soluble in water, that their solubility could not be otherwise detected of themselves, are made to appear soluble by means of siliceous matter; such is feld-spar, one of the component parts of rock-granite.

Feld-spar is a compound of siliceous, argillaceous, and calcareous earth, intimately united together. This compound siliceous body being, for ages, exposed to the weather, the calcareous part of it is dissolved, and the siliceous part is left in form of a soft white earth. But whether this dissolution is performed by pure water, or by means also of an acid, may perhaps be questioned. This, however, is certain, that we must consider siliceous substances as insoluble in water.

The water of Glezer in Iceland undoubtedly contains this substance in solution; but there is no reason to believe, that it is here dissolved by any other than the natural means; that is, an alkaline substance, by which siliceous bodies may be rendered soluble in water[5].

[Note 5: This conjecture, which I had thus formed, has been fully confirmed by the accurate analysis of those waters. See vol. 3d. of the Phil. Trans. of Edin.]

It may be, therefore, asserted, that no siliceous body having the hardness of flint, nor any crystallization of that substance, has ever been formed, except by fusion. If, by any art, this substance shall be dissolved in simple water, or made to crystallise from any solution, in that case, the assertion which has been here made may be denied. But where there is not the vestige of any proof, to authorise the supposition of flinty matter being dissolved by water, or crystallized from that solution, such an hypothesis cannot be admitted, in opposition to general and evident appearances[6].

[Note 6: The Chevalier de Dolomieu has imagined an ingenious theory for the solution of siliceous substances in water [Journal de Physique, Mai 1792.]. This theory has not been taken up merely at a venture, but is founded upon very accurate and interesting chemical experiments. Hitherto, however, the nature of the siliceous substance is not sufficiently known, to enable us to found, upon chemical principles, the mineral operations of nature. That siliceous substance may be dissolved, or rendered soluble in

water, by means of alkaline salt, and that it may be also volatilised by means of the fluor acid, is almost all that we know upon the subject. But this is saying no more in relation to the mineral operations employed upon the siliceous substance, than it would be, in relation to those upon gold, to say that this metal is dissolved by aqua regia.

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It is to be admitted, that every simple substance may have its menstruum, by means of which it may be retained with water in a dissolved state; but from this it does not follow, that it is by the means of aqueous solutions of all those mineral bodies, that nature operates the consolidation of bodies, which we find actually accomplished with all those different substances. It is the business of this work to show, that from all appearances in the mineral regions, as well as those upon the surface in the atmosphere, the supposition, of that manner of consolidating bodies by solution, is inconsistent both with natural appearances, and also with chemical principles.

Our ingenious author, who has, with, great diligence as well as an enlightened mind, observed the operations of nature upon the surface of the earth, here says, “ce n’est pas sans etonnement que je remarque depuis long-temps que jamais aucune eau qui coule a la surface de la terre n’attaque le quartz, aucune n’en tient en dissolution, pendant que celles qui circulent interieurement le corrodent aussi souvent qu’elles le deposent.”—How dangerous it is in science for ingenious men to allow themselves to form conclusions, which the principles on which they reason do not strictly warrant, we have a remarkable example in the present case.

M. de Dolomieu sees no corrosion of quartz, or solution of that substance, upon the surface of the earth; from this, then, he concludes, that siliceous substance is not dissolved in that situation of things. On the other hand, he finds siliceous bodies variously concreted among the solid strata of the earth; and, from this he concludes, that siliceous substance has been both dissolved by water in the strata, and also there again concreted and crystallised in having been separated from the water. This is certainly what we all perceive; but we do not all allow ourselves to draw such inconclusive inferences from our premises. Notwithstanding the greatest accuracy of our observations, quartz may be dissolvable in a minute degree by water, upon the surface of this earth; and, all the appearances of siliceous bodies, in the mineral regions, where we cannot immediately see the operation, may be better explained by fusion than by aqueous solution.

But, from his chemical experiments, our author has conjectured that there may be a phlogistic substance, by means of which the siliceous earth is dissolved when in darkness; and that this solvent loses its power, if exposed to the light of day. I have one observation to oppose to this ingenious theory. Under deep black mosses, through which no ray of light can penetrate, every condition for dissolving siliceous bodies should be found, according to the supposition in question; neither will sufficient time be found wanting, in those deep mosses, upon the summits of our mountains; yet, examine the matter of fact? not the smallest solution is to be perceived in the siliceous parts of the stones which are found under those mosses, but every particle of iron is dissolved, so that the surface of every stone is white, and nothing but the siliceous earth of the feld-spar, and perhaps the argillaceous, is left.

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Here we have in this author an instructive example: No person, in my opinion, has made such enlightened or scientific experiments, or such judicious observations with regard to the nature of siliceous substance, as a compound thing; no person reasons more distinctly in general, or sees more clearly the importance of his principles; yet, with regard to mineral concretions, how often has he been drawn thus inadvertently into improper generalization! I appeal to the analogy which, in this treatise, he has formed, between the stalactical concretions upon the surface of the earth, and the mineral concretions of siliceous substance. As an example of the great lights, and penetrating genius, of this assiduous studier of nature, I refer to the judicious observations which he has made upon the subject of aluminous earth, in this dissertation.

I am surprised to find this enlightened naturalist seeking, in the origin of this globe of our earth, a general principle of fluidity or solution in water, like the alkahest of the alchymists, by means of which the different substances in the chemical constitution of precious stones might have been united as well as crystallised. One would have thought, that a philosopher, so conversant in the operations of subterraneous fire, would have perceived, that there is but one general principle of fluidity or dissolution, and that this is heat.]

Besides this proof for the fusion of siliceous bodies, which is indirect, arising from the in dissolubility of that substance in water, there is another, which is more direct, being founded upon appearances which are plainly inconsistent with any other supposition, except that of simple fluidity induced by heat. The proof I mean is, the penetration of many bodies with a flinty substance, which, according to every collateral circumstance, must have been performed by the flinty matter in a simply fluid state, and not in a state of dissolution by a solvent.

These are flinty bodies perfectly insulated in strata both of chalk and sand. It requires but inspection to be convinced. It is not possible that flinty matter could be conveyed into the middle of those strata, by a menstruum in which it was dissolved, and thus deposited in that place, without the smallest trace of deposition in the surrounding parts.

But, besides this argument taken from what does not appear, the actual form in which those flinty masses are found, demonstrates, *first*, That they have been introduced among those strata in a fluid state, by injection from some other place. 2_dly_, That they have been dispersed in a variety of ways among those strata, then deeply immersed at the bottom of the sea; and, *lastly*, That they have been there congealed from the state of fusion, and have remained in that situation, while those strata have been removed from the bottom of the ocean to the surface of the present land.

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To describe those particular appearances would draw this paper beyond the bounds of an essay. We must, therefore, refer those who would inquire more minutely into the subject, to examine the chalk-countries of France and England, in which the flint is found variously formed; the land-hills interspersed among those chalk-countries, which have been also injected by melted flint; and the pudding-stone of England, which I have not seen in its natural situation. More particularly, I would recommend an examination of the insulated masses of stone, found in the sand-hills by the city of Brussels; a stone which is formed by an injection of flint among sand, similar to that which, in a body of gravel, had formed the pudding-stone of England[7].

[Note 7: Accurate descriptions of those appearances, with drawings, would be, to natural history, a valuable acquisition.]

All these examples would require to be examined upon the spot, as a great part of the proof for the fusion of the flinty substance, arises, in my opinion, from the form in which those bodies are found, and the state of the surrounding parts. But there are specimens brought from many different places, which contain, in themselves, the most evident marks of this injection of the flinty substance in a fluid state. These are pieces of fossil wood, penetrated with a siliceous substance, which are brought from England, Germany, and Lochneagh in Ireland.

It appears from these specimens, that there has sometimes been a prior penetration of the body of wood, either with irony matter, or calcareous substance. Sometimes, again, which is the case with that of Lochneagh, there does not seem to have been any penetration of those two substances. The injected flint appears to have penetrated the body of this wood, immersed at the bottom of the sea, under an immense compression of water. This appears from the wood being penetrated partially, some parts not being penetrated at all.

Now, in the limits between those two parts, we have the most convincing proofs, that it had been flint in a simple fluid state which had penetrated the wood, and not in a state of solution.

First, Because, however little of the wood is left unpenetrated, the division is always distinct between the injected part and that which is not penetrated by the fluid flint. In this case, the flinty matter has proceeded a certain length, which is marked, and no farther; and, beyond this boundary, there is no partial impregnation, nor a gradation of the flintifying operation, as must have been the case if siliceous matter had been deposited from a solution. 2_dly_, The termination of the flinty impregnation has assumed such a form, precisely, as would naturally happen from a fluid flint penetrating that body.

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In other specimens of this mineralising operation, fossil wood, penetrated, more or less, with ferruginous and calcareous substances, has been afterwards penetrated with a flinty substance. In this case, with whatever different substances the woody body shall be supposed to have been penetrated in a state of solution by water, the regular structure of the plant would still have remained, with its vacuities, variously filled with the petrifying substances, separated from the aqueous menstruum, and deposited in the vascular structure of the wood. There cannot be a doubt with regard to the truth of this proposition; for, as it is, we frequently find parts of the consolidated wood, with the vascular structure remaining perfectly in its natural shape and situation; but if it had been by aqueous solution that the wood had been penetrated and consolidated, all the parts of that body would be found in the same natural shape and situation.

This, however, is far from being the case; for while, in some parts, the vascular structure is preserved entire, it is also evident, that, in general, the woody structure is variously broken and dissolved by the fusion and crystallization of the flint. There are so many and such various convincing examples of this, that, to attempt to describe them, would be to exceed the bounds prescribed for this dissertation; but such specimens are in my possession, ready for the inspection of any person who may desire to study the subject.

We may now proceed to consider sulphureous substances, with regard to their solubility in water, and to the part which these bodies have acted in consolidating the strata of the globe.

The sulphureous substances here meant to be considered, are substances not soluble in, water, so far as we know, but fusible by heat, and inflammable or combustible by means of heat and vital air. These substances are of two kinds; the one more simple, the other more compound.

The most simple kind is composed of two different substances, *viz*. phlogiston, with certain specific substances; from which result, on the one hand, sulphur, and, on the other, proper coal and metals. The more compound sort, again, is oily matter, produced by vegetables, and forming bituminous bodies.

The *first* of these is found naturally combined with almost all metallic substances, which are then said to be mineralised with sulphur. Now, it is well known, that this mineralising operation is performed by means of heat or fusion; and there is no person skilled in chemistry that will pretend to say, this may be done by aqueous solution. The combination of iron and sulphur, for example, may easily be performed by fusion; but, by aqueous solution, this particular combination is again resolved, and forms an acido-metallic, that is, a vitriolic substance, after the phlogiston (by means of which it is insoluble in water) has been separated from the composition, by the assistance of vital air.

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The variety of these sulphureo-metallic substances, in point of composition, is almost indefinite; but, unless they were all soluble in water, this could not have happened by the action of that solvent. If we shall allow any one of those bodies to have been formed by the fluidity of heat, they must all have been formed in the same manner; for there is such a chain of connection among those bodies in the mineral regions, that they must all have been composed, either, on the one hand, by aqueous solution, or, on the other, by means of heat and fusion.

Here, for example, are crystallised together in one mass, 1_st, Pyrites_, containing sulphur, iron, copper; 2_dly, Blend_, a composition of iron, sulphur, and calamine; 3_dly, Galena_, consisting of lead and sulphur; 4_thly, Marmor metallicum_, being the terra ponderosa, saturated with the vitriolic acid; a substance insoluble in water; 5_thly, Fluor_, a saturation of calcareous earth, with a peculiar acid, called the *acid of spar*, also insoluble in water; 6_thly, Calcareous spar_, of different kinds, being calcareous earth saturated with fixed air, and something besides, which forms a variety in this substance; *lastly, Siliceous substance*, or *Quartz crystals*. All these bodies, each possessing its proper shape, are mixed in such a manner as it would be endless to describe, but which may be expressed in general by saying, that they are mutually contained in, and contain each other.

Unless, therefore; every one of these different substances may be dissolved in water, and crystallised from it, it is in vain to look for the explanation of these appearances in the operations of nature, by the means of aqueous solution.

On the other hand, heat being capable of rendering all these substances fluid, they may be, with the greatest simplicity, transported from one place to another; and they may be made to concrete altogether at the same time, and distinctly separate in any place. Hence, for the explanation of those natural appearances, which are so general, no further conditions are required, than the supposition of a sufficient intensity of subterraneous fire or heat, and a sufficient degree of compression upon those bodies, which are to be subjected to that violent heat, without calcination or change. But, so far as this supposition is not gratuitous, the appearances of nature will be thus explained.

I shall only mention one specimen, which must appear most decisive of the question. It is, I believe, from an Hungarian mine. In this specimen, petro-silex, pyrites, and cinnabar, are so mixed together, and crystallised upon each other, that it is impossible to conceive any one of those bodies to have had its fluidity and concretion from a cause which had not affected the other two. Now, let those who would deny the fusion of this siliceous body explain how water could dissolve these three different bodies, and deposit them in their present shape. If, on the contrary, they have not the least shadow of reason for such a gratuitous supposition, the present argument must be admitted in its full force.

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Sulphur and metals are commonly found combined in the mineral regions. But this rule is not universal; for they are also frequently in a separate state. There is not, perhaps, a metal, among the great number which are now discovered, that may not be found native, as they are called, or in their metallic state.

Metallic substances are also thus found in some proportion to the disposition of the particular metals, to resist the mineralising operations, and to their facility of being metallised by fire and fusion. Gold, which refuses to be mineralised with sulphur, is found generally in its native state. Iron, again, which is so easily mineralised and scorified, is seldom found in its malleable state. The other metals are all found more or less mineralised, though some of them but rarely in the native state.

Besides being found with circumstances thus corresponding to the natural facility, or to the impediments attending the metallization of those different calces, the native metals are also found in such a shape, and with such marks, as can only agree with the fusion of those bodies; that is to say, those appearances are perfectly irreconcilable with any manner of solution and precipitation.

For the truth of this assertion, among a thousand other examples, I appeal to that famous mass of native iron discovered by Mr Pallas in Siberia. This mass being so well known to all the mineralists of Europe, any comment upon its shape and structure will be unnecessary[8].

[Note 8: Since this Dissertation was written, M. de la Peyrouse has discovered a native manganese. The circumstances of this mineral are so well adapted for illustrating the present doctrine, and so well related by M. de la Peyrouse, that I should be wanting to the interest of mineral knowledge, were I not to give here that part of his Memoir.

“Lorsque je fis inserer dans le journal de physique de l’annee 1780, au mois de Janvier, une Dissertation contenant la classification des mines de manganese, je ne connoissois point, a cette epoque, la mine de manganese native. Elle a la couleur de son regule: Elle salit les doigts de la meme teinte. Son tissu parait aussi lamelleux, et les lames semblent affecter une sorte de divergence. Elle a ainsi que lui, l’eclat metallique; comme lui elle se laisse aplatir sous le marteau, et s’exfolie si l’on redouble les coups; mais une circonstance qui est trop frappante pour que je l’omette, c’est la figure de la manganese native, si prodigieusement conforme a celle du regule, qu’on s’y laisseroit tromper, si la mine n’etoit encore dans sa gangue: Figure tres-essentielle a observer ici, parce qu’elle est due a la nature meme de la manganese. En effet, pour reduire toutes les mines en general, il faut employer divers flux appropries. Pour la reduction de la manganese, bien loin d’user de ce moyen, il faut, au contraire, eloigner tout flux, produire la fusion, par la seule violence et la promptitude

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du feu. Et telle est la propension naturelle et prodigieuse de la manganese a la vitrification, qu'on n'a pu parvenir encore a reduire son regule en un seul culot; on trouve dans le creuset plusieurs petits boutons, qui forment autant de culots separes. Dans la mine de manganese native, elle n'est point en une seule masse; elle est disposee egalement en plusieurs culots separes, et un peu aplatis, comme ceux que l'art produit; beaucoup plus gros, a la verite, parce que les agens de la nature doivent avoir une autre energie, que ceux de nos laboratoires; et cette ressemblance si exacte, semble devoir vous faire penser que la mine native a ete produite par le feu, tout comme son regule. La presence de la chaux argentee de la manganese, me permettroit de croire que la nature n'a fait que reduire cette chaux. Du reste, cette mine native est tres-pure, et ne contient aucune partie attirable a l'aimant. Cette mine, unique jusqu'a ce moment, vient, tout comme les autres manganese que j'ai decrites, des mines de fer de *Sem*, dans la vallee de *Viedersos*, en Comte de Foix."—*Journal de Physique, Janvier 1786*.]

We come now to the *second* species of inflammable bodies called oily or bituminous. These substances are also found variously mixed with mineral bodies, as well as forming strata of themselves; they are, therefore, a proper subject for a particular examination.

In the process of vegetation, there are produced oily and resinous substances; and, from the collection of these substances at the bottom of the ocean, there are formed strata, which have afterwards undergone various degrees of beat, and have been variously changed, in consequence of the effects of that heat, according as the distillation of the more volatile parts of those bodies has been suffered to proceed.

In order to understand this, it must be considered, that, while immersed in water, and under insuperable compression, the vegetable, oily, and resinous substances, would appear to be unalterable by heat; and it is only in proportion as certain chemical separations take place, that these inflammable bodies are changed in their substance by the application of heat. Now, the most general change of this kind is in consequence of evaporation, or the distillation of their more volatile parts, by which oily substances become bituminous, and bituminous substances become coaly.

There is here a gradation which may be best understood, by comparing the extremes.

On the one hand, we know by experiment, that oily and bituminous substances can be melted and partly changed into vapour by heat, and that they become harder and denser, in proportion as the more volatile parts have evaporated from them. On the other hand, coaly substances are destitute of fusibility and volatility, in proportion as they have been exposed to greater degrees of heat, and to other circumstances favourable to the dissipation of their more volatile and fluid parts.

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If, therefore, in mineral bodies, we find the two extreme states of this combustible substance, and also the intermediate states, we must either conclude, that this particular operation of heat has been thus actually employed in nature, or we must explain those appearances by some other means, in as satisfactory a manner, and so as shall be consistent with other appearances.

In this case, it will avail nothing to have recourse to the false analogy of water dissolving and crystallising salts, which has been so much employed for the explanation of other mineral appearances. The operation here in question is of a different nature, and necessarily requires both the powers of heat and proper conditions for evaporation.

Therefore, in order to decide the point, with regard to what is the power in nature by which mineral bodies have become solid, we have but to find bituminous substance in the most complete state of coal, intimately connected with some other substance, which is more generally found consolidating the strata, and assisting in the concretion of mineral substances. But I have in my possession the most undoubted proof of this kind. It is a mineral vein, or cavity, in which are blended together coal of the most fixed kind, quartz and marmor metallicum. Nor is this all; for the specimen now referred to is contained in a rock of this kind, which every naturalist now-a-days will allow to have congealed from a fluid state of fusion. I have also similar specimens from the same place, in which the coal is not of that fixed and infusible kind which burns without flame or smoke, but is bituminous or inflammable coal.

We have hitherto been resting the argument upon a single point, for the sake of simplicity or clearness, not for want of those circumstances which shall be found to corroborate the theory. The strata of fossil coal are found in almost every intermediate state, as well as in those of bitumen and charcoal. Of the one kind is that fossil coal which melts or becomes fluid upon receiving heat; of the other, is that species of coal, found both in Wales and Scotland, which is perfectly infusible in the fire, and burns like coals, without flame or smoke. The one species abounds in oily matter, the other has been distilled by heat, until it has become a *caput mortuum*, or perfect coal.

The more volatile parts of these bituminous bodies are found in their separate state on some occasions. There is a stratum of limestone in Fifeshire, near Raith, which, though but slightly tinged with a black colour, contains bituminous matter, like pitch, in many cavities, which are lined with calcareous spar crystallised. I have a specimen of such a cavity, in which the bitumen is in sphericles, or rounded drops, immersed in the calcareous spar.

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Now, it is to be observed, that, if the cavity in the solid limestone or marble, which is lined with calcareous crystals containing pyrites, had been thus encrusted by means of the filtration of water, this water must have dissolved calcareous spar, pyrites, and bitumen. But these natural appearances would not even be explained by this dissolution and supposed filtration of those substances. There is also required, *first*, A cause for the separation of those different substances from the aqueous menstruum in which they had been dissolved; *2dly*, An explanation of the way in which a dissolved bitumen should be formed into round hard bodies of the most solid structure; and, *lastly*, Some probable means for this complicated operation being performed, below the bottom of the ocean, in the close cavity of a marble stratum.

Thus, the additional proof, from the facts relating to the bituminous substances, conspiring with that from the phenomena of other bodies, affords the strongest corroboration of this opinion, that the various concretions found in the internal parts of strata have not been occasioned by means of aqueous solution, but by the power of heat and operation of simple fusion, preparing those different substances to concrete and crystallise in cooling.

The arguments which have been now employed for proving that strata have been consolidated by the power of heat, or by the means of fusion, have been drawn chiefly from the insoluble nature of those consolidating substances in relation to water, which is the only general menstruum that can be allowed for the mineral regions. But there are found, in the mineral kingdom, many solid masses of saltgem, which is a soluble substance. It may be now inquired, How far these masses, which are not infrequent in the earth, tend either to confirm the present theory, or, on the contrary, to give countenance to that which supposes water the chief instrument in consolidating strata.

The formation of salt at the bottom of the sea, without the assistance of subterranean fire, is not a thing unsupposable, as at first sight it might appear. Let us but suppose a rock placed across the gut of Gibraltar, (a case nowise unnatural), and the bottom of the Mediterranean would be certainly filled with salt, because the evaporation from the surface of that sea exceeds the measure of its supply.

But strata of salt, formed in this manner at the bottom of the sea, are as far from being consolidated by means of aqueous solution, as a bed of sand in the same situation; and we cannot explain the consolidation of such a stratum of salt by means of water, without supposing subterranean heat employed, to evaporate the brine which would successively occupy the interstices of the saline crystals. But this, it may be observed, is equally departing from the natural operation of water, as the means for consolidating the sediment of the ocean, as if we were to suppose the same thing done by heat and fusion. For the question is not, If subterranean heat be of sufficient intensity for the purpose of consolidating strata by the fusion of their substances; the question is, Whether it be by means of this agent, subterranean heat, or by water alone, without the operation of a melting heat, that those materials have been variously consolidated.

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The example now under consideration, consolidated mineral salt, will serve to throw some light upon the subject; for, as it is to be shown, that this body of salt had been consolidated by perfect fusion, and not by means of aqueous solution, the consolidation of strata of indissoluble substances, by the operation of a melting heat, will meet with all that confirmation which the consistency of natural appearances can give.

The salt rock in Cheshire lies in strata of red marl. It is horizontal in its direction. I do not know its thickness, but it is dug thirty or forty feet deep. The body of this rock is perfectly solid, and the salt, in many places, pure, colourless, and transparent, breaking with a sparry cubical structure. But the greatest part is tinged by the admixture of the marl, and that in various degrees, from the slightest tinge of red, to the most perfect opacity. Thus, the rock appears as if it had been a mass of fluid salt, in which had been floating a quantity of marly substance, not uniformly mixed, but every where separating and subsiding from the pure saline substance.

There is also to be observed a certain regularity in this separation of the tinging from the colourless substance, which, at a proper distance, gives to the perpendicular section of the rock a distinguishable figure in its structure. When looking at this appearance near the bottom of the rock, it, at first, presented me with the figure of regular stratification; but, upon examining the whole mass of rock, I found, that it was only towards the bottom that this stratified appearance took place; and that, at the top of the rock, the most beautiful and regular figure was to be observed; but a figure the most opposite to that of stratification. It was all composed of concentric circles; and these appeared to be the section of a mass, composed altogether of concentric spheres, like those beautiful systems of configuration which agates so frequently present us with in miniature. In about eight or ten feet from the top, the circles growing large, were blended together, and gradually lost their regular appearance, until, at a greater depth, they again appeared in resemblance of a stratification.

This regular arrangement of the floating marly substance in the body of salt, which is that of the structure of a coated pebble, or that of concentric spheres, is altogether inexplicable upon any other supposition, than the perfect fluidity or fusion of the salt, and the attractions and repulsions of the contained substances. It is in vain to look, in the operations of solution and evaporation, for that which nothing but perfect fluidity or fusion can explain.

This example of a mineral salt congealed from a melted state, may be confirmed from another which I have from Dr Black, who suggested it to me. It is an alkaline salt, found in a mineral state, and described in the Philosophical Transactions, *anno* 1771. But to understand this specimen, something must be premised with regard to the nature of fossil alkali.

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The fossil alkali crystallises from a dissolved state, in combining itself with a large portion of the water, in the manner of alum; and, in this case, the water is essential to the constitution of that transparent crystalline body; for, upon the evaporation of the water, the transparent salt loses its solidity, and becomes a white powder. If, instead of being gently dried, the crystalline salt is suddenly exposed to a sufficient degree of heat, that is, somewhat more than boiling water, it enters into the state of aqueous fusion, and it boils, in emitting the water by means of which it had been crystallised in the cold, and rendered fluid in that heated state. It is not possible to crystallise this alkaline salt from a dissolved state, without the combination of that quantity of water, nor to separate that water without destroying its crystalline state.

But in this mineral specimen, we have a solid crystalline salt, with a structure which, upon fracture, appears to be sparry and radiated, something resembling that of zeolite. It contains no water in its crystallization, but melts in a sufficient heat, without any aqueous fusion. Therefore, this salt must have been in a fluid state of fusion, immediately before its congelation and crystallization.

It would be endless to give examples of particular facts, so many are the different natural appearances that occur, attended with a variety of different circumstances.

There is one, however, which is peculiarly distinct, admits of sufficiently accurate description, and contains circumstances from which conclusions may be drawn with clearness. This is the ironstone, which is commonly found among the argillaceous strata, attendant upon fossil coal, both in Scotland and in England.

This stone is generally found among the bituminous schistus, or black argillaceous strata, either in separate masses of various shapes and sizes, or forming of itself strata which are more or less continuous in their direction among the schistous or argillaceous beds.

This mineral contains, in general, from 40 to 50 *per cent.* of iron, and it loses near one third of its weight in calcination. Before calcination it is of a grey colour, is not penetrable by water, and takes a polish. In this state, therefore, it is perfectly solid; but being calcined, it becomes red, porous, and tender.

The fact to be proved with regard to these iron-stones is this, That they have acquired their solid state from fusion, and not in concreting from any aqueous solution.

To abridge this disquisition, no argument is to be taken from contingent circumstances, (which, however, are often found here as well as in the case of marbles); such only are to be employed as are general to the subject, and arise necessarily from the nature of the operation.

It will be proper to describe a species of these stones, which is remarkably regular in its form. It is that found at Aberlady, in East Lothian.

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The form of these iron-stones is that of an oblate or much compressed sphere, and the size from two or three inches diameter to more than a foot. In the circular or horizontal section, they present the most elegant septarium[9]; and, from the examination of this particular structure, the following conclusions may be drawn.

First, That, the septa have been formed by the uniform contraction of the internal parts of the stone, the volume of the central parts diminishing more than that of the circumference; by this means, the separations of the stone diminish, in a progression from the center towards the circumference.

2d, That there are only two ways in which the septa must have received the spar or spatthose ore with which they are filled, more or less, either, *first* By insinuation into the cavity of the septa after these were formed; or, *2dly*, By separation from the substance of the stone, at the same time that the septa were forming.

[Note 9: Plate I.]

Were the first supposition true, appearances would be observable, showing that the sparry substance had been admitted, either through the porous structure of the stone, or through proper apertures communicating from without. Now, if either one or other of these had been the case, and that the stone had been consolidated from no other cause than concretion from a dissolved state, that particular structure of the stone, by means of which the spar had been admitted, must appear at present upon an accurate examination.

This, however, is not the case, and we may rest the argument here. The septa reach not the circumference; the surface of the stone is solid and uniform in every part; and there is not any appearance of the spar in the argillaceous bed around the stone.

It, therefore, necessarily follows, that the contraction of the iron-stone, in order to form septa, and the filling of these cavities with spar, had proceeded *pari passu*; and that this operation must have been brought about by means of fusion, or by congelation from a state of simple fluidity and expansion.

It is only further to be observed, that all the arguments which have been already employed, concerning mineral concretions from a simply fluid state, or that of fusion, here take place. I have septaria of this kind, in which, besides pyrites, iron-ore, calcareous spar, and another that is ferruginous and compound, there is contained siliceous crystals; a case which is not so common. I have them also attended with circumstances of concretion and crystallization, which, besides being extremely rare, are equally curious and interesting.

There is one fact more which is well worth our attention, being one of those which are so general in the mineral regions. It is the crystallizations which are found in close cavities of the most solid bodies.

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Nothing is more common than this appearance. Cavities are every where found closely lined with crystallizations, of every different substance which may be supposed in those places. These concretions are well known to naturalists, and form part of the beautiful specimens which are preserved in the cabinets of collectors, and which the German mineralists have termed *Drusen*. I shall only particularise one species, which may be described upon principle, and therefore may be a proper subject on which to reason, for ascertaining the order of production in certain bodies. This body, which we are now to examine, is of the agate species.

We have now been considering the means employed by nature in consolidating strata which were originally of an open structure; but in perfectly solid strata we find bodies of agate, which have evidently been formed in that place where they now are found. This fact, however, is not still that of which we are now particularly to inquire; for this, of which we are to treat, concerns only a cavity within this agate; now, whatever may have been the origin of the agate itself, we are to show, from what appears within its cavity, that the crystallizations which are found in this place had arisen from a simply fluid state, and not from that of any manner of solution.

The agates now in question are those of the coated kind, so frequent in this country, called pebbles. Many of these are filled with a siliceous crystallization, which evidently proceeds from the circumference towards the centre. Many of them, again, are hollow. Those cavities are variously lined with crystallized substances; and these are the object of the present examination.

But before describing what is found within, it is necessary to attend to this particular circumstance, that the cavity is perfectly inclosed with many solid coats, impervious to air or water, but particularly with the external cortical part, which is extremely hard, takes the highest polish, and is of the most perfect solidity, admitting the passage of nothing but light and heat.

Within these cavities, we find, *1st*, The coat of crystals with which this cavity is always lined; and this is general to all substances concreting, in similar circumstances, from a state of fusion; for when thus at liberty they naturally crystallise. *2dly*, We have frequently a subsequent crystallization, resting on the first, and more or less immersed in it. *3dly*, There is also sometimes a third crystallization, superincumbent on the second, in like manner as the second was on the first. I shall mention some particulars.

I have one specimen, in which the primary crystals are siliceous, the secondary thin foliaceous crystals of deep red but transparent iron-ore, forming elegant figures, that have the form of roses. The tertiary crystallization is a frosting of small siliceous crystals upon the edges of the foliaceous crystals.

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In other specimens, there is first a lining of colourless siliceous crystals, then another lining of amethystine crystals, and sometimes within that, fuliginous crystals. Upon these fuliginous and amethystine crystals are many sphericles or hemispheres of red compact iron-ore, like haematites.

In others, again, the primary crystals are siliceous, and the secondary calcareous. Of this kind, I have one which has, upon the calcareous crystals, beautiful transparent siliceous crystals, and iron sphericles both upon all these crystals, and within them.

Lastly, I have an agate formed of various red and white coats, and beautifully figured. The cavity within the coated part of the pebble is filled up without vacuity, first, with colourless siliceous crystals; secondly, with fuliginous crystals; and, lastly, with white or colourless calcareous spar. But between the spar and crystals there are many sphericles, seemingly of iron, half sunk into each of these two different substances.

From these facts, I may now be allowed to draw the following conclusions:

1_st_, That concretion had proceeded from the surface of the agate body inwards. This necessarily follows from the nature of those figured bodies, the figures of the external coats always determining the shape of those within, and never, contrarily, those within affecting those without.

2_dly_, That when the agate was formed, the cavity then contained every thing which now is found within it, and nothing more.

3_dly_, That the contained substances must have been in a fluid state, in order to their crystallizing.

Lastly, That as this fluid state had not been the effect of solution in a menstruum, it must have been fluidity from heat and fusion.

Let us now make one general observation and argument with regard to the formation of those various coated, concreted, crystallized, and configured bodies. Were the crystallization and configuration found to proceed from a central body, and to be directed from that centre outwards, then, without inquiring into collateral appearances, and other proofs with regard to the natural concretion of those substances, we might suppose that these concretions might have proceeded from that central body gradually by accretion, and that the concreting and crystallizing substances might have been supplied from a fluid which had before retained the concreting substance in solution; in like manner as the crystallizations of sugar, which are formed in the solution of that saccharine substance, and are termed candies, are formed upon the threads which are extended in the crystallizing vessel for that purpose. But if, on the contrary, we are to consider those mineral bodies as spheres of alternate coats, composed of agate, crystal, spars, *etc*.; and if all those crystallizations have their *bases* upon the

uncrystallized coat which is immediately external to it, and their *apices* turned inwards into the next internal solid coat, it is not possible to conceive that a structure of this kind could have been formed in any manner from a solution. But this last manner is the way without exception in which those mineral bodies are found; therefore we are to conclude, that the concretion of those bodies had proceeded immediately from a state of fusion or simple fluidity.

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In granite these cavities are commonly lined with the crystal corresponding to the constituent substances of the stone, *viz*. quartz, feld-spar, and mica or talk. M. de Saussure, (Voyages dans les Alpes, tom. ii. sec. 722.), says, “On trouve frequemment des amas considerables de spath calcaire, crystallise dans les grottes ou se forme le crystal de roche; quoique ces grottes soient renfermees dans le coeur des montagnes d’un granit vif, & qu’on ne voie aucun roc calcaire au dessus de ces montagnes.”

So accurate an observer, and so complete a naturalist, must have observed how the extraneous substance had been introduced into this cavity, had they not been formed together the cavity and the calcareous crystals. That M. de Saussure perceived no means for that introduction, will appear from what immediately follows in that paragraph. “Ces rocs auroient-ils ete detruits, ou bien ce spath n’est il que le produit d’une secretion des parties calcaires que l’on fait etres dispersees entre les divers elemens du granit?”

Had M. de Saussure allowed himself to suppose all those substances in fusion, of which there cannot be a doubt, he would soon have resolved both this difficulty, and also that of finding molybdena crystallized along with feld-spar, in a cavity of this kind. sec. 718.

To this argument, taken from the close cavities in our agates, I am now to add another demonstration. It is the case of the calcedony agate, containing a body of calcareous spar; here it is to be shown, that, while the calcareous body was altogether inclosed within the calcedony nodular body, these two substances had been perfectly soft, and had mutually affected each others shape, in concreting from a fluid state. In order to see this, we are to consider that both those substances have specific shapes in which they concrete from the third state; the sparry structure of the one is well known; the spherical or mammelated crystallization of the calcedony, is no less conspicuous; this last is, in the present case, spherical figures, which are some of them hemispheres, or even more. The figures which we have now in contemplation are so distinctly different as cannot be mistaken; the one is a rhombic figure bounded by planes; the other is a most perfect spherical form; and both these are specific figures, belonging respectively to the crystallization of those two substances.

The argument now to be employed for proving that those two bodies had concreted from the fluid state of fusion, and not from any manner of solution, is this: That, were the one of those bodies to be found impressing the other with its specific figure, we must conclude that the impressing body had concreted or crystallized while the impressed body was in a soft or fluid state; and that, if they are both found mutually impressing and impressed by each other, they must have both been in the fluid and concreting state together. Now the fact is, that the calcareous body is perfectly

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inclosed within the solid calcedony, and that they are mutually impressed by each others specific figure, the sparry structure of the calcareous body impressing the calcedony with its type of planes and angles, at the same time that, in other parts, the spherical figures of the calcedony enter the solid body of the spar, and thus impress their mammelated figures into that part which is contiguous. It is therefore inconceivable, that these appearances could have been produced in any other manner than by those two bodies concreting from a simply fluid state.

There are in jaspers and agates many other appearances, from whence the fusion of those substances may be concluded with great certainty and precision; but it is hoped, that what has been now given may suffice for establishing that proposition without any doubt.

It must not be here objected, That there are frequently found siliceous crystals and amethysts containing water; and that it is impossible to confine water even in melted glass. It is true, that here, at the surface of the earth, melted glass cannot, in ordinary circumstances, be made to receive and inclose condensed water; but let us only suppose a sufficient degree of compression in the body of melted glass, and we can easily imagine it to receive and confine water as well as any other substance. But if, even in our operations, water, by means of compression, may be made to endure the heat of red hot iron without being converted into vapour, what may not the power of nature be able to perform? The place of mineral operations is not on the surface of the earth; and we are not to limit nature with our imbecility, or estimate the powers of nature by the measure of our own.[10]

[Note 10: This is so material a principle in the theory of consolidating the strata of the earth by the fusion of mineral substances, that I beg the particular attention of the reader to that subject. The effect of compression upon compound substances, submitted to increased degrees of heat, is not a matter of supposition, it is an established principle in natural philosophy. This, like every other physical principle, is founded upon matter of fact or experience; we find, that many compound substances may with heat be easily changed, by having their more volatile parts separated when under a small compression; but these substances are preserved without change when sufficiently compressed. Our experiments of this kind are necessarily extremely limited; they are not, however, for that reason, the less conclusive. The effects of increasing degrees of heat are certainly prevented by increasing degrees of compression; but the rate at which the different effects of those powers proceed, or the measure of those different degrees of increase that may be made without changing the constitution of the compound substance, are not known; nor is there any limit to be set to that operation, so far as we know. Consequently, it is a physical principle, That the evaporation of volatile substances by heat, or the reparation of them from a compound substance,

consequently the effect of fire in changing that compound substance, may be absolutely prevented by means of compression.

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It now remains to be considered, how far there is reason to conclude that there had been sufficient degrees of compression in the mineral regions, for the purpose of melting the various substances with which we find strata consolidated, without changing the chemical constitution of those compound substances.

Had I, in reasoning *a priori*, asserted, That all mineral bodies might have been melted without change, when under sufficient compression, there might have arisen, in the minds of reasoning men, some doubt with regard to the certainty of that proposition, however probable it were to be esteemed: But when, in reasoning *a posteriori*, it is found that all mineral bodies have been actually melted, then, all that is required to establish the proposition on which I have founded my theory, is to see that there must have been immense degrees of compression upon the subjects in question; for we neither know the degree of heat which had been employed, nor that of compression by which the effect of the heat must have been modified.

Now, in order to see that there had been immense compression, we have but to consider that the formation of the strata, which are to be consolidated, was at the bottom of the ocean, and that this place is to us unfathomable. If it be farther necessary to show that it had been at such unfathomable depth strata were consolidated, it will be sufficient to observe, it is not upon the surface of the earth, or above the level of the sea, that this mineral operation can take place; for, it is there that those consolidated bodies are redissolved, or necessarily going into decay, which is the opposite to that operation which we are now inquiring after; therefore, if they were consolidated in any other place than at the bottom of the sea, it must have been between that place of their formation and the surface of the sea; but that is a supposition which we have not any reason to make; therefore, we must conclude that it was at the bottom of the ocean those stratified bodies had been consolidated.]

To conclude this long chemico-mineral disquisition, I have specimens in which the mixture of calcareous, siliceous, and metallic substances, in almost every species of concretion which is to be found in mineral bodies, may be observed, and in which there is exhibited, in miniature, almost every species of mineral transaction, which, in nature, is found upon a scale of grandeur and magnificence. They are nodules contained in the whin-stone, porphyry, or basaltes of the Calton-hill, by Edinburgh; a body which is to be afterwards examined, when it will be found to have flowed, and to have been in fusion, by the operation of subterraneous heat.

This evidence, though most conclusive with regard to the application of subterraneous heat, as the means employed in bringing into fusion all the different substances with which strata may be found consolidated, is not directly a proof that strata had been consolidated by the fusion of their proper substance. It was necessary to see the general nature of the evidence, for the universal application of subterraneous heat, in the fusion of every kind of mineral body. Now, that this has been done, we may give

examples of strata consolidated without the introduction of foreign matter, merely by the softening or fusion of their own materials.

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For this purpose, we may consider two different species of strata, such as are perfectly simple in their nature, of the most distinct substances, and whose origin is perfectly understood, consequently, whose subsequent changes may be reasoned upon with certainty and clearness. These are the siliceous and calcareous strata; and these are the two prevailing substances of the globe, all the rest being, in comparison of these, as nothing; for unless it be the bituminous or coal strata, there is hardly any other which does not necessarily contain more or less of one or other of these two substances. If, therefore, it can be shown, that both of those two general strata have been consolidated by the simple fusion of their substance, no *desideratum* or doubt will remain, with regard to the nature of that operation which has been transacted at great depths of the earth, places to which all access is denied to mortal eyes.

We are now to prove, *first*, That those strata have been consolidated by simple fusion; and, *2dly*, That this operation is universal, in relation to the strata of the earth, as having produced the various degrees of solidity or hardness in these bodies.

I shall first remark, that a fortuitous collection of hard bodies, such as gravel and sand, can only touch in points, and cannot, while in that hard state, be made to correspond so precisely to each others shape as to consolidate the mass. But if these hard bodies should be softened in their substance, or brought into a certain degree of fusion, they might be adapted mutually to each other, and thus consolidate the open structure of the mass. Therefore, to prove the present point, we have but to exhibit specimens of siliceous and calcareous strata which have been evidently consolidated in this manner.

Of the first kind, great varieties occur in this country. It is, therefore, needless to describe these particularly. They are the consolidated strata of gravel and sand, often containing abundance of feld-spar, and thus graduating into granite; a body, in this respect, perfectly similar to the more regular strata which we now examine.

The second kind, again, are not so common in this country, unless we consider the shells and coralline bodies in our lime-stones, as exhibiting the same example, which indeed they do. But I have a specimen of marble from Spain, which may be described, and which will afford the most satisfactory evidence of the fact in question.

This Spanish marble may be considered as a species of pudding-stone, being formed of calcareous gravel; a species of marble which, from Mr Bowles' Natural History, appears to be very common in Spain. The gravel of which this marble is composed, consists of fragments of other marbles of different kinds. Among these, are different species of *oolites* marble, some shell marbles, and some composed of a chalky substance, or of undistinguishable parts. But it appears, that all these different marbles had been consolidated or made hard, then broken into fragments, rolled and worn by attrition, and thus collected together, along with some sand or small siliceous bodies, into one mass. Lastly, This compound body is consolidated in such a manner as to give the most distinct evidence, that this had been executed by the operation of heat or simple fusion.

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The proof I give is this, That besides the general conformation of those hard bodies, so as to be perfectly adapted to each other's shape, there is, in some places, a mutual indentation of the different pieces of gravel into each other; an indentation which resembles perfectly that junction of the different bones of the *cranium*, called sutures, and which must have necessarily required a mixture of those bodies while in a soft or fluid state.

This appearance of indentation is by no means singular, or limited to one particular specimen. I have several specimens of different marbles, in which fine examples of this species of mixture may be perceived. But in this particular case of the Spanish pudding-stone, where the mutual indentation is made between two pieces of hard stone, worn round by attrition, the softening or fusion of these two bodies is not simply rendered probable, but demonstrated.

Having thus proved, that those strata had been consolidated by simple fusion, as proposed, we now proceed to show, that this mineral operation had been not only general, as being found in all the regions of the globe, but universal, in consolidating our earth in all the various degrees, from loose and incoherent shells and sand, to the most solid bodies of the siliceous and calcareous substances.

To exemplify this in the various collections and mixtures of sands, gravels, shells, and corals, were endless and superfluous. I shall only take, for an example, one simple homogeneous body, in order to exhibit it in the various degrees of consolidation, from the state of simple incoherent earth to that of the most solid marble. It must be evident that this is chalk; naturally a soft calcareous earth, but which may be also found consolidated in every different degree.

Through the middle of the Isle of Wight, there runs a ridge of hills of indurated chalk. This ridge runs from the Isle of Wight directly west into Dorsetshire, and goes by Corscastle towards Dorchester, perhaps beyond that place. The sea has broke through this ridge at the west end of the Isle of Wight, where columns of the indurated chalk remain, called the Needles; the same appearance being found upon the opposite shore in Dorsetshire.

In this field of chalk, we find every gradation of that soft earthy substance to the most consolidated body of this indurated ridge, which is not solid marble, but which has lost its chalky property, and has acquired a kind of stony hardness.

We want only further to see this cretaceous substance in its most indurated and consolidated state; and this we have in the north of Ireland, not far from the Giants Causeway. I have examined cargoes of this lime-stone brought to the west of Scotland, and find the most perfect evidence of this body having been once a mass of chalk, which is now a solid marble.

Thus, if it is by means of fusion that the strata of the earth have been, in many places, consolidated, we must conclude, that all the degrees of consolidation, which are indefinite, have been brought about by the same means.

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Now, that all the strata of the mineral regions, which are those only now examined, have been consolidated in some degree, is a fact for which no proof can be offered here, but must be submitted to experience and inquiry; so far, however, as they shall be considered as consolidated in any degree, which they certainly are in general, we have investigated the means which had been employed in that mineral operation.

We have now considered the concretions of particular bodies, and the general consolidation of strata; but it may be alleged, that there is a great part of the solid mass of this earth not properly comprehended among those bodies which have been thus proved to be consolidated by means of fusion. The body here alluded to is granite; a mass which is not generally stratified, and which, being a body perfectly solid, and forming some part in the structure of this earth, deserves to be considered.

The nature of granite, as a part of the structure of the earth, is too intricate a subject to be here considered, where we only seek to prove the fusion of a substance from the evident marks which are to be observed in a body. We shall, therefore, only now consider one particular species of granite; and if this shall appear to have been in a fluid state of fusion, we may be allowed to extend this property to all the kind.

The species now to be examined comes from the north country, about four or five miles west from Portfoy, on the road to Huntly. I have not been upon the spot, but am informed that this rock is immediately connected or continuous with the common granite of the country. This indeed appears in the specimens which I have got; for, in some of these, there is to be perceived a gradation from the regular to the irregular sort.

This rock may indeed be considered, in some respects, as a porphyry; for it has an evident ground, which is feld-spar, in its sparry state; and it is, in one view, distinctly maculated with quartz, which is transparent, but somewhat dark-coloured[11].

[Note 11: Plate II. fig. 1. 2. 3.]

Considered as a porphyry, this specimen is no less singular than as a granite. For, instead of a siliceous ground, maculated with the rhombic feld-spar, which is the common state of porphyry, the ground is uniformly crystallised, or a homogeneous regular feld-spar, maculated with the transparent siliceous substance. But as, besides the feld-spar and quartz, which are the constituent parts of the stone, there is also mica, in some places, it may, with propriety, be termed a granite.

The singularity of this specimen consists, not in the nature or proportions of its constituent parts, but in the uniformity of the sparry ground, and the regular shape of the quartz mixture. This siliceous substance, viewed in one direction, or longitudinally, may be considered as columnar, prismatical, or continued in lines running nearly parallel. These columnar bodies of quartz are beautifully impressed with a figure on the sides, where they are in contact with the spar. This figure is that of furrows or channels, which

are perfectly parallel, and run across the longitudinal direction of the quartz. This is represented in fig. 4. This striated figure is only seen when, by fracture, the quartz is separated from the contiguous spar.

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But what I would here more particularly represent is, the transverse section of those longitudinal siliceous bodies These are seen in fig. 1. 2. and 3. They have not only separately the forms of certain typographic characters, but collectively give the regular lineal appearance of types set in writing.

It is evident from the inspection of this fossil, that the sparry and siliceous substances had been mixed together in a fluid state; and that the crystallization of the sparry substance, which is rhombic, had determined the regular structure of the quartz, at least in some directions.

Thus, the siliceous substance is to be considered as included in the spar, and as figured, according to the laws of crystallization proper to the sparry ground; but the spar is also to be found included in the quartz. It is not, indeed, always perfectly included or inclosed on all sides; but this is sometimes the case, or it appears so in the section. Fig. 5. 6. 7. 8. 9. and 10. are those cases magnified, and represent the different figured quartz inclosing the feld-spar. In one of them, the feld-spar, which is contained within the quartz, contains also a small triangle of quartz, which it incloses. Now, it is not possible to conceive any other way in which those two substances, quartz and feld-spar, could be thus concreted, except by congelation from a fluid state, in which they had been mixed.

There is one thing more to be observed with regard to this curious species of granite. It is the different order or arrangement of the crystallization or internal structure of the feld-spar ground, in two contiguous parts of the same mass. This is to be perceived in the polished surface of the stone, by means of the reflection of light.

There is a certain direction in which, viewing the stone, when the light falls with a proper obliquity, we see a luminous reflection from the internal parts of the stone. This arises from the reflecting surfaces of the sparry structure or minute cracks, all turned in one direction, consequently, giving that luminous appearance only in one point of view.

Now, all the parts of the stone in which the figured quartz is directed in the same manner, or regularly placed in relation to each other, present that shining appearance to the eye at one time, or in the same point of direction. But there are parts of the mass, which, though immediately contiguous and properly continuous, have a different disposition of the figured quartz; and these two distinguished masses, in the same surface of the polished stone, give to the eye their shining appearance in very different directions. Fig. 3. shows two of those figured and shining masses, in the same plane or polished surface.

It must be evident, that, as the crystallization of the sparry structure is the figuring cause of the quartz bodies, there must be observed a certain correspondency between those two things, the alinement (if I may be allowed the expression) of the quartz, and the shining of the sparry ground. It must also appear, that at the time of congelation of the

fluid spar, those two contiguous portions had been differently disposed in the crystallization of their substance. This is an observation which I have had frequent opportunities of making, with respect to masses of calcareous spar.

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Upon the whole, therefore, whether we shall consider granite as a stratum or as an irregular mass, whether as a collection of several materials, or as the separation of substances which had been mixed, there is sufficient evidence of this body having been consolidated by means of fusion, and in no other manner.

We are thus led to suppose, that the power of heat and operation of fusion must have been employed in consolidating strata of loose materials, which had been collected together and amassed at the bottom of the ocean. It will, therefore, be proper to consider, what are the appearances in consolidated strata that naturally should follow, on the one hand, from fluidity having been, in this manner, introduced by means of heat, and, on the other, from the interstices being filled by means of solution; that so we may compare appearances with the one and other of those two suppositions, in order to know that with which they may be only found consistent.

The consolidation of strata with every different kind of substance was found to be inconsistent with the supposition, that aqueous solution had been the means employed for this purpose. This appearance, on the contrary, is perfectly consistent with the idea, that the fluidity of these bodies had been the effect of heat; for, whether we suppose the introduction of foreign matter into the porous mass of a stratum for its consolidation, or whether we shall suppose the materials of the mass acquiring a degree of softness, by means of which, together with an immense compression, the porous body might be rendered solid; the power of heat, as the cause of fluidity and vapour, is equally proper and perfectly competent. Here, therefore, appearances are as decidedly in favour of the last supposition, as they had been inconsistent with the first.

But if strata have been consolidated by means of aqueous solution, these masses should be found precisely in the same state as when they were originally deposited from the water. The perpendicular section of those masses might show the compression of the bodies included in them, or of which they are composed; but the horizontal section could not contain any separation of the parts of the stratum from one another.

If, again, strata have been consolidated by means of heat, acting in such a manner as to soften their substance, then, in cooling, they must have formed rents or separations of their substance, by the unequal degrees of contraction which the contiguous strata may have suffered. Here is a most decisive mark by which the present question must be determined.

There is not in nature any appearance more distinct than this of the perpendicular fissures and separations in strata. These are generally known to workmen by the terms of veins or backs and cutters; and there is no consolidated stratum that wants these appearances. Here is, therefore, a clear decision of the question, Whether it has been by means of heat, or by means of aqueous solution, that collections of loose bodies at the bottom of the sea have been consolidated into the hardest rocks and most perfect marbles[12].

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[Note 12: This subject is extremely interesting, both to the theory of the earth, and to the science cf the mining art; I will now illustrate that theory, with an authority which I received after giving this dissertation to the Royal Society. It is in the second volume of M. de Saussure's *voyages dans les Alpes*. Here I find proper examples for illustrating that subject of mineralogy; and I am happy to have this opportunity of giving the reasoning of a man of science upon the subject, and the opinion of a person who is in every respect so well qualified to judge upon a point of this kind.

The first example is of a marble in the Alps, (*voyages dans les Alpes.*) tom. 2. page 271.

"La pate de ces breches est tantot blanche, tantot grise, et les fragmens qui y font renfermes font, les uns blancs, les autres gris, d'autres roux, et presque toujours d'une couleur differente de celle de la pate qui les lit. Ils sont tous de nature calcaire; tels etaient au moins tous ceux que j'ai pus observer; et ce qu'il-y-a de remarquable, c'est qu'ils sont tous poses dans le sens des feuillets de la pierre; on diroit en les voyant, qu'ils ont tous ete comprimes et ecrases dans le meme sens. Cette meme pierre est melee de mica, sur-tout dans les interstices des couches et entre les fragmens et la pate qui les reunit; mais on ne voit point de mica dans les fragmens eux-memes. On trouve aussi dans ces breches des infiltrations de quartz. Cette pierre est coupee par des frequentes fissures perpendiculaires aux plans des couches. On voit clairement que ces fentes out ete formees par l'inegal affaissement des couches, et non par une retraite spontanee: car les morceaux ou fragmens etrangers sont tous partages et coupes net par ces fissures au lieu que dans les divisions naturelles des couches, ces memes fragmens sont entiers et saillans au dehors de la surface. Les noeuds de quartz et les divers crystaux, que renferment les roches feuilletees, presentent le meme phenomene, et l'on peut en tirer la meme consequence; ils font partages dans les fentes, et entiers dans les separations des couches."

He finds those particular strata in the other side of the mountain *col de la Seigne*, and gives us the following observations:

"Plus bas on passe entre deux bancs de ces memes breches, entre lesquels sont interposees des couches d'ardoises noires et de gres feuilletes micaces, dont la situation est la meme.

"On retrouve encore ces breches vers le has de la descente, au pied de pyramides calcaires dont j'ai parle plus haut. Je trouvai en 1774 de tres-jolis crystaux de roche qui s'etaient formes dans les fentes de cette breche. Il y avoit meme un melange de quartz et de mica qui s'etoit moule dans quelques-une de ces fentes. C'etoit donc une roche semblable aux primitives, et pourtant d'une formation posterieure a celle de la pierre calcaire. Et quel systeme pourroit nous persuader que la nature ne puisse encore produire ce qu'elle a produit autrefois!"

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M. de Saussure has here given us an example of a calcareous Braccia, as he calls it, but which is rather a pudding stone, with veins or contractions of the mass. He does not seem to understand these as consequences of the consolidation of those strata; this, however, is the only light in which these appearances may be explained, when those bodies are thus divided without any other separation in the mass.

The second example is found in the vertical strata of those mountains through which the Rhone has made its way in running from the great valley of the *Vallais* towards the lake of Geneva. (Chapitre xlviii.)

“C’est une espece de petrosilex gris, dur, sonore, un peu transparent, qui se debite en feuillets minces parfaitement plans et reguliers. Ces feuillets, ou plutot ces couches, courent a 35 degres du nord par est, en montant du cote de l’ouest sous un angle de 80 degres. Ces couches sont coupees par des fentes qui leur sont a-peu-pres perpendiculaires et qui le sont aussi a l’horizon. Cette pierre s’emploie aux memes usage que l’ardoise, mais elle est beaucoup plus forte et plus durable, parce qu’elle est plus dure et moins accessible aux impressions de l’eau et de l’air.

Sec. 1047. “Ces petrosilex feuilletes changent peu-a-peu de nature, en admettant dans les interstices de leurs feuillets des parties de feldspath. Ils out alors l’apparence d’une roche feuilletee, quartzeuse et micacee, (*quartzum fornacum W.*). Mais cette apparence est trompeuse; car on n’y trouve pas un atome de quartz: toutes les parties blanches qui donnent du feu contre l’acier, font du feldspath; et les parties grise ecailleuses ne font point du mica, ce sont de lames minces du petrosilex dont j’ai deja parle.”

Here is evidently what I would call petuntze strata, or porcelane stone, that is, strata formed by the deposits of such materials as might come from the *detritus* of granite, arranged at the bottom of the sea, and consolidated by heat in the mineral regions. We have precisely such stratified masses in the Pentland hills near Edinburgh. I have also a specimen of the same kind, brought from the East Indies, in which there is the print of an organized body. I believe it to be of some coralline or zoophite.

Sec. 1048. “Cette roche melangee continue jusqu’a ce que le rocher s’eloigne un peu du grand chemin. La, ce rocher se presente coupe a pic dans une grande etendue, et divise par de grandes fentes obliques, a-peu-pres paralleles entr’elles. Ces fentes partagent la montagne en grandes tranches de 50 a 60 pieds d’epaisseur, que de loin semblent etre des couches. Mais lorsqu’on s’en approche, on voit, par le tissu meme de la pierre feuilletee, que ses vraies couches font avec l’horizon des angles de 70 a 75 degre, et que ces grandes divisions sont de vraies fentes par lesquelles un grand nombre de couches consecutives sont coupees presque perpendiculairement a leurs plans. Les masses de rocher, comprises entre ces grandes fentes, sont encore divisees par d’autres fentes plus petites, dont la plupart sont paralleles aux grandes,

d’autres leur sont obliques; mais toutes sont a tres-peu-pres perpendiculaires aux plans des couches dont la montagne est composee.”

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Here is a distinct view of that which may be found to take place in all consolidated strata, whatever be the composition of the stratum; and it is this appearance which is here maintained to be a physical demonstration, that those strata had been consolidated by means of heat softening their materials. In that case, those stratified bodies, contracting in cooling, form veins and fissures traversing perpendicularly their planes; and these veins are afterwards filled with mineral substances. These are what I have here distinguished as the *particular* veins of mineral masses; things perfectly different from proper mineral or metallic veins, which are more general, as belonging to immense masses of those strata; and which had been formed, not from the contraction, but from the disrupture of those masses, and by the forcible injection of fluid mineral substances from below. Now these two species of veins, the particular and the general, although occasionally connected, must be in science carefully distinguished; in the one, we see the means which had been employed for the consolidation of the strata; in the other, we see that power by which the strata have been raised from the bottom of the sea and placed in the atmosphere.]

Error never can be consistent, nor can truth fail of having support from the accurate examination of every circumstance. It is not enough to have found appearances decisive of the question, with regard to the two suppositions which have been now considered, we may farther seek confirmation of that supposition which has been found alone consistent with appearances.

If it be by means of heat and fusion that strata have been consolidated, then, in proportion to the degree of consolidation they have undergone from their original state, they should, *caeteris paribus*, abound more with separations in their mass. But this conclusion is found consistent with appearances. A stratum of porous sand-stone does not abound so much with veins and cutters as a similar stratum of marble, or even a similar stratum of sand-stone that is more consolidated. In proportion, therefore, as strata have been consolidated, they are in general intersected with veins and cutters; and in proportion as strata are deep in their perpendicular section, the veins are wide, and placed at greater distances. In like manner, when strata are thin, the veins are many, but proportionally narrow.

It is thus, upon chemical principles, to be demonstrated, That all the solid strata of the globe have been condensed by means of heat, and hardened from a state of fusion. But this proposition is equally to be maintained from principles which are mechanical. The strata of the globe, besides being formed of earths, are composed of sand, of gravel, and fragments of hard bodies, all which may be considered as, in their nature, simple; but these strata are also found composed of bodies which are not simple, but are fragments of former strata, which had been consolidated, and afterwards were broken and worn by attrition, so as to be made gravel. Strata composed in this manner have been again consolidated; and now the question is, By what means?

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If strata composed of such various bodies had been consolidated, by any manner of concretion, from the fluidity of a dissolution, the hard and solid bodies must be found in their entire state, while the interstices between those constituent parts of the stratum are filled up. No partial fracture can be conceived as introduced into the middle of a solid mass of hard matter, without having been communicated from the surrounding parts. But such partial separations are found in the middle of those hard and solid masses; therefore, this compound body must have been consolidated by other means than that of concretion from a state of a solution.

The Spanish marble already described, as well as many consolidated strata of siliceous gravel, of which I have specimens, afford the clearest evidence of this fact. These hard bodies are perfectly united together, in forming the most solid mass; the contiguous parts of some of the rounded fragments are interlaced together, as has already been observed; and there are partial shrinkings of the mass forming veins, traversing several fragments, but perfectly filled with the sparry substance of the mass, and sometimes with parts of the stone distinctly floating in the transparent body of spar. Now, there is not, besides heat or fusion, any known power in nature by which these effects might be produced. But such effects are general to all consolidated masses, although not always so well illustrated in a cabinet specimen.

Thus we have discovered a truth that is confirmed by every appearance, so far as the nature of the subject now examined admits. We now return to the general operation, of forming continents of those materials which had been deposited at the bottom of the sea.

SECTION III.

Investigation of the Natural Operations employed in the Production of Land above the Surface of the Sea.

We seek to know that operation by means of which masses of loose materials, collected at the bottom of the sea, were raised above its surface, and transformed into solid land.

We have found, that there is not in this globe (as a planet revolving in the solar system) any power or motion adapted to the purpose now in view; nor, were there such a power, could a mass of simply collected materials have continued any considerable time to resist the waves and currents natural to the sea, but must have been quickly carried away, and again deposited at the bottom of the ocean. But we have found, that there had been operations, natural to the bowels of this earth; by which those loose and unconnected materials have been cemented together, and consolidated into masses of great strength and hardness; those bodies are thus enabled to resist the force of waves and currents, and to preserve themselves, for a sufficient time, in their proper shape and place, as land above the general surface of the ocean.

We now desire to know, how far those internal operations of the globe, by which solidity and stability are procured to the beds of loose materials, may have been also employed in raising up a continent of land, to remain above the surface of the sea.

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There is nothing so proper for the erection of land above the level of the ocean, as an expansive power of sufficient force, applied directly under materials in the bottom of the sea, under a mass that is proper for the formation of land when thus erected. The question is not, how such a power may be procured; such a power has probably been employed. If, therefore, such a power should be consistent with that which we found had actually been employed in preparing the erected mass; or, if such a power is to be reasonably concluded as accompanying those operations which we have found natural to the globe, and situated in the very place where this expansive power appears to be required, we should thus be led to perceive, in the natural operations of the globe, a power as efficacious for the elevation of what had been at the bottom of the sea into the place of land, as it is perfect for the preparation of those materials to serve the purpose of their elevation.

In opposition to this conclusion, it will not be allowed to allege; that we are ignorant how such a power might be exerted under the bottom of the ocean; for, the present question is not, what had been the cause of heat, which has appeared to have been produced in that place, but if this power of heat, which has certainly been exerted at the bottom of the ocean for consolidating strata, had been employed also for another purpose, that is, for raising those strata into the place of land.

We may, perhaps, account for the elevation of land, by the same cause with that of the consolidation of strata, already investigated, without explaining the means employed by nature in procuring the power of heat, or showing from what general source of action this particular power had been derived; but, by finding in subterranean heat a cause for any other change, besides the consolidation of porous or incoherent bodies, we shall generalise a fact, or extend our knowledge in the explanation of natural appearances.

The power of heat for the expansion of bodies, is, so far as we know, unlimited; but, by the expansion of bodies placed under the strata at the bottom of the sea, the elevation of those strata may be effected; and the question now to be resolved regards the actual exertion of this power of expansion. How far it is to be concluded as having been employed in the production of this earth above the level of the sea.

Before attempting to resolve that question, it may be proper to observe, there has been exerted an extreme degree of heat below the strata formed at the bottom of the sea; and this is precisely the action of a power required for the elevation of those heated bodies into a higher place. Therefore, if there is no other way in which we may conceive this event to have been brought about, consistent with the present state of things, or what actually appears, we shall have a right to conclude, that such had been the order of procedure in natural things, and that the strata formed at the bottom of the sea had been elevated, as well as consolidated, by means of subterraneous heat.

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The consolidation of strata by means of fusion or the power of heat, has been concluded from the examination of nature, and from finding, that the present state of things is inconsistent with any other supposition. Now, again, we are considering the only power that may be conceived as capable of elevating strata from the bottom of the sea, and placing such a mass above the surface of the water. It is a truth unquestionable, that what had been originally at the bottom of the sea, is at present the highest of our land. In explaining this appearance, therefore, no other alternative is left, but either to suppose strata elevated by the power of heat above the level of the present sea, or the surface of the ocean reduced many miles below the height at which it had subsisted during the collection and induration of the land which we inhabit.

Now, if, on the one hand, we are to suppose no general power of subterraneous fire or heat, we leave to our theory no means for the retreat of the sea, or the lowering of its surface; if, on the other hand, we are to allow the general power of subterraneous heat, we cannot have much difficulty in supposing, either the surface of the sea to have subsided, or the bottom of the ocean, in certain parts, to have been raised by a subterranean power above the level of its surface, according as appearances shall be found to require the one or other of those conclusions. Here, therefore, we are again remitted to the history of nature, in order to find matter of fact by which this question may be properly decided.

If the present land had been discovered by the subsiding of the waters, there has not been a former land, from whence materials had been procured for the construction of the present, when at the bottom of the sea; for, there is no vestige remaining of that land, the whole land of the present earth having been formed evidently at the bottom of the sea. Neither could the natural productions of the sea have been accumulated, in the shape in which we now find them, on the surface of this earth; for, How should the Alps and Andes have been formed within the sea from the natural productions of the water? Consequently, this is a supposition inconsistent with every natural appearance.

The supposition, therefore, of the subsidence of the former ocean, for the purpose of discovering the present land, is beset with more difficulty than the simple erection of the bottom of the former ocean; for, *first*, There is a place to provide for the retirement of the waters of the ocean; and, *2dly*, There is required a work of equal magnitude; this is, the swallowing up of that former continent, which had procured the materials of the present land.

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On the one hand, the subsiding of the surface of the ocean would but make the former land appear the higher; and, on the other, the sinking the body of the former land into the solid globe, so as to swallow up the greater part of the ocean after it, if not a natural impossibility, would be at least a superfluous exertion of the power of nature. Such an operation as this would discover as little wisdom in the end elected, as in the means appropriated to that end; for, if the land be not wasted and worn away in the natural operations of the globe, Why make such a convulsion in the world in order to renew the land? If, again, the land naturally decays, Why employ so extraordinary a power, in order to hide a former continent of land, and puzzle man?

Let us now consider how far the other proposition, of strata being elevated by the power of heat above the level of the sea, may be confirmed from the examination of natural appearances.

The strata formed at the bottom of the ocean are necessarily horizontal in their position, or nearly so, and continuous in their horizontal direction or extent. They may change, and gradually assume the nature of each other, so far as concerns the materials of which they are formed; but there cannot be any sudden change, fracture, or displacement, naturally in the body of a stratum. But, if these strata are cemented by the heat of fusion, and erected with an expansive power acting below, we may expect to find every species of fracture, dislocation, and contortion, in those bodies, and every degree of departure from a horizontal towards a vertical position.

The strata of the globe are actually found in every possible position: For, from horizontal, they are frequently found vertical; from continuous, they are broken and separated in every possible direction; and, from a plane, they are bent and doubled. It is impossible that they could have originally been formed, by the known laws of nature, in their present state and position; and the power that has been necessarily required for their change, has not been inferior to that which might have been required for their elevation from the place in which they had been formed.

In this cafe, natural appearances are not anomalous. They are, indeed, infinitely various, as they ought to be, according to the rule; but all those varieties in appearances conspire to prove one general truth, *viz*. That all which we see had been originally composed according to certain principles, established in the constitution of the terraqueous globe; and that those regular compositions had been afterwards greatly changed by the operations of another power, which had introduced apparent confusion among things first formed in order and by rule.

It is concerning the operation of this second power that we are now inquiring; and here the apparent irregularity and disorder of the mineral regions are as instructive, with regard to what had been transacted in a former period of time, as the order and regularity of those same regions are conclusive, in relation to the place in which a former state of things had produced that which, in its changed state, we now perceive.

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We are now to conclude, that the land on which we dwell had been elevated from a lower situation by the same agent which had been employed in consolidating the strata, in giving them stability, and preparing them for the purpose of the living world. This agent is matter actuated by extreme heat, and expanded with amazing force.

If this has been the case, it will be reasonable to expect, that some of the expanded matter might be found condensed in the bodies which have been heated by that igneous vapour; and that matter, foreign to the strata, may have been thus introduced into the fractures and separations of those indurated masses.

We have but to open our eyes to be convinced of this truth. Look into the sources of our mineral treasures; ask the miner, from whence has come the metal into his vein? Not from the earth or air above,—not from the strata which the vein traverses; these do not contain one atom of the minerals now considered. There is but one place from whence these minerals may have come; this is the bowels of the earth, the place of power and expansion, the place from whence must have proceeded that intense heat by which loose materials have been consolidated into rocks, as well as that enormous force by which the regular strata have been broken and displaced.

Our attention is here peculiarly called upon, where we have the opportunity of examining those mineral bodies, which have immediately proceeded from the unknown region, that place of power and energy which we want to explore; for, if such is the system of the earth, that materials are first deposited at the bottom of the ocean, there to be prepared in a certain manner, in order to acquire solidity, and then to be elevated into the proper place of land, these mineral veins, which contain matter absolutely foreign to the surface of the earth, afford the most authentic information with regard to the operations which we want to understand. It is these veins which we are to consider as, in some measure, the continuation of that mineral region, which lies necessarily out of all possible reach of our examination. It is, therefore, peculiarly interesting to know the state in which things are to be found in this place, which may be considered as intermediate between the solid land, upon the one hand, and the unknown regions of the earth, upon the other.

We are now to examine those mineral veins; and these may be considered, first, in relation to their form, independent of their substance or particular contents; and, secondly, in relation to the contained bodies, independent of their form.

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In examining consolidated strata, we remarked veins and cutters as a proof of the means by which those bodies had been consolidated. In that case, the formation of these veins is a regulated process, determined by the degree of fusion, and the circumstances of condensation or refrigeration. In respect of these, the mineral veins now to be examined are anomalous. They are; but we know not why or how. We see the effect; but, in that effect, we do not see the cause. We can say, negatively, that the cause of mineral veins is not that by which the veins and fissures of consolidated strata have been formed; consequently, that it is not the measured contraction and regulated condensation of the consolidated land which has formed those general mineral veins; however, veins, similar in many respects, have been formed by the cooperation of this cause.

Having thus taken a view of the evident distinction between the veins or contractions that are particular to the consolidated body in which they are found, and those more general veins which are not limited to that cause, we may now consider what is general in the subject, or what is universal in these effects of which we wish to investigate the cause.

The event of highest generalization or universality, in the form of those mineral veins, is fracture and dislocation. It is not, like that of the veins of strata, simple separation and measured contraction; it is violent fracture and unlimited dislocation. In the one case, the forming cause is in the body which is separated; for, after the body had been actuated by heat, it is by the reaction of the proper matter of the body, that the chasm which constitutes the vein is formed. In the other case, again, the cause is extrinsic in relation to the body in which the chasm is formed. There has been the most violent fracture and divulsion; but the cause is still to seek; and it appears not in the vein; for it is not every fracture and dislocation of the solid body of our earth, in which minerals, or the proper substances of mineral veins, are found.

We are now examining matter of fact, real effects, from whence we would investigate the nature of certain events which do not now appear. Of these, two kinds occur; one which has relation to the hardness and solidity, or the natural constitution of the body; the other, to its shape or local situation. The first has been already considered; the last is now the subject of inquiry.

But, in examining those natural appearances, we find two different kinds of veins; the one necessarily connected with the consolidating cause; the other with that cause of which we now particularly inquire. For, in those great mineral veins, violent fracture and dislocation is the principle; but there is no other principle upon which strata, or masses formed at the bottom of the sea, can be placed at a height above its surface. Hence, in those two different operations, of forming mineral veins, and erecting strata from a lower to a higher place, the principle is the same; for, neither can be done without violent fracture and dislocation.

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We now only want to know, how far it is by the same power, as well as upon the same principle, that these two operations have been made. An expansive force, acting from below, is the power most proper for erecting masses; but whether it is a power of the same nature with that which has been employed in forming mineral veins, will best appear in knowing the nature of their contents. These, therefore, may be now considered.

Every species of fracture, and every degree of dislocation and contortion, may be perceived in the form of mineral veins; and there is no other general principle to be observed in examining their form. But, in examining their contents, some other principle may appear, so far as, to the dislocating power or force, there may be superadded matter, by which something in relation to the nature of the power may be known. If, for example, a tree or a rock shall be found simply split asunder, although there be no doubt with regard to some power having been applied in order to produce the effect, yet we are left merely to conjecture at the power. But when wedges of wood or iron, or frozen water, should be found lodged in the cleft, we might be enabled, from this appearance, to form a certain judgment with regard to the nature of the power which had been applied. This is the case with mineral veins. We find them containing matter, which indicates a cause; and every information in this case is interesting to the theory.

The substances contained in mineral veins are precisely the same with those which, in the former section, we have considered as being made instrumental in the consolidation of strata; and they are found mixed and concreted in every manner possible.

But, besides this evidence for the exertion of extreme heat, in that process by which those veins were filled, there is another important observation to be gathered from the inspection of this subject. There appears to have been a great mechanical power employed in the filling of these veins, as well as that necessarily required in making the first fracture and divulsion.

This appears from the order of the contents, or filling of these veins, which is a thing often observed to be various and successive. But what it is chiefly now in view to illustrate, is that immense force which is manifested in the fracture and dispersion of the solid contents which had formerly filled those veins. Here we find fragments of rock and spar floating in the body of a vein filled with metallic substances; there, again, we see the various fragments of metallic masses floating in the sparry and siliceous contents.

One thing is demonstrable from the inspection of the veins and their contents; this is, the successive irruptions of those fluid substances breaking the solid bodies which they meet, and floating those fragments of the broken bodies in the vein. It is very common to see three successive series of those operations; and all this may be perceived in a small fragment of stone, which a man of science may examine in his closet, often better than descending to the mine, where all the examples are found on an enlarged scale.

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Let us now consider what power would be required to force up, from the most unfathomable depth of the ocean, to the Andes or the Alps, a column of fluid metal and of stone. This power cannot be much less than that required to elevate the highest land upon the globe. Whether, therefore, we shall consider the general veins as having been filled by mineral steams, or by fluid minerals, an elevating power of immense force is still required, in order to form as well as fill those veins. But such a power acting under the consolidated masses at the bottom of the sea, is the only natural means for making those masses land.

If such have been the operations that are necessary for the production of this land; and if these operations are natural to the globe of this earth, as being the effect of wisdom in its contrivance, we shall have reason to look for the actual manifestation of this truth in the phaenomena of nature, or those appearances which more immediately discover the actual cause in the perceived effect.

To see the evidence of marble, a body that is solid, having been formed of loose materials collected at the bottom of the sea, is not always easy, although it may be made abundantly plain; and to be convinced that this calcareous stone, which calcines so easily in our fires, should have been brought into fusion by subterraneous heat, without suffering calcination, must require a chain of reasoning which every one is not able to attain[13]. But when fire bursts forth from the bottom of the sea, and when the land is heaved up and down, so as to demolish cities in an instant, and split asunder rocks and solid mountains, there is nobody but must see in this a power, which may be sufficient to accomplish every view of nature in erecting land, as it is situated in the place most advantageous for that purpose.

[Note 13: Mr le Chevalier de Dolomieu, in considering the different effects of heat, has made the following observation; Journal de Physique, Mai 1792.

“Je dis *le feu tel que nous l’employons* pour distinguer le feu naturel des volcans, du feu de nos fourneaux et de celui de nos chalumeaux. Nous sommes obliges de donner une grande activite a son action pour suppleer et au volume qui ne seroit pas a notre disposition et au tems que nous sommes forces de menager, et cette maniere d’appliquer une chaleur tres-active, communique le mouvement et le desordre jusques dans les molecules constituantes. Agregation et composition, tout est trouble. Dans les volcans la grand masse du feu supplee a son intensite, le tems remplace son activite, de maniere qu’il tourmente moins les corps fournis a son action; il menage leur composition en relachant leur agregation, et les pierres qui eut ete rendues fluides par l’embrasement volcanique peuvent reprendre leur etat primitif; la plupart des substances qu’un feu plus actif auroit expulsees y restent encore. Voila pourquoi les laves ressemblent tellement aux

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pierres naturelles des especes analogues, qu'elles ne peuvent en etre distinguees; voila egalement pourquoi les verres volcaniques eux-meme renferment encore des substances elastiques qui les font boursoufler lorsque nous les fondons de nouveau, et pourquoi ces verres blanchissent aussi, pour lors, par la dissipation, d'une substance grasse qui a resiste a la chaleur des volcans, et que volatilise la chaleur par laquelle nous obtenons leur second fusion."

No doubt, the long application of heat may produce changes in bodies very different from those which are occasioned by the sudden application of a more intense heat; but still there must be sufficient intensity in that power, so as to cause fluidity, without which no chemical change can be produced in bodies. The essential difference, however, between the natural heat of the mineral regions, and that which we excite upon the surface of the earth, consists in this; that nature applies heat under circumstances which we are not able to imitate, that is, under such compression as shall prevent the decomposition of the constituent substances, by the separation of the more volatile from the more fixed parts. This is a circumstance which, so far as I know, no chemist or naturalist has hitherto considered; and it is that by which the operations of the mineral regions must certainly be explained. Without attending to this great principle in the mineralizing operations of subterraneous fire, it is impossible to conceive the fusion and concretion of those various bodies, which we examine when brought up to the surface of the earth.]

The only question, therefore, which it concerns us to decide at present, is, Whether those operations of extreme heat, and violent mechanic force, be only in the system as a matter of accident; or if, on the contrary, they are operations natural to the globe, and necessary in the production of such land as this which we inhabit? The answer to this is plain: These operations of the globe remain at present with undiminished activity, or in the fullness of their power.

A stream of melted lava flows from the sides of Mount Aetna. Here is a column of weighty matter raised from a great depth below, to an immense height above, the level of the sea, and rocks of an enormous size are projected from its orifice some miles into the air. Every one acknowledges that here is the liquefying power and expansive force of subterranean fire, or violent heat. But, that Sicily itself had been raised from the bottom of the ocean, and that the marble called Sicilian Jasper, had its solidity upon the same principle with the lava, would stumble many a naturalist to acknowledge. Nevertheless, I have in my possession a table of this marble, from which it is demonstrable, that this calcareous stone had flowed, and been in such a state of fusion and fluidity as lava.

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Here is a comparison formed of two mineral substances, to which it is of the highest importance to attend. The solidity and present state of the one of these is commonly thought to be the operation of fire; of the other, again, it is thought to be that of water. This, however, is not the case. The immediate state and condition of both these bodies is now to be considered as equally the effect of fire or heat. The reason of our forming such a different judgment with regard to these two subjects is this; we see, in the one case, the more immediate connection of the cause and the effect, while, in the other, we have only the effects from whence we are in science to investigate the cause.

But, if it were necessary always to see this immediate connection, in order to acknowledge the operation of a power which, at present, is extinguished in the effect, we should lose the benefit of science, or general principles, from whence particulars may be deduced, and we should be able to reason no better than the brute. Man is made for science; he reasons from effects to causes, and from causes to effects; but he does not always reason without error. In reasoning, therefore, from appearances which are particular, care must be taken how we generalise; we should be cautious not to attribute to nature, laws which may perhaps be only of our own invention.

The immediate question now before us is not, If the subterraneous fire, or elevating power, which we perceive sometimes as operating with such energy, be the consolidating cause of strata formed at the bottom of the sea; nor, if that power be the means of making land appear above the general surface of the water? for, though this be the end we want to arrive at ultimately, the question at present in agitation respects the laws of nature, or the generality of particular appearances.

Has the globe within it such an active power as fits it for the renovation of that part of its constitution which may be subject to decay? Are those powerful operations of fire, or subterraneous heat, which so often have filled us with terror and astonishment, to be considered as having always been? Are they to be concluded as proper to every part upon the globe, and as continual in the system of this earth? If these points in question shall be decided in the affirmative, we can be at no loss in ascertaining the power which has consolidated strata, nor in explaining the present situation of those bodies, which had their origin at the bottom of the sea. This, therefore, should be the object of our pursuit; and in order to have demonstration in a case of physical inquiry, we must again have recourse to the book of nature.

The general tendency of heat is to produce fluidity and softness; as that of cold is, on the contrary, to harden soft and fluid bodies. But this softening power of heat is not uniform in its nature; it is made to act with very different effect, according to the nature of the substance to which it is applied. We are but limited in the art of increasing the heat or the cold of bodies; we find, however, extreme difference in their substances with respect to fusibility.

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A fusible substance, or mineral composition in a fluid state, is emitted from those places of the earth at which subterraneous fire and expansive force are manifested in those eruptive operations. In examining these emitted bodies, men of science find a character for such productions, in generalising the substance, and understanding the natural constitution of those bodies. It is in this manner that such a person, finding a piece of lava in any place of the earth, says with certainty, Here is a stone which had congealed from a melted state.

Having thus found a distinguishing character for those fused substances called, in general, Lavas, and having the most visible marks for that which had been actually a volcano, naturalists, in examining different countries, have discovered the most undoubted proofs of many ancient volcanos, which had not been before suspected. Thus, volcanos will appear to be not a matter of accident, or as only happening in a particular place, they are general to the globe, so far as there is no place upon the earth that may not have an eruption of this kind; although it is by no means necessary for every place to have had those eruptions.

Volcanos are natural to the globe, as general operations; but we are not to consider nature as having a burning mountain for an end in her intention, or as a principal purpose in the general system of this world. The end of nature in placing an internal fire or power of heat, and a force of irresistible expansion, in the body of this earth, is to consolidate the sediment collected at the bottom of the sea, and to form thereof a mass of permanent land above the level of the ocean, for the purpose of maintaining plants and animals. The power appointed for this purpose is, as on all other occasions, where the operation is important, and where there is any danger of a shortcoming, wisely provided in abundance; and there are contrived means for disposing of the redundancy. These, in the present case, are our volcanos.

A volcano is not made on purpose to frighten superstitious people into fits of piety and devotion, nor to overwhelm devoted cities with destruction; a volcano should be considered as a spiracle to the subterranean furnace, in order to prevent the unnecessary elevation of land, and fatal effects of earthquakes; and we may rest assured, that they, in general, wisely answer the end of their intention, without being in themselves an end, for which nature had exerted such amazing power and excellent contrivance.

Let us take a view of the most elevated places of the earth; if the present theory is just, it is there that we should find volcanos. But is not this the case? There are volcanos in the Andes; and round the Alps we find many volcanos, which are in France upon the one side, and in Germany upon the other, as well as upon the Italian side, where Vesuvius still continues to exhibit violent eruptions.

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It is not meant to allege, that it is only upon the summit of a continent volcanos should appear. Subterraneous fire has sometimes made its appearance in bursting from the bottom of the sea. But, even in this last case, land was raised from the bottom of the sea, before the eruption made its exit into the atmosphere. It must also be evident, that, in this case of the new island near Santorini, had the expansive power been retained, instead of being discharged, much more land might have been raised above the level of the ocean.

Now, the eruption of that elastic force through the bottom of the sea, may be considered as a waste of power in the operations of the globe, where the elevation of indurated strata is an object in the exertion of that power; whereas, in the centre of a continent sufficiently elevated above the level of the sea, the eruption of that fiery vapour calculated to elevate the land, while it may occasionally destroy the habitations of a few, provides for the security and quiet possession of the many.

In order to see the wisdom of this contrivance, let us consider the two extreme places at which this eruption of ignited matter may be performed. These are, on the one hand, within a continent of land, and, on the other, at the bottom of the ocean. In the one case, the free eruption of the expanding power should be permitted; because the purpose for which it had been calculated to exist has been accomplished. In the other, again, the free eruption of that powerful matter should be repressed; because there is reserved for that power much of another operation in that place. But, according to the wise constitution of things, this must necessarily happen. The eruption of the fiery vapour from volcanos on the continent or land, is interrupted only occasionally, by the melted bodies flowing in the subterraneous chimney; whereas, at the bottom of the ocean, the contact of the water necessarily tends to close the orifice, by accumulating condensed matter upon the weakest place.

If this be a just theory of the natural operations of the globe, we shall have reason to expect, that great quantities of this melted matter, or fusible substance, may be found in form of lava, among the strata of the earth, where there are no visible marks of any volcano, or burning mountain, having existed. Here, therefore, is an important point to be determined; for, if it shall appear that much of this melted matter, analogous to lava, has been forced to flow among the strata which had been formed at the bottom of the sea, and now are found forming dry land above its surface, it will be allowed, that we have discovered the secret operations of nature concocting future land, as well as those by which the present habitable earth had been produced from the bottom of the abyss. Here, therefore, we shall at present rest the argument, with endeavouring to show that such is actually the case.

It appears from Cronstedt's Mineralogy, that the rock-stone, called trap by the Swedes, the amygdaloides and the schwarts-stein of the Germans, are the same with the whin-stone of this country. This is also fully confirmed by specimens from Sweden, sent me by my friend Dr Gahn. Whatever, therefore, shall be ascertained with regard to our

whin-stone, may be so far generalized or extended to the countries of Norway, Sweden, and Germany.

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The whin-stone of Scotland is also the same with the toad-stone of Derbyshire, which is of the amygdaloides species; it is also the same with the flagstone of the south of Staffordshire, which is a simple whin-stone, or perfect trap. England, therefore, must be included in this great space of land, the mineral operations of which we explore; and also Ireland, of which the Giant's Causeway, and many others, are sufficient proof.

In the south of Scotland, there is a ridge of hills, which extends from the west side of the island in Galloway to the east side in Berwickshire, composed of granite, of schistus, and of siliceous strata. The Grampians on the north, again, form another range of mountains of the same kind; and between these two great fields of broken, tumbled, and distorted strata, there lies a field of lesser hardness and consolidation, in general; but a field in which there is a great manifestation of subterraneous fire, and of exerted force.

The strata in this space consist, in general, of sand-stone, coal, lime-stone or marble, iron-stone, and marl or argillaceous strata, with strata of analogous bodies, and the various compositions of these. But what is to the present purpose is this, that, through all this space, there are interspersed immense quantities of whinstone; a body which is to be distinguished as very different from lava; and now the disposition of this whin-stone is to be considered.

Sometimes it is found in an irregular mass or mountain, as Mr Cronstedt has properly observed; but he has also said, that this is not the case in general. His words are: "It is oftener found in form of veins in mountains of another kind, running commonly in a serpentine manner, contrary or across to the direction of the rock itself."

The origin of this form, in which the trap or whin-stone appears, is most evident to inspection, when we consider that this solid body had been in a fluid state, and introduced, in that state, among strata, which preserved their proper form. The strata appear to have been broken, and the two correspondent parts of those strata are separated to admit the flowing mass of whin-stone.

A fine example of this kind may be seen upon the south side of the Earn, on the road to Crief. It is twenty-four yards wide, stands perpendicular, and appears many feet above the surface of the ground. It runs from that eastward, and would seem to be the same with that which crosses the river Tay, in forming Campsy-lin above Stanley, as a lesser one of the same kind does below it. I have seen it at Lednoc upon the Ammon, where it forms a cascade in that river, about five or six miles west of Campsy-lin. It appears to run from the Tay east through Strathmore, so that it may be considered as having been traced for twenty or thirty miles, and westwards to Drummond castle, perhaps much farther.

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Two small veins of the same kind, only two or three feet wide, may be seen in the bed of the Water of Leith, traversing the horizontal strata, the one is above St Bernard's well, the other immediately below it. But, more particularly, in the shire of Ayr, to the north of Irvine, there are to be seen upon the coast, between that and Scarmorly, in the space of about twenty miles, more than twenty or thirty such dykes (as they are called) of whin-stone. Some of them are of a great thickness; and, in some places, there is perceived a short one, running at right angles, and communicating with other two that run parallel.

There is in this country, and in Derbyshire[14], another regular appearance of this stone, which Cronstedt has not mentioned. In this case, the strata are not broken in order to have the whin-stone introduced, they are separated, and the whin-stone is interjected in form of strata, having various degrees of regularity, and being of different thickness. On the south side of Edinburgh, I have seen, in little more than the space of a mile from east to west, nine or ten masses of whin-stone interjected among the strata. These masses of whin-stone are from three or four to an hundred feet thick, running parallel in planes inclined to the horizon, and forming with it an angle of about twenty or thirty degrees, as may be seen at all times in the hill of Salisbury Craggs.

[Note 14: See Mr Whitehurst's Theory of the Earth.]

Having thus described these masses, which have flowed by means of heat among the strata of the globe, strata which had been formed by subsidence at the bottom of the sea, it will now be proper to examine the difference that subsists between these subterraneous lavas, as they may be termed, and the analogous bodies which are proper lavas, in having issued out of a volcano.[15]

[Note 15: The Chevalier de Dolomieu, in his accurate examination of Aetna and the Lipari islands, has very well observed the distinction of these two different species of lavas; but without seeming to know the principle upon which this essential difference depends. No bias of system, therefore, can here be supposed as perverting the Chevalier's view, in taking those observations; and these are interesting to the present theory, as corresponding perfectly with the facts from whence it has been formed. It will be proper to give the account of these in his own words.

La zeolite est tres-commune dans certains laves de l'Ethna; il seroit peut-etre possible d'y en rencontrer des morceaux aussi gros que ceux que fournit l'isle de Ferroe. Quoique cette substance semble ici appartenir aux laves, je ne dirai cependant point que toutes les zeolites soient volcaniques, ou unies a des matieres volcaniques; celles que l'on trouve en Allemagne sont, dit-on, dans des circonstances differentes; mais je doit annoncer que je n'ai trouve cette substance en Sicile, que dans les seules laves qui evidemment ont coule dans la mer, et qui

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out ete recouvertes par ses eaux. La zeolite des laves n'est point une dejection volcanique, ni une production du feu, ni meme un matiere que les laves aient enveloppee lorsqu'elles etoient fluides; elle est le resultat d'une operation et d'une combinaison posterieure, auxquelles les eaux de la mer ont concouru. Les laves qui n'ont pas ete submergees, n'en contiennent jamais. J'ai trouve ces observations si constantes, que par-tout ou je rencontrois de la zeolite, j'etois sur de trouver d'autres preuves de submersion, et partout ou je voyois des laves recouvertes des depots de l'eau, j'etois sur de trouver de la zeolite, et un de ces faits m'a toujours indique l'autre. Je me suis servi avec succes de cette observation pour diriger mes recherches, et pour connoitre l'antiquite des laves. *Mineralogie de Volcans, par M. Faujas de Saint-Fond*. Here would appear to be the distinction of subterraneous lava, in which zeolite and calcareous spar may be found, and that which has flowed from a volcano, in which neither of these are ever observed.]

There can be no doubt that these two different species of bodies have had the same origin, and that they are composed of the same materials nearly; but from the different circumstances Of their production, there is formed a character to these bodies, by which, they may be perfectly distinguished. The difference of those circumstances consists in this; the one has been emitted to the atmosphere in its fluid state the other only came to be exposed to the light in a long course of time, after it had congealed under the compression of an immense load of earth, and after certain operations, proper to the mineral regions, had been exercised upon the indurated mass. This is the cause of the difference between those erupted lavas, and our whin-stone, toad-stone, and the Swedish trap, which may be termed subterraneous lava. The visible effects of those different operations may now be mentioned.

In the erupted lavas, those substances which are subject to calcine and vitrify in our fires, suffer similar changes, when delivered from a compression which had rendered them fixed, though in an extremely heated state. Thus, a lava in which there is much calcareous spar, when it comes to be exposed to the atmosphere, or delivered from the compressing force of its confinement, effervesces by the explosion of its fixed air; the calcareous earth, at the same time, vitrifies with the other substances. Hence such violent ebullition in volcanos, and hence the emission of so much pumice-stone and ashes, which are of the same nature.

In the body of our whin-stone, on the contrary, there is no mark of calcination or vitrification. We frequently find in it much calcareous spar, or the *terra calcarea aerata*, which had been in a melted state by heat, and had been crystallized by congelation into a sparry form. Such is the *lapis amygdaloides*, and many of our whin-stone rocks, which contain pebbles crystallized and variously figured, both calcareous, siliceous, and of a mixture in which both these substances form distinct parts. The specimens of this kind, which I have from the whin-stone or porphyry rock of the Calton-hill, exhibit every

species of mineral operation, in forming jasper, figured agate, and marble; and they demonstrate, that this had been performed by heat or fusion.

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I do not mean to say, that this demonstration is direct; it is conditional, and proceeds upon the supposition, that the basaltic or porphyry rock, in which those specimens are found, is a body which had been in a melted state. Now, this is a supposition for which I have abundance of evidence, were it required; but naturalists are now sufficiently disposed to admit that proposition; they even draw conclusions from this fact, which, I think, they are not sufficiently warranted in doing; that is, from this appearance, they infer the former existence of volcanos in those places. For my part, though I have made the most strict examination, I never saw any vestige of such an event. That there are, in other countries, evident marks of volcanos which have been long extinguished, is unquestionably true; but naturalists, imagining that there are no other marks of subterraneous fire and fusion, except in the production of a lava, attribute to a volcano, as a cause, these effects, which only indicate the exertion of that power which might have been the cause of a volcano.

If the theory now given be just, a rock of marble is no less a mark of subterraneous fire and fusion, than that of the basaltes; and the flowing of basaltic streams among strata broken and displaced, affords the most satisfactory evidence of those operations by which the body of our land had been elevated above the surface of the sea; but it gives no proof that the eruptive force of mineral vapours had been discharged in a burning mountain. Now, this discharge is essential in the proper idea of a volcano.

Besides this internal mark of an unerupted lava in the substance of the stone or body of the flowing mass, there are others which belong to it in common with all other mineral strata, consolidated by subterraneous fire, and changed from the place of their original formation; this is, the being broken and dislocated, and having veins of foreign matter formed in their separations and contractions.

If these are mineral operations, proper to the lower regions of the earth, and exerted upon bodies under immense compression, such things will be sometimes found in the unerupted lavas, as well as in the contiguous bodies with which they are associated. If, on the contrary, these are operations proper to the surface of the earth, where the dissolving power of water and air take place, and where certain stalactical and ferruginous concretions are produced by these means; then, in erupted lavas, we should find mineral concretions, which concretions should be denied to bodies which had been consolidated at the bottom of the sea; that is to say, where, without the operation of subterraneous fire, no changes of that kind could have taken place, as has already been observed. But in the unerupted species of lava, that is to say, in our whin-stone, every species of mineral appearance is occasionally to be found. Let those who have the opportunity to examine, say, what arc to be found in proper lavas,

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that is, those of the erupted kind. Sir William Hamilton informed me, when I showed him those mineral veins and spars in our whin-stone, that he had never observed the like, in lavas We have now formed some conclusions with regard to the nature and production of those parts of the land of this globe which we have had the means of examining perfectly; but; from the accounts of travellers, and from, the specimens which are brought to us from distant parts, we have reason to believe, that all the rest of the earth is of the same nature with that which has been now considered. The great masses of the earth are the same every where; and all the different species of earths, of rocks or stone, which have as yet appeared, are to be found in the little space of this our island.

It is true, that there are peculiar productions in the mineral kingdom which are rare, as being found only in few places; but these things are merely accidental in relation to the land, for they belong in property to those parts of the mineral region which we never see. Such are, the diamond of the east, the platina of the west, and the tin of Cornwall, Germany, and Sumatra. Gold and silver, though found in many countries, do not appear to be immediately necessary in the production of a habitable country. Iron, again, is universal in the operations of the globe, and is found often in that profusion which equals its utility. Between these two extremes, we find all other minerals, that is to say, here and there in moderate quantity, and apparently in some proportion to their use. But all these substances are to be considered as the vapours of the mineral regions, condensed occasionally in the crevices of the land; and it is only the rocks and strata (in which those mineral veins are found) that are now examined with regard to their original composition, at the bottom of the sea, as well as to that, operation by which those bodies had been indurated in their substance, and elevated from the place in which they had been formed.

Thus, we have sufficient reason to believe, that, in knowing the construction of the land in Europe, we know the constitution of the land in every part of the globe. Therefore, we may proceed to form general conclusions, from the knowledge of the mineral region, thus acquired in studying those parts which are seen.

Having thus found, *first*, That the consolidated and indurated masses of our strata had suffered the effects of violent heat and fusion; *2dly*, That those strata, which had been formed in a regular manner at the bottom of the sea, have been violently bended, broken, and removed from their original place and situation; and, *lastly*, Having now found the most indubitable proof, that the melting, breaking, and removing power of subterraneous fire, has been actually exerted upon this land which we examine, we cannot hesitate in ascribing these operations as a cause to those effects which are exposed to our view. Now, these may be considered as consisting in the solid state and present situation of those stratified bodies, originally formed by subsidence in the ocean; appearances which cannot, in reason, be ascribed to any other cause, and which, upon this principle, are perfectly explained.

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It is not meant to specify every particular in the means employed by nature for the elevation of our land. It is sufficient to have shown, that there is, in nature, means employed for the consolidating of strata, formed originally of loose and incoherent materials; and that those same means have also been employed in changing the place and situation of those strata. But how describe an operation which man cannot have any opportunity of perceiving? Or how imagine that, for which, perhaps, there are not proper data to be found? We only know, that the land is raised by a power which has for principle subterraneous heat; but, how that land is preserved in its elevated station, is a subject in which we have not even the means to form conjecture; at least, we ought to be cautious how we indulge conjecture in a subject where no means occur for trying that which is but supposition.

We now proceed, from the facts which have been properly established, to reason with regard to the duration of this globe, or the general view of its operations, as a living world, maintaining plants and animals.

SECTION IV.

System of Decay and Renovation observed in the Earth.

Philosophers observing an apparent disorder and confusion in the solid parts of this globe, have been led to conclude, that there formerly existed a more regular and uniform state, in the constitution of this earth; that there had happened some destructive change; and that the original structure of the earth had been broken and disturbed by some violent operation, whether natural, or from a super-natural cause. Now, all these appearances, from which conclusions of this kind have been formed, find the most perfect explanation in the theory which we have been endeavouring to establish; for they are the facts from whence we have reasoned, in discovering the nature and constitution of this earth: Therefore, there is no occasion for having recourse to any unnatural supposition of evil, to any destructive accident in nature, or to the agency of any preternatural cause, in explaining that which actually appears.

It is necessary for a living or inhabited world, that this should consist of land and water. It is also necessary, that the land should be solid and stable, refilling, with great power, the violent efforts of the ocean; and, at the same time, that this solid land should be resolved by the influence of the sun and atmosphere, so as to decay, and thus become a soil for vegetation. But these general intentions are perfectly fulfilled in the constitution of our earth, which has been now investigated. This great body being formed of different mixed masses, having various degrees of hardness and solubility, proper soil for plants is supplied from the gradual resolution of the solid parts; fertility in those soils arises from the mixture of different elementary substances; and stability is procured to that vegetable world, by the induration of certain bodies, those rocks and stones, which protect the softer masses of clay and soil.

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In this manner, also, will easily be explained those natural appearances which diversify the surface of the earth for the use of plants and animals, and those objects which beautify the face of nature for the contemplation of mankind. Such are, the distinctions of mountains and valleys, of lakes and rivers, of dry barren deserts and rich watered plains, of rocks which stand apparently unimpaired by the lapse of time, and sands which fluctuate with the winds and tides. All these are the effects of steady causes; each of these has its proper purpose in the system of the earth; and in that system is contained another, which is that of living growing bodies, and of animated beings.

But, besides this, man, the intellectual being, has, in this subject of the mineral kingdom, the means of gratifying the desire of knowledge, a faculty by which he is distinguished from the animal, and by which he improves his mind in knowing causes. Man is not satisfied, like the brute, in seeing things which are; he seeks to know how things have been, and what they are to be. It is with pleasure that he observes order and regularity in the works of nature, instead of being disgusted with disorder and confusion; and he is made happy from the appearance of wisdom and benevolence in the design, instead of being left to suspect in the Author of nature, any of that imperfection which he finds in himself.

Let us now take a view of that system of mineral economy, in which may be perceived every mark of order and design, of provident wisdom and benevolence.

We have been endeavouring to prove, that all the continents and islands of this globe had been raised above the surface of the ocean; we have also aimed at pointing out the cause of this translation of matter, as well as of the general solidity of that which is raised to our view; but however this theory shall be received, no person of observation can entertain a doubt, that all, or almost all we see of this earth, had been originally formed at the bottom of the sea. We have now another object in our view; this is to investigate the operations of the globe, at the time that the foundation of this land was laying in the waters of the ocean, and to trace the existence and the nature of things, before the present land appeared above the surface of the waters. We should thus acquire some knowledge of the system according to which this world is ruled, both in its preservation and production; and we might be thus enabled to judge, how far the mineral system of the world shall appear to be contrived with all the wisdom, which is so manifest in what are termed the animal and vegetable kingdoms.

It must not be imagined that this undertaking is a thing unreasonable in its nature; or that it is a work necessarily beset with any unsurmountable difficulty; for, however imperfectly we may fulfill this end proposed, yet, so far as it is to natural causes that are to be ascribed the operations of former time, and so far as, from the present state of things, or knowledge of natural history, we have it in our power to reason from effect to cause, there are, in the constitution of the world, which we now examine, certain means to read the annals of a former earth.

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The object of inquiry being the operations of the globe, during the time that the present earth was forming at the bottom of the sea, we are now to take a very general view of nature, without descending into those particulars which so often occupy the speculations of naturalists, about the present state of things. We are not at present to enter into any discussion with regard to what are the primary and secondary mountains of the earth; we are not to consider what is the first, and what the last, in those things which now are seen; whatever is most ancient in the strata which we now examine, is supposed to be collecting at the bottom of the sea, during the period concerning which we are now to inquire.

We have already considered those operations which had been necessary in forming our solid land, a body consisting of materials originally deposited at the bottom of the ocean; we are now to investigate the source from whence had come all those materials, from the collection of which the present land is formed; and from knowing the state in which those materials had existed, previously to their entering the composition of our strata, we shall learn something concerning the natural history of this world, while the present earth was forming in the sea.

We have already observed, that all the strata of the earth are composed either from the calcareous relicts of sea animals, or from the collection of such materials as we find upon our shores. At a gross computation, there may perhaps be a fourth part of our solid land, which is composed from the matter that had belonged to those animals. Now, what a multitude of living creatures, what a quantity of animal economy must have been required for producing a body of calcareous matter which is interspersed throughout all the land of the globe, and which certainly forms a very considerable part of that mass! Therefore, in knowing how those animals had lived, or with what they had been fed, we shall have learned a most interesting part of the natural history of this earth; a part which it is necessary to have ascertained, in order to see the former operations of the globe, while preparing the materials of the present land. But, before entering upon this subject, let us examine the other materials of which our land is formed.

Gravel forms a part of those materials which compose our solid land; but gravel is no other than a collection of the fragments of solid stones worn round, or having their angular form destroyed by agitation in water, and the attrition upon each other, or upon similar hard bodies. Consequently, in finding masses of gravel in the composition of our land, we must conclude, that there had existed a former land, on which there had been transacted certain operations of wind and water, similar to those which are natural to the globe at present, and by which new gravel is continually prepared, as well as old gravel consumed or diminished by attrition upon our shores.

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Sand is the material which enters, perhaps in greatest quantity, the composition of our land. But sand, in general, is no other than small fragments of hard and solid bodies, worn or rounded more or less by attrition; consequently, the same natural history of the earth, which is investigated from the masses of gravel, is also applicable to those masses of sand which we find forming so large a portion of our present land throughout all the earth[16].

[Note 16: Sand is a term that denotes no particular substance; although by it is commonly meant a siliceous substance, as being by far the most prevalent. Sand is one of the modifications, of size and shape, in a hard body or solid substance, which may be infinitely diversified. The next modification to be distinguished in mineral bodies is that of gravel; and this differs in no respect from sand, except in point of size. Next after gravel, in the order of ascent, come stones; and these bear nearly the same relation to gravel as gravel does to sand. Now, by stones is to be understood the fragments of rocks or solid mineral bodies; and there is a perfect gradation from those stones to sand. I have already endeavoured to explain the formation of those stony substances; and now I am treating of a certain system of circulation, which is to be found among minerals.

M. de Luc censures me for not giving the origin of sand, of which I form the strata of the earth. He seems to have misunderstood my treatise. I do not pretend, as he does in his theory, to describe the beginning of things; I take things such as I find them at present, and from these I reason with regard to that which must have been. When, from a thing which is well known, we explain another which is less so, we then investigate nature; but when we imagine things without a pattern or example in nature, then, instead of natural history, we write only fable.

M. de Luc, in the letter already mentioned, says, “that sand may be, and I think it is, a substance which has formed *strata* by *precipitation in a liquid*.” This is but an opinion, which may be either true or false. If it be true, it is an operation of the mineral kingdom of which I am ignorant. In all the sand which I have ever examined, I have never seen any that might not be referred to the species of mineral substance from which it had been formed. When this author shall have given us any kind of information with regard to the production of sand *by precipitation in a liquid*, it will then be time enough to think of forming the strata of the earth with that sand.]

Clay is now to be considered as the last of those materials of which our strata are composed; but, in order to understand the nature of this ingredient, something must be premised.

Clay is a mixture of different earths or hard substances, in an impalpable state. Those substances are chiefly the siliceous and aluminous earths. Other earths are occasionally mixed in clays, or perhaps always to be found in some small portion. But this does not affect the general character of clay; it only forms a special variety in the

subject. A sensible or considerable portion of calcareous earth, in the composition of clay, constitutes a marl, and a sufficient admixture of sand, a loam.

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An indefinite variety of those compositions of clay form a large portion of the present strata, all indurated and consolidated in various degrees; but this great quantity of siliceous, argillaceous, and other compound substances, in form of earth or impalpable sediment, corresponds perfectly with that quantity of those same substances which must have been prepared in the formation of so much gravel and sand, by the attrition of those bodies in the moving waters.

Therefore, from the consideration of those materials which compose the present land, we have reason to conclude, that, during the time this land was forming, by the collection of its materials at the bottom of the sea, there had been a former land containing materials similar to those which we find at present in examining the earth. We may also conclude, that there had been operations similar to those which we now find natural to the globe, and necessarily exerted in the actual formation of gravel, sand, and clay. But what we have now chiefly in view to illustrate is this, that there had then been in the ocean a system of animated beings, which propagated their species, and which have thus continued their several races to this day.

In order to be convinced of that truth, we have but to examine the strata of our earth, in which we find the remains of animals. In this examination, we not only discover every genus of animal which at present exists in the sea, but probably every species, and perhaps some species with which at present we are not acquainted. There are, indeed, varieties in those species, compared with the present animals which we examine, but no greater varieties than may perhaps be found among the same species in the different quarters of the globe. Therefore, the system of animal life, which had been maintained in the ancient sea, had not been different from that which now subsists, and of which it belongs to naturalists to know the history.

It is the nature of animal life to be ultimately supported from matter of vegetable production. Inflammable matter may be considered as the *pabulum* of life. This is prepared in the bodies of living plants, particularly in their leaves exposed to the sun and light. This inflammable matter, on the contrary, is consumed in animal bodies, where it produces heat or light, or both. Therefore, however animal matter, or the pabulum of life, may circulate through a series of digesting powers, it is constantly impaired or diminishing in the course of this economy, and, without the productive power of plants, it would finally be extinguished.[17]

[Note 17: See Dissertations on different subjects of Natural Philosophy, part II.]

The animals of the former world must have been sustained during indefinite successions of ages. The mean quantity of animal matter, therefore, must have been preserved by vegetable production, and the natural waste of inflammable substance repaired with continual addition; that is to say, the quantity of inflammable matter necessary to the animal consumption, must have been provided by means of

vegetation. Hence we must conclude, that there had been a world of plants, as well as an ocean replenished with living animals.

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We are now, in reasoning from principles, come to a point decisive of the question, and which will either confirm the theory, if it be just, or confute our reasoning, if we have erred. Let us, therefore, open the book of Nature, and read in her records, if there had been a world bearing plants, at the time when this present world was forming at the bottom of the sea.

Here the cabinets of the curious are to be examined; but here some caution is required, in order to distinguish things perfectly different, which sometimes are confounded.

Fossil wood, to naturalists in general, is wood dug up from under ground, without inquiring whether this had been the production of the present earth, or that which had preceded it in the circulation of land and water. The question is important, and the solution of it is, in general, easy. The vegetable productions of the present earth, however deep they may be found buried beneath its surface, and however ancient they may appear, compared with the records of our known times, are new, compared with the solid land on which they grew; and they are only covered with the produce of a vegetable soil, or the alluvion of the present land on which we dwell, and on which they had grown. But the fossil bodies which form the present subject of inquiry, belonged to former land, and are found only in the sea-born strata of our present earth. It is to these alone that we appeal, in order to prove the certainty of former events.

Mineralised wood, therefore, is the object now inquired after; that wood which had been lodged in the bottom of the sea, and there composed part of a stratum, which hitherto we have considered as only formed of the materials proper to the ocean. Now, what a profusion of this species of fossil wood is to be found in the cabinets of collectors, and even in the hands of lapidaries, and such artificers of polished stones! In some places, it would seem to be as common as the agate.

I shall only mention a specimen in my own collection. It is wood petrified with calcareous earth, and mineralised with pyrites. This specimen of wood contains in itself, even without the stratum of stone in which it is embedded, the most perfect record of its genealogy. It had been eaten or perforated by those sea worms which destroy the bottoms of our ships. There is the clearest evidence of this truth. Therefore, this wood had grown upon land which flood above the level of sea, while the present land was only forming at the bottom of the ocean.

Wood is the most substantial part of plants, as shells are the more permanent part of marine animals. It is not, however, the woody part alone of the ancient vegetable world that is transmitted to us in the record of our mineral pages. We have the type of many species of foliage, and even of the most delicate flower; for, in this way, naturalists have determined, according to the Linnaean system, the species, or at least the genus, of the plant. Thus, the existence of a vegetable system at the period now in contemplation, so far from being doubtful, is a matter of physical demonstration.

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The profusion of this vegetable matter, delivered into the ocean, which then generated land, is also evidenced in the amazing quantities of mineral coal which is to be found in perhaps every region of the earth.

Nothing can be more certain, than that all the coaly or bituminous strata have had their origin from the substance of vegetable bodies that grew upon the land. Those strata, tho', in general, perfectly consolidated, often separate horizontally in certain places; and there we find the fibrous or vascular structure of the vegetable bodies. Consequently, there is no doubt of fossil coal being a substance of vegetable production, however animal substances also may have contributed in forming this collection of oleaginous or inflammable matter.

Having thus ascertained the state of a former earth, in which plants and animals had lived, as well as the gradual production of the present earth, composed from the materials of a former world, it must be evident, that here are two operations which are necessarily consecutive. The formation of the present earth necessarily involves the destruction of continents in the ancient world; and, by pursuing in our mind the natural operations of a former earth, we clearly see the origin of that land, by the fertility of which, we, and all the animated bodies of the sea, are fed. It is in like manner, that, contemplating the present operations of the globe, we may perceive the actual existence of those productive causes, which are now laying the foundation of land in the unfathomable regions of the sea, and which will, in time, give birth to future continents.

But though, in generalising the operations of nature, we have arrived at those great events, which, at first sight, may fill the mind with wonder and with doubt, we are not to suppose, that there is any violent exertion of power, such as is required in order to produce a great event in little time; in nature, we find no deficiency in respect of time, nor any limitation with regard to power. But time is not made to flow in vain; nor does there ever appear the exertion of superfluous power, or the manifestation of design, not calculated in wisdom to effect some general end.

The events now under consideration may be examined with a view to see this truth; for it may be inquired, Why destroy one continent in order to erect another? The answer is plain; Nature does not destroy a continent from having wearied of a subject which had given pleasure, or changed her purpose, whether for a better or a worse; neither does she erect a continent of land among the clouds, to show her power, or to amaze the vulgar man; Nature has contrived the productions of vegetable bodies, and the sustenance of animal life, to depend upon the gradual but sure destruction of a continent; that is to say, these two operations necessarily go hand in hand. But with such wisdom has nature ordered things in the economy of this world, that the destruction of one continent is not brought about without the renovation of the earth in the production of another; and the animal and vegetable bodies, for which the world above the surface of the sea is leveled with its bottom, are among the means employed

in those operations, as well as the sustenance of those living beings is the proper end in view.

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Thus, in understanding the proper constitution of the present earth, we are led to know the source from whence had come all the materials which nature had employed in the construction of the world which appears; a world contrived in consummate wisdom for the growth and habitation of a great diversity of plants and animals; and a world peculiarly adapted to the purposes of man, who inhabits all its climates, who measures its extent, and determines its productions at his pleasure.

The whole of a great object or event fills us with wonder and astonishment, when all the particulars, in the succession of which the whole had been produced, may be considered without the least emotion. When, for example, we behold the pyramids of Egypt, our mind is agitated with a crowd of ideas that highly entertains the person who understands the subject; but the carrying a heavy stone up to the top of a hill or mountain would give that person little pleasure or concern. We wonder at the whole operation of the pyramid, but not at any one particular part.

The raising up of a continent of land from the bottom of the sea, is an idea that is too great to be conceived easily in all the parts of its operations, many of which are perhaps unknown to us; and, without being properly understood, so great an idea may appear like a thing that is imaginary. In like manner, the co-relative, or corresponding operation, the destruction of the land, is an idea that does not easily enter into the mind of man in its totality, although he is daily witness to part of the operation. We never see a river in a flood, but we must acknowledge the carrying away of part of our land, to be sunk at the bottom of the sea; we never see a storm upon the coast, but we are informed of a hostile attack of the sea upon our country; attacks which must, in time, wear away the bulwarks of our soil, and sap the foundations of our dwellings. Thus, great things are not understood without the analysing of many operations, and the combination of time with many events happening in succession.

Let us now consider what is to be the subject of examination, and where it is that we are to observe those operations which must determine either the stability or the instability of this land on which we live.

Our land has two extremities; the tops of the mountains, on the one hand, and the sea-shores, on the other: It is the intermediate space between these two, that forms the habitation of plants and animals. While there is a sea-shore and a higher ground there is that which is required in the system of the world: Take these away, and there would remain an aqueous globe, in which the world would perish. But, in the natural operations of the world, the land is perishing continually; and this is that which now we want to understand.

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Upon the one extremity of our land, there is no increase, or there is no accession of any mineral substance. That place is the mountain-top, on which nothing is observed but continual decay. The fragments of the mountain are removed in a gradual succession from the highest station to the lowest. Being arrived at the shore, and having entered the dominion of the waves, in which they find perpetual agitation, these hard fragments, which had eluded the resolving powers natural to the surface of the earth, are incapable of resisting the powers here employed for the destruction of the land. By the attrition of one hard body upon another, the moving stones and rocky shore, are mutually impaired. And that solid mass, which of itself had potential liability against the violence of the waves, affords the instruments of its own destruction, and thus gives occasion to its actual instability.

In order to understand the system of the heavens, it is necessary to connect together periods of measured time, and the distinguished places of revolving bodies. It is thus that system may be observed, or wisdom, in the proper adapting of powers to an intention. In like manner, we cannot understand the system of the globe, without seeing that progress of things which is brought about in time, thus measuring the natural operations of the earth with those of the heavens. This is properly the business of the present undertaking.

Our object is to know the time which had elapsed since the foundation of the present continent had been laid at the bottom of the ocean, to the present moment in which we speculate on these operations. The space is long; the data for the calculations are, perhaps, deficient: No matter; so far as we know our error, or the deficiency in our operation, we proceed in science, and shall conclude in reason. It is not given to man to know what things are truly in themselves, but only what those things are in his thought. We seek not to know the precise measure of any thing; we only understand the limits of a thing, in knowing what it is not, either on the one side or the other.

We are investigating the age of the present earth, from the beginning of that body which was in the bottom of the sea, to the perfection of its nature, which we consider as in the moment of our existence; and we have necessarily another aera, which is collateral, or correspondent, in the progress of those natural events. This is the time required, in the natural operations of this globe, for the destruction of a former earth; an earth equally perfect with the present and an earth equally productive of growing plants and living animals. Now, it must appear, that, if we had a measure for the one of those corresponding operations, we would have an equal knowledge of the other.

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The formation of a future earth being in the bottom of the ocean, at depths unfathomable to man, and in regions far beyond the reach of his observation, here is a part of the process which cannot be taken as a principle in forming an estimate of the whole. But, in the destruction of the present earth, we have a process that is performed within the limits of our observation; therefore, in knowing the measure of this operation, we shall find the means of calculating what had passed on a former occasion, as well as what will happen in the composition of a future earth. Let us, therefore, now attempt to make this estimate of time and labour.

The highest mountain may be levelled with the plain from whence it springs, without the loss of real territory in the land; but when the ocean makes encroachment on the basis of our earth, the mountain, unsupported, tumbles with its weight; and with the accession of hard bodies, moveable with the agitation of the waves, gives to the sea the power of undermining farther and farther into the solid basis of our land. This is the operation which is to be measured; this is the mean proportional by which we are to estimate the age of worlds that have terminated, and the duration of those that are but beginning.

But how shall we measure the decrease of our land? Every revolution of the globe wears away some part of some rock upon some coast; but the quantity of that decrease, in that measured time, is not a measurable thing. Instead of a revolution of the globe, let us take an age. The age of man does no more in this estimate than a single year. He sees, that the natural course of things is to wear away the coast, with the attrition of the sand and stones upon the shore; but he cannot find a measure for this quantity which shall correspond to time, in order to form an estimate of the rate of this decrease.

But man is not confined to what he sees; he has the experience of former men. Let us then go to the Romans and the Greeks in search of a measure of our coasts, which we may compare with the present state of things. Here, again, we are disappointed; their descriptions of the shores of Greece and of Italy, and their works upon the coast, either give no measure of a decrease, or are not accurate enough for such a purpose.

It is in vain to attempt to measure a quantity which escapes our notice, and which history cannot ascertain; and we might just as well attempt to measure the distance of the stars without a parallax, as to calculate the destruction of the solid land without a measure corresponding to the whole.

The description which Polybius has given of the Pontus Euxinus, with the two opposite Bosphori, the Meotis, the Propontis, and the Port of Byzantium, are as applicable to the present state of things as they were at the writing of that history. The filling up of the bed of the Meotis, an event which, to Polybius, appeared not far off, must also be considered as removed to a very distant period, though the causes still continue to operate as before.

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But there is a thing in which history and the present state of things do not agree. It is upon the coast of Spain, where Polybius says there was an island in the mouth of the harbour of New Carthage. At present, in place of the island, there is only a rock under the surface of the water. It must be evident, however, that the loss of this small island affords no proper ground of calculation for the measure or rate of wasting which could correspond to the coast in general; as neither the quantity of what is now lost had been measured, nor its quality ascertained.

Let us examine places much more exposed to the fury of the waves and currents than the coast of Carthagena, the narrow fretum, for example, between Italy and Sicily. It does not appear, that this passage is sensibly wider than when the Romans first had known it. The Isthmus of Corinth is also apparently the same at present as it had been two or three thousand years ago. Scilla and Charibdis remain now, as they had been in ancient times, rocks hazardous for coasting vessels which had to pass that strait.

It is not meant by this to say, these rocks have not been wasted by the sea, and worn by the attrition of moving bodies, during that space of time; were this true, and that those rocks, the bulwarks of the land upon those coasts, had not been at all impaired from that period, they might remain for ever, and thus the system of interchanging the place of sea and land upon this globe might be frustrated. It is only meant to affirm, that the quantity which those rocks, or that coast, have diminished from the period of our history, has either been too small a thing for human observation, or, which is more probable, that no accurate measurement of the subject, by which this quantity of decrease might have been ascertained, had been taken and recorded. It must be also evident, that a very small operation of an earthquake would be sufficient to render every means of information, in this manner of mensuration, unsatisfactory or precarious.

Pliny says Italy was distant from Sicily a mile and a half; but we cannot suppose that this measure was taken any otherwise than by computation, and such a measure is but little calculated to afford us the just means of a comparison with the present distance. He also says, indeed, that Sicily had been once joined with Italy. His words are: “Quondam Brutio agro cohaerens, mox interfuso mari avulsa.[18]” But all that we can conclude from this history of Pliny is, that, in all times, to people considering the appearances of those two approached coasts, it had seemed probable, that the sea formed a passage between the two countries which had been once united; in like manner as is still more immediately perceived, in that smaller disjunction which is made between the island of Anglesey and the continent of Wales.

[Note 18: Lib. 3. cap. 8.]

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The port of Syracuse, with the island which forms the greater and lesser, and the fountain of Arethusa, the water of which the ancients divided from the sea with a wall, do not seem to be altered. From Sicily to the coast of Egypt, there is an uninterrupted course of sea for a thousand miles; consequently, the wind, in such a stretch of sea, should bring powerful waves against those coasts: But, on this coast of Egypt, we find the rock on which was formerly built the famous tower of Pharos; and also, at the eastern extremity of the port Eunoste, the sea-bath, cut in the solid rock upon the shore. Both those rocks, buffeted immediately with the waves of the Mediterranean sea, are, to all appearance, the same at this day as they were in ancient times.[19]

[Note 19: Lettres sur l'Egypte, M. Savary.]

Many other such proofs will certainly occur, where the different parts of those coasts are examined by people of observation and intelligence. But it is enough for our present purpose, that this decrease of the coasts in general has not been observed; and that it is as generally thought, that the land is gaining upon the sea, as that the sea is gaining upon the land.

To sum up the argument, we are certain, that all the coasts of the present continents are wasted by the sea, and constantly wearing away upon the whole; but this operation is so extremely slow, that we cannot find a measure of the quantity in order to form an estimate: Therefore, the present continents of the earth, which we consider as in a state of perfection, would, in the natural operations of the globe, require a time indefinite for their destruction.

But, in order to produce the present continents, the destruction of a former vegetable world was necessary; consequently, the production of our present continents must have required a time which is indefinite. In like manner, if the former continents were of the same nature as the present, it must have required another space of time, which also is indefinite, before they had come to their perfection as a vegetable world.

We have been representing the system of this earth as proceeding with a certain regularity, which is not perhaps in nature, but which is necessary for our clear conception of the system of nature. The system of nature is certainly in rule, although we may not know every circumstance of its regulation. We are under a necessity, therefore, of making regular suppositions, in order to come at certain conclusions which may be compared with the present state of things.

It is not necessary that the present land should be worn away and wasted, exactly in proportion as new land shall appear; or, conversely, that an equal proportion of new land should always be produced as the old is made to disappear. It is only required, that at all times, there should be a just proportion of land and water upon the surface of the globe, for the purpose of a habitable world.

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Neither is it required in the actual system of this earth, that every part of the land should be dissolved in its structure, and worn away by attrition, so as to be floated in the sea. Parts of the land may often sink in a body below the level of the sea, and parts again may be restored, without waiting for the general circulation of land and water, which proceeds with all the certainty of nature, but which advances with an imperceptible progression. Many of such apparent irregularities may appear without the least infringement on the general system. That system is comprehended in the preparation of future land at the bottom of the ocean, from those materials which the dissolution and attrition of the present land may have provided, and from those which the natural operations of the sea afford.

In thus accomplishing a certain end, we are not to limit nature with the uniformity of an equable progression, although it be necessary in our computations to proceed upon equalities. Thus also, in the use of means, we are not to prescribe to nature those alone which we think suitable for the purpose, in our narrow view. It is our business to learn of nature (that is by observation) the ways and means, which in her wisdom are adopted; and we are to imagine these only in order to find means for further information, and to increase our knowledge from the examination of things which actually have been. It is in this manner, that intention may be found in nature; but this intention is not to be supposed, or vainly imagined, from what we may conceive to be.

We have been now supposing, that the beginning of our present earth had been laid in the bottom of the ocean, at the completion of the former land; but this was only for the sake of distinctness. The just view is this, that when the former land of the globe had been complete, so as to begin to waste and be impaired by the encroachment of the sea, the present land began to appear above the surface of the ocean. In this manner we suppose a due proportion to be always preserved of land and water upon the surface of the globe, for the purpose of a habitable world, such as this which we possess. We thus, also, allow time and opportunity for the translation of animals and plants to occupy the earth.

But, if the earth on which we live, began to appear in the ocean at the time when the last began to be resolved, it could not be from the materials of the continent immediately preceding this which we examine, that the present earth had been constructed; for the bottom of the ocean must have been filled with materials before land could be made to appear above its surface.

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Let us suppose that the continent, which is to succeed our land, is at present beginning to appear above the water in the middle of the Pacific Ocean, it must be evident, that the materials of this great body, which is formed and ready to be brought forth, must have been collected from the destruction of an earth, which does not now appear. Consequently, in this true statement of the case, there is necessarily required the destruction of an animal and vegetable earth prior to the former land; and the materials of that earth which is first in our account, must have been collected at the bottom of the ocean, and begun to be concocted for the production of the present earth, when the land immediately preceding the present had arrived at its full extent.

This, however, alters nothing with regard to the nature of those operations of the globe. The system is still the same. It only protracts the indefinite space of time in its existence, while it gives us a view of another distinct period of the living world; that is to say, the world which we inhabit is composed of the materials, not of the earth which was the immediate predecessor of the present, but of the earth which, in ascending from the present, we consider as the third, and which had preceded the land that was above the surface of the sea, while our present land was yet beneath the water of the ocean. Here are three distinct successive periods of existence, and each of these is, in our measurement of time, a thing of indefinite duration.

We have now got to the end of our reasoning; we have no data further to conclude immediately from that which actually is: But we have got enough; we have the satisfaction to find, that in nature there is wisdom, system, and consistency. For having, in the natural history of this earth, seen a succession of worlds, we may from this conclude that there is a system in nature; in like manner as, from seeing revolutions of the planets, it is concluded, that there is a system by which they are intended to continue those 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 this physical inquiry is, that we find no vestige of a beginning,—no prospect of an end.

CHAPTER II.

An Examination of Mr KIRWAN'S Objections to the Igneous Origin of Stony Substances.

A theory which is founded on a new principle, a theory which has to make its way in the public mind by overturning the opinions commonly received by philosophising men, and one which has nothing to recommend it but the truth of its principles, and the view of wisdom or design to which it leads, neither of which may perhaps be perceived by the generality of people, such a theory, I say, must meet with the strongest opposition from the prejudices of the learned, and from the superstition

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of those who judge not for themselves in forming their notions, but look up to men of science for authority. Such is the case with some part of the Theory of the Earth, which I have given, and which will probably give offence to naturalists who have espoused an opposite opinion. In order, then, to obtain the approbation of the public, it may not be enough to give a theory that should be true, or altogether unexceptionable it may be necessary to defend every point that shall be thought exceptionable by other theorists, and to show the fallacy of every learned objection that may be made against it. It is thus, in general, that truth and error are forced to struggle together, in the progress of science; and it is only in proportion as science removes erroneous conceptions, which are necessarily in the constitution of human knowledge, that truth will find itself established in natural philosophy.

Mr Kirwan has written a dissertation, entitled, *Examination of the Supposed Igneous Origin of Stony Substances*, which was read in the Royal Irish Academy. The object of that dissertation is to state certain objections, which have occurred to him, against the Theory of the Earth published in the Transactions of the Edinburgh Royal Society; and he has attacked that theory in all the points where it appears to him to be vulnerable. It is to these objections that I am now to give an answer. The authority given to this dissertation, by the Royal Irish Academy, as well as the reputation of the author, make it necessary for me to endeavour to put in their true light the facts alleged in that performance, and to analyse the arguments employed, in order to judge of the reasoning by which the theory of mineral fusion is refuted in this Examination.

A theory founded on truth, and formed according to the proper rules of science, can ever suffer from a strict examination, by which it would be but the more and more confirmed. But, where causes are to be traced through a chain of various complicated effects, an examination not properly conducted upon accurate analytical principles, instead of giving light upon a subject in which there had been obscurity and doubt, may only serve to perplex the understanding, and bring confusion into a subject which was before sufficiently distinct. To redress that evil, then, must require more labour and some address; and this is an inconveniency that may be looked for, more or less, in every controversial discussion.

I do not mean to enter any farther into the defence of my theory in this chapter, than what is necessary to answer a man of science and respectability, who has stated his objections. The observations which he has made appear to me to be founded on nothing more than common prejudice, and misconceived notions of the subject. I am therefore to point out that erroneous train of reasoning, into which a hasty superficial view of things, perhaps, has led the patron of an opposite opinion to see my theory in an unfavourable light.

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This, however, is not all; for, that train of inconsequential reasoning is so congenial with the crude and inconsiderate notion generally entertained, of solid mineral bodies having been formed by the infiltration of water into the earth, that no opportunity should be lost of exposing an erroneous manner of reasoning, which is employed in supporting a hypothesis founded upon certain operations of the surface of this earth that cannot be properly applied to the formation of mineral bodies. This object, therefore, so far as it may come in the way, will be attended to in this discussion, although I shall have another opportunity of farther enlarging upon that subject.

Our author begins by examining a geological operation, the very opposite to that of mineral consolidation, and which would seem to have little connection with the subject of this dissertation. In my theory, I advanced two propositions with regard to the economy of this world: First, That the solid masses of this earth, when exposed to the atmosphere, decay, and are resolved into loose materials, of which the vegetable soil upon the surface is in part composed; and, secondly, That these loose materials are washed away by the currents of water, and thus carried at last into the sea. Our author says “Here are two suppositions, neither of which is grounded on facts;” and yet he has but the moment before made the following confession: “That the soil, however, receives an increase from some species of stones that moulder by exposition to the air cannot be denied, but there is no proof that all soil has arisen from decomposition.”—Surely *all soil*, that is made from the *hard and compact* body of the land, which is my proposition, must have arisen from *decomposition*; and I have no where said, that *all* the soil of this earth is made from the decomposition or detritus of those stony substances; for, masses of looser sand and softer substances contribute still more to the formation of vegetable soils.

With regard to the other proposition, our author says, “Soil is not constantly carried away by the water, even from mountains.”—I have not said that it is *constantly* washed away; for, while it is soil in which plants grow, it is not travelling to the sea, although it be on the road, and must there arrive in time. I have said, that it is *necessarily* washed away, that is, occasionally. M. de Luc’s authority is then referred to, as refuting this operation of water and time upon the soil. Now, I cannot help here observing, that our author seems to have as much misapprehended M. de Luc’s argument as he has done mine. That philosopher, in his letters to the Queen, has described most accurately the decay of the rocks and solid mountains of the Alps and Jura, and the travelling of their materials by water, although he does not carry them to the sea. It is true, indeed, that this author, who supposes the present earth on which we dwell very young, is anxious to make an earth, *in time*, that shall not decay nor be washed away at all; but that time is not come yet; therefore the authority, here given against my theory, is the speculative supposition, or mere opinion, of a natural philosopher, with regard to an event which may never come to pass, and which I shall have occasion to consider fully in another place.

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Our author had just now said, that I have advanced two suppositions, *neither of which is grounded on facts*: Now, with regard to the one, he has acknowledged, that the mouldering of stones takes place, which is the fact on which that proposition is grounded; and with regard to the other, the only authority given against it is founded expressly upon the moving of soil by means of the rain water, in order to make sloping plains of mountains. Here, therefore, I have grounded my propositions upon facts; and our author has founded his objections, first, upon a difficulty which he has himself removed; and, secondly, upon nothing but a visionary opinion, with regard to an earth which is not yet made, and which, when once made, is never more to change.

After making some unimportant observations,—of all water not flowing into the sea,—-and of the travelled materials being also deposited upon the plains, *etc*. our author thus proceeds: “Hence the conclusion of our author relative to the imperfect constitution of the globe falls to the ground; and the pains he takes to learn, *by what means a decayed world may be renovated*, are superfluous.”—The object of my theory is to show, that this decaying nature of the solid earth is the very *perfection* of its constitution, as a living world; therefore, it was most proper that I should *take pains to learn* by what means the decayed parts might be renovated. It is true, indeed, that this will be superfluous, when once that constitution of the earth, which M. de Luc thinks is preparing, shall be finished; but, in the mean time, while rivers carry the materials of our land, and while the sea impairs the coast, I may be allowed to suppose that this is the actual constitution of the earth.

I cannot help here animadverting upon what seems to be our author’s plan, in making these objections, which have nothing to do with his examination. He accuses me of giving this world a false or imperfect constitution, (in which the solid land is considered as resolvable, and the materials of that land as being washed away into the sea,) for no other reason, that I can see, but because this may imply the formation of a future earth, which he is not disposed to allow; and, he is now to deny the stratified construction of this present earth to have been made by the deposits of materials at the bottom of the sea, because that would prove the existence of a former earth, which is repugnant to his notion of the origin of things, and is contrary, as he says, to reason, and the tenor of the Mosaic history. Let me observe, in passing, that M. de Luc, of whose opinions our author expresses much approbation, thinks that he proves, from the express words and tenor of the Mosaic history, that the present earth was at the bottom of the sea not many years ago, and that the former earth had then disappeared.

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But, what does our author propose to himself, in refusing to admit my view of the operations which are daily transacting upon the surface of this earth, where there is nothing dark or in the least mysterious, as there may be in the mineral regions? Does he mean to say, that it is not the purpose of this world to provide soil for plants to grow in? Does he suppose that this soil is not moveable with the running water of the surface? and, Does he think that it is not necessary to replace that soil which is removed? This is all that I required in that constitution of the world which he has thus attacked; and I wish that he or any person would point out, in what respect I had demanded any thing unreasonable, or any thing that is not actually to be observed every day.

Thus I have endeavoured to show, that our author has attacked my theory in a part where I believe it must be thought invulnerable; but this is only, I presume, in order that he may make an attack with more advantage upon another part, *viz*. the composition of strata from the materials of an earth thus worn out in the service of vegetation,—-materials which are necessarily removed in order to make way for that change of things in which consists the active and living system of this world. If he succeed in this attempt to refute my theory of the original formation of strata, he would then doubtless find it more easy to persuade philosophers that the means which I employ in bringing those materials again to light, when transformed into such solid masses as the system of this earth requires, are extravagant, unnatural, and unnecessary. Let us then see how he sets about this undertaking.

With regard to the composition of the earth, it is quoted from my theory, that *the solid parts of the globe are in general composed of sand, gravel, argillaceous and calcareous strata, or of various compositions of these with other substances*; our author then adds, “This certainly cannot be affirmed as a fact, but rather the contrary; it holds only true of the surface, the basis of the greater part of Scotland is evidently a granitic rock, to say nothing of the continents, both of the Old and New World, according to the testimony of all mineralogists.” This proposition, with regard to the general composition of the earth, I have certainly not assumed, I have maintained it as a fact, after the most scrupulous examination of all that, with the most diligent search, I have been able to see, and of all that authors have wrote intelligibly upon the subject. If, therefore, I have so misrepresented this great geological fact on which my theory is absolutely founded, I must have erred with open eyes; and my theory of the earth, like others which have gone before it, will, upon close examination, appear to be unfounded, as the dissertation now before us is endeavouring to represent it.

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Our author here, I think, alleges that the contrary to this, my fundamental proposition, is the truth; and he has given us Scotland as an example in which his assertion (founded upon the testimony of all mineralogists), is illustrated. Now my geological proposition should certainly be applicable to Scotland, which is the country that I ought to be best acquainted with; consequently, if what our author here asserts be true, I would have deserved that blame which he is willing to throw on me. Let me then beg the readers attention for a moment, that I may justify myself from that charge, and place in its proper light this authority, upon so material a point in geology.

I had examined Scotland from the one end to the other before I saw one stone of granite in its native place, I have moreover examined almost all England and Wales, (excepting Devonshire and Cornwall) without seeing more of granite than one spot, not many hundred yards of extent; this is at Chap; and I know, from information, that there is another small spot in the middle of England where it is just seen. But, let me be more particular with regard to Scotland, the example given in proof.

I had travelled every road from the borders of Northumberland and Westmoreland to Edinburgh; from Edinburgh, I had travelled to Port-Patrick, and from that along the coast of Galloway and Airshire to Inverary in Argyleshire, and I had examined every spot between the Grampians and the Tweedale mountains from sea to sea, without seeing granite in its place. I had also travelled from Edinburgh by Grief, Rannock, Dalwhiny, Fort Augustus, Inverness, through east Ross and Caithness, to the Pentland-Frith or Orkney islands, without seeing one block of granite in its place. It is true, I met with it on my return by the east coast, when I just saw it, and no more, at Peterhead and Aberdeen; but that was all the granite I had ever seen when I wrote my Theory of the Earth. I have, since that time, seen it in different places; because I went on purpose to examine it, as I shall have occasion to describe in the course of this work.

I may now with some confidence affirm, from my own observation, and from good information with regard to those places where I have not been, except the northwest corner, I may affirm, I say, that instead of the basis of the greatest part of Scotland being a granitic rock, which our author has maintained as an evident thing, there is very little of it that is so; not perhaps one five hundred part. So far also as I am to judge from my knowledge of the mineral construction of England and Wales, which I have examined with the greatest care, and from the mineral chart which my friend Mr Watt made for me from his knowledge of Cornwall, I would say that there is scarcely one five hundred part of Britain that has granite for its basis. All the rest, except the porphyry and basaltes, consists of stratified bodies, which are composed more or less of the materials which I mentioned, generally, in the above quotation, and which our author would dispute.

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But do not let me take the advantage of this error of our author with regard to the mineralogy of Scotland, and thus draw what may be thought an undue conclusion in favour of my general theory; let us go over and examine the continent of Europe, and see if it is any otherwise there than in Britain. From the granite of the Ural mountains, to that which we find in the Pyrenees, there is no reason, so far as I have been able to learn, to conclude that things are formed either upon any other principle, or upon a different scale. But, instead of one five hundred part, let us suppose there to be one fiftieth part of the earth in general resting upon granite, I could not have expressed myself otherwise than I have done; for, when I maintained that the earth in general consisted of stratified bodies, I said that this was only *nine tenths, or perhaps ninety-nine hundredths* of the whole, and I mentioned that there were other masses of a different origin, which should be considered separately. Our author, on the contrary, asserts that the Old and New Worlds, as well as Scotland, are placed upon granite as a basis, which he says is according to the testimony of all mineralogists. I shall have occasion to examine this opinion of mineralogists, in comparing it with those masses of granite which appear to us; and I hope fully to refute the geological, as well as mineralogical notions with regard to that body. In the mean time, let me make the following reflection, which here naturally occurs.

My Theory of the Earth is here examined,—not with the system of nature, or actual state of things, to which it certainly should have corresponded,—but with the systematic views of a person, who has formed his notions of geology from the vague opinion of others, and not from what he has seen. Had the question been, How far my theory agreed with other theories, our author might very properly have informed his readers that it was diametrically opposite to the opinions of mineralogists; but, this was no reason for concluding it to be erroneous; on the contrary, it is rather a presumption that I may have corrected the error of mineralogists who have gone before me, in like manner as it is most reasonable to presume that our author may have corrected mine. Let us then proceed to examine how far this shall appear to be the case.

Our author has stated very fairly from the Theory, *viz*. *That all the strata of the earth, not only those consisting of calcareous masses, but others superincumbent on these, have had their origin at the bottom of the sea, by the collection of sand, gravel, shells, coralline, and crustaceous bodies, and of earths and clays variously mixed, separated, and accumulated.* He then adds, “Various geological observations contradict this conclusion. There are many stratified mountains of argillaceous slate, gneiss, serpentine, jasper, and even marble, in which either sand, gravel, shells, coralline, or crustaceous bodies are never, or scarce ever found.”

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Here our author seems to have deceived himself, by taking a very partial view of things which should be fully examined, and well understood, before general conclusions are to be drawn from those appearances; for, although those particular objects may not be visible in the strata which he has enumerated, or many others, they are found in those strata which are either immediately connected and alternated with them, or with similar strata; something to that purpose I think I have said; and, if I had not, it certainly requires no deep penetration to have seen this clear solution of that appearance of those objects not being found in every particular stratum. He says that those marks of known materials are never or scarce ever found;—by *scarce ever* he surely means that they are sometimes found; but if they shall only *once* be found, his argument is lost. I have not drawn my geological conclusion from every particle in strata being distinguishable, but from there being certain distinguishable particles in strata, and from our knowing what had been the former state and circumstances of those distinguished parts.

If every stone or part of a stratum, in which those known objects are not immediately visible, must be considered as so *many geological observations that contradict my theory*, (of strata being formed from the materials of a former earth), then, surely every stone and every stratum which visibly contains any of those materials, must prove my theory. But if every stratum, where these are found in any part of it, is to be concluded as having had its origin at the bottom of the sea; and, if every concomitant stratum, though not having those objects visible or sufficiently distinct, must be considered as having had the same or a similar origin, that pretended contradiction of my theory comes to no more than this, that every individual stone does not bear in it the same or equal evidence of that general proposition which necessarily results from the attentive consideration of the whole, including every part.

But to see how necessary it is to judge in this manner, not partially, but upon the whole, we may observe, that there are two ways by which the visible materials or distinguishable bodies of a former earth, not only *may* be rendered invisible in the composition of our present earth, but *must* be so upon many occasions. These are, *first*, by mechanical comminution, which necessarily happens, more or less, in that operation by which bodies are moved against one another, and thus transported from the land to the bottom of the deepest seas; *secondly*, by chemical operations, (whatever these may be, whether the action of water or of fire, or both), which are also necessarily employed for consolidating those loose materials, that are to form the rocks and stones of this earth, and by means of which those materials are to have their distinguishable shapes affected in all degrees and obliterated. Therefore, to

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demand the visible appearance of those materials in every stratum of the earth, or in every part of a stratum, is no other than to misunderstand the subject altogether. The geological observations, which have been thus alleged as contradicting my theory, are stratified bodies, containing proofs of the general origin which I attribute to the earth, but proofs which may not always be seen with equal facility as those which even convince the vulgar.

Our author has surely perplexed himself with what writers of late have said concerning primitive mountains as they are called, a subject of deeper search, than is commonly imagined, as I hope to show in the course of this work. It is an interesting subject of investigation, as giving us the actual view of those operations of nature which, in forming my Theory of the Earth, more general principles had led me to conclude *might be*. But, it is a subject which, I am afraid, will lead me to give farther offence to our author, however innocent I may be in giving nothing but what I have from nature.

The reason for saying so is this; I am blamed for having endeavoured to trace back the operations of this world to a remote period, by the examination of that which actually appears, contrary, as is alleged, “to reason, and the tenor of the Mosaic history, thus leading to an abyss, from which human reason recoils, *etc*.” In a word, (says our author), “to make use of his own expression, *We find no vestige of a beginning.* Then this system of successive worlds must have been eternal.” Such is the logic by which, I suppose, I am to be accused of atheism. Our author might have added, that I have also said—*we see no prospect of an end*; but what has all this to do with the idea of eternity? Are we, with our ideas of *time*, (or mere succession), to measure that of eternity, which never succeeded any thing, and which will never be succeeded? Are we thus to measure eternity, that boundless thought, with those physical notions of ours which necessarily limit both space and time? and, because we see not the beginning of created things, Are we to conclude that those things which we see have always been, or been without a cause? Our author would thus, inadvertently indeed, lead himself into that gulf of irreligion and absurdity into which, he alleges, I have *boldly plunged*.

In examining this present earth, we find that it must have had its origin at the bottom of the sea, although our author seems willing to deny that proposition. Farther, in examining the internal construction of this stratified and sea-born mass, we find that it had been composed of the moved materials of a former earth; and, from the most accurate and extensive examination of those materials, which in many places are indeed much disguised, we are led necessarily to conclude, that there had been a world existing, and containing an animal, a vegetable, and a mineral system. But, in thus tracing back the natural operations which

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have succeeded each other, and mark to us the course of time past, we come to a period in which we cannot see any farther. This, however, is not the beginning of those operations which proceed in time and according to the wise economy of this world; nor is it the establishing of that, which, in the course of time, had no beginning; it is only the limit of our retrospective view of those operations which have come to pass in time, and have been conducted by supreme intelligence.

My principal anxiety was to show how the constitution of this world had been wisely contrived; and this I endeavoured to do, not from supposition or conjecture, but from its answering so effectually the end of its intention, *viz*. the preserving of animal life, which we cannot doubt of being its purpose. Here then is a world that is not eternal, but which has been the effect of wisdom or design.

With regard again to the prospective view of the creation, How are we to see the end of that wise system of things which so properly fulfils the benevolent intention of its maker, —in giving sustenance to the animal part, and information to intellectual beings, who, in these works of nature, read what much concerns their peace of mind,—their intellectual happiness? What then does our author mean, in condemning that comprehensive view which I have endeavoured to take of nature? Would he deny that there is to be perceived wisdom in the system of this world, or that a philosopher, who looks into the operations of nature, may not plainly read the power and wisdom of the Creator, without recoiling, as he says, from the abyss? The abyss, from which a man of science should recoil, is that of ignorance and error.

I have thus shown, that, from not perceiving the wise disposition of things upon the surface of this earth for the preservation of vegetable bodies, our author has been led to deny the necessary waste of the present earth, and the consequent preparation of materials for the construction of another; I have also shown, that he denies the origin which I had attributed to the stratified parts of this earth, as having been the collection of moving materials from a former earth; and now I am come to consider the professed purpose of this paper, *viz*. the examination of solid stony substances which we find in those strata of our earth, as well as in more irregular masses. Here, no doubt, my theory would have been attacked with greater success, had our author succeeded in pointing out its error with regard to the original composition of those indurated bodies, to which I ascribe fusion as the cause of their solidity. For, if we should, according to our author's proposition, consider those consolidated bodies as having been originally formed in that solid state, here the door might be shut against any farther investigation; —But to what purpose?—Surely not to refute my theory, but to explode every physical inquiry farther on the subject, and thus to lead us back into the science of darkness and of scepticism. But let us proceed to see our author's sentiments on this subject.

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As I had proved from matter of fact, or the actual appearances of nature, that all the strata of the earth had been formed at the bottom of the sea, by the subsidence of those materials which either come from the decaying land, or are formed in the sea itself, it was necessary that I should consider in what manner those spongy or porous bodies of loose materials, gathered together at the bottom of the sea, could have acquired that consolidated state in which we find them, now that they are brought up to our examination. Upon this occasion, our author says, “The particles which now form the solid parts of the globe need not be supposed to have originally been either spongy or porous, the interior parts at the depth of a few miles might have been originally, as at present, a solid mass.” If, indeed, we shall make that supposition, we may then save ourselves the trouble of considering either how the strata of the earth have been formed or consolidated; for, they might have been so originally. But, how can a naturalist who had ever seen a piece of Derbyshire marble, or any other shell limestone, make that supposition? Here are, to the satisfaction of every body of common understanding who looks at them, bodies which are perfectly consolidated, bodies which have evidently been formed at the bottom of the sea, and therefore which were not originally a solid mass. Mr Bertrand, it is true, wrote a book to prove that those appearances were nothing but a *lusus naturae*; and, I suppose he meant, with our author, that those strata had been also originally, as at present, a solid mass.

With regard to the consolidation of strata, that cardinal point for discussion, our author gives the following answer: “Abstracting from his own gratuitous hypothesis, it is very easy to satisfy our author on this head; the concreting and consolidating power in most cases arises from the mutual attraction of the component particles of stones to each other.” This is an answer with regard to the *concreting power*, a subject about which we certainly are not here inquiring. Our author, indeed, has mentioned a *consolidating power*; but that is an improper expression; we are here inquiring, How the interstices, between the collected materials of strata, deposited at the bottom of the sea, have been filled with a hard substance, instead of the fluid water which had originally occupied those spaces. Our author then continues; “If these particles leave any interstices, these are filled with water, which no ways obstructs their solidity when the points of contact are numerous; hence the decrepitation of many species of stones when heated.”

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If I understand our author's argument, the particles of stone are, by their mutual attractions, to leave those hard and solid bodies which compose the strata, that is to say, those hard bodies are to dissolve themselves; but, To what purpose? This must be to fill up the interstices, which we must suppose occupied by the water. In that case, we should find the original interstices filled with the substances which had composed the strata, and we should find the water translated into the places of those bodies; here would be properly a transmutation, but no consolidation of the strata, such as we are here to look for, and such as we actually find among those strata. It may be very easy for our author to form those explanations of natural phenomena; it costs no tedious observation of facts, which are to be gathered with labour, patience, and attention; he has but to look into his own fancy, as philosophers did in former times, when they saw the abhorrence of a vacuum and explained the pump. It is thus that we are here told the consolidation of strata *arises from the mutual attraction of the component particles of stones to each other*; the power, by which the particles of solid stony bodies retain their places in relation to each other, and resist separation from the mass, may, no doubt, be properly enough termed their mutual attractions; but we are not here inquiring after that power; we are to investigate the power by which the particles of hard and stony bodies had been separated, contrary to their mutual attractions, in order to form new concretions, by being again brought within the spheres of action in which their mutual attractions might take place, and make them one solid body. Now, to say that this is by their mutual attraction, is either to misunderstand the proper question, or to give a most preposterous answer.

It is not every one who is fit to reason with regard to abstract general propositions; I will now, therefore, state a particular case, in illustration of that proposition which has been here so improperly answered. The strata of Derbyshire marbles were originally immense collections at the bottom of the sea, of calcareous bodies consisting almost wholly of various fragments of the *entrochi*; and they were then covered with an indefinite number of other strata under which these *entrochi* must have been buried. In this original state of those strata, I suppose the interstices between the fragments of the coralline bodies to have been left full of sea-water; at present we find those interstices completely filled with a most perfectly solid body of marble; and the question is, whether that consolidating operation has been the work of water and solution, by our naturalist's termed infiltration; or if it has been performed, as I have maintained, by the softening power or heat, or introduction of matter in the fluid state of fusion. Our author does not propose any other method for the consolidation of those loose and incoherent bodies,

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but he speaks of the *mutual attraction of the component particles of stone to each other*; Will that fill the interstices between the coralline bodies with solid marble, as well as consolidate the coralline bodies themselves? or, if it should, How are those interstices to be thus filled with a substance perfectly different from the deposited bodies, which is also frequently the case? But, how reason with a person who, with this consolidation of strata, confounds the well known operation by which the mortar, made with caustic lime and sand, becomes a hard body! One would imagine that he were writing to people of the last age, and not to chemical philosophers who know so well how that mortar is concreted.

To my argument, That these porous strata are found *consolidated with every different species of mineral substance*, our author makes the following observation: "Here the difficulties to the supposition of an aqueous solution are placed in the strongest light; yet it must be owned that they partly arise from the author's own gratuitous supposition, that strata existed at the bottom of the sea previous to their consolidation;"—gratuitous supposition!—so far from being a supposition of any kind, it is a self evident proposition; the terms necessarily imply the conclusion. I beg the readers attention for a moment to this part of our author's animadversion, before proceeding to consider the whole; for, this is a point so essential in my theory, that if it be a gratuitous supposition, as is here asserted, it would certainly be in vain to attempt to build upon it the system of a world.

That strata may exist, whether at the bottom of the sea, or any other where, without being consolidated, will hardly be disputed; for, they are actually found consolidated in every different degree. But, when strata are found consolidated, at what time is it that we are to suppose this event to have taken place, or this accident to have happened to them?—Strata are formed at the bottom of water, by the subsidence or successive deposits of certain materials; it could not therefore be during their formation that such strata had been consolidated; consequently, we must necessarily *conclude*, without any degree of *supposition*, that *strata had existed at the bottom of the sea previous to their consolidation*, unless our author can show how they may have been consolidated previous to their existing.

This then is what our author has termed a gratuitous supposition of mine, and which, he adds, "is a circumstance which will not be allowed by the patrons of the aqueous origin of stony substances, as we have already seen."—I am perfectly at a loss to guess at what is here alluded to *by having been already seen*, unless it be that which I have already quoted, concerning things which have been never seen, that is, *those interior parts of the earth which were originally a solid mass*.—I have hardly patience to answer such reasoning;—a reasoning which is not founded upon any principle, which holds up nothing but chimera to our view, and which ends in nothing that is intelligible;—but, others, perhaps, may see this dissertation of our author's in a different light; therefore, it is my duty to analyse the argument, however insignificant it may seem to me.

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I have minutely examined all the stratified bodies which I have been able, during a lifetime, to procure, both in this country of Britain, and from all the quarters of the globe; and the result of my inquiry has been to conclude, that there is nothing among them in an original state, as the reader will see in the preceding chapter. With regard again to the masses which are not stratified, I have also given proof that they are not in their original state, such as granite, porphyry, serpentine, and basaltes; and I shall give farther satisfaction, I hope, upon that head, in the course of this work. I have therefore concluded, That there is nothing to be found in an original state, so far as we see, in the construction of this earth. But, our author answers, That the interior parts *might have been in an original state of solidity*.—So might they have been upon the surface of the earth, or on the summits of our mountains; but, we are not inquiring What they *might have been*, but What they truly *are*. It is from this actual state in which the solid parts of the earth are found, that I have endeavoured to trace back the different states in which they must have been; and, by generalising facts, I have formed a theory of the earth. If this be a wrong principle or manner of proceeding in a physical investigation, or if, proceeding upon that principle, I have made the induction by reasoning improperly on any occasion, let this be corrected by philosophers, who may reason more accurately upon the subject. But to oppose a physical investigation with this proposition, *that things might have been otherwise*, is to proceed upon a very different principle,—a principle which, instead of tending to bring light out of darkness, is only calculated to extinguish that light which we may have acquired.

I shall afterwards have occasion to examine how far the philosophers, who attribute to aqueous solution the origin of stony substances, have proceeded in the same inductive manner of reasoning from effect to cause, as they ought to do in physical subjects, and not by feigning causes, or following a false analogy; in the mean time, I am to answer the objections which have been made to the theory of the earth.

In opposition to the theory of consolidating bodies by fusion, our author has taken great pains to show, that I cannot provide materials for such a fire as would be necessary, nor find the means to make it burn had I those materials. Had our author read attentively my theory he would have observed, that I give myself little or no trouble about that fire, or take no charge with regard to the procuring of that power, as I have not founded my theory on the *supposition* of subterraneous fire, however that fire properly follows as a conclusion from those appearances on which the theory is founded. My theory is founded upon the general appearances of mineral bodies, and upon this, that mineral bodies must necessarily have

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been in a state of fusion. I do not pretend to prove, demonstratively, that they had been even hot, however that conclusion also naturally follows from their having been in fusion. It is sufficient for me to demonstrate, That those bodies must have been, more or less, in a state of softness and fluidity, without any species of solution. I do not say that this fluidity had been without heat; but, if that had been the case, it would have answered equally well the purpose of my theory, so far as this went to explain the consolidation of strata or mineral bodies, which, I still repeat, must have been by simple fluidity, and not by any species of solution, or any other solvent than that universal one which permeates all bodies, and which makes them fluid.

Our author has justly remarked the difficulty of fire burning below the earth and sea. It is not my purpose here to endeavour to remove those difficulties, which perhaps only exist in those suppositions which are made on this occasion; my purpose is to show, that he had no immediate concern with that question, in discussing the subject of the consolidation which we actually find in the strata of the earth, unless my theory, with regard to the igneous origin of stony substances, had proceeded upon the supposition of a subterraneous fire. It is surely one thing to employ fire and heat to melt mineral bodies, in supposing this to be the cause of their consolidation, and another thing to acknowledge fire or heat as having been exerted upon mineral bodies, when it is clearly proved, from actual appearances, that those bodies had been in a melted state, or that of simple fluidity. Here are distinctions which would be thrown away upon the vulgar; but, to a man of science, who analyses arguments, and reasons strictly from effect to cause, this is, I believe, the proper way of coming at the truth. If the patrons of the aqueous origin of stony substances can give us any manner of scientifical, *i.e.* intelligible investigation of that process, it shall be attended to with the most rigid impartiality, even by a patron of the igneous origin of those substances, as he wishes above all things to distinguish, in the mineral operations, those which, on the one hand, had been the effect of water, from those which, on the other hand, had been the immediate effect of fire or fusion;—this has been my greatest study. But, while mineralists or geologists give us only mere opinions, What is science profited by such inconsequential observations, as are founded upon nothing but our vulgar notions? Is the figure of the earth, *e.g.* to be doubted, because, according to the common notion of mankind, the existence of an antipod is certainly to be denied?

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I am not avoiding to meet that question with regard to the providing of materials for such a mineral fire as may be required; no question I desire more to be asked to resolve; but it must not be in the manner that our author has put that question. He has included this supposed difficulty among a string of other arguments by which he would refute my theory with regard to the igneous origin of stony substances, as if I had made that fire a necessary condition or a principle in forming my theory of consolidation. Now, it is precisely the reverse; and this is what I beg that mineral philosophers will particularly attend to, and not give themselves so much unnecessary trouble, and me so disagreeable a talk. I have proved that those stony substances have been in the fluid state of fusion; and from this, I have inferred the former existence of an internal heat, a subterraneous fire, or a certain cause of fusion by whatever name it shall be called, and by whatever means it shall have been procured. The nature of that operation by which strata had been consolidated, like that by which they had been composed, must, according to my philosophy, be decided by ocular demonstration; from examining the internal evidence which is to be found in those bodies as we see them in the earth; because the consolidating operation is not performed in our sight, no more than their stratification which our author has also denied to have been made, as I have said, by the deposits of materials at the bottom of the sea. Now, with regard to the means of procuring subterraneous fire, if the consolidating operation shall be thus decided to have been that of fusion, as I think I have fully shown, and for which I have as many witnesses, perhaps as there are mineral bodies, then our author's question, (how I am to procure a fire) in the way that he has put it, as an argument against the fusion, would be at least useless; for, though I should here confess my ignorance with regard to the means of procuring fire, the evidence of the melting operation, or former fluidity of those mineral bodies, would not be thereby in the least diminished. If again no such evidence for the fusion of those bodies shall appear, and it be concluded that they had been consolidated by the action of water alone, as our author seems inclined to maintain, he would have no occasion to start difficulties about the procuring of fire, in order to refute a theory which then would fall of itself as having no foundation.

But in order to see this author's notion of the theory which he is here examining, it may be proper to give a specimen of his reasoning upon this subject of heat. He says, "That my supposition of heat necessary for consolidating strata is *gratuitous*, not only because it is unnecessary, as we have already shown, but also because it is inconsistent with our author's own theory." Let us now consider those two propositions. *First*, it is unnecessary, *as we have already shown*;—I have

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already taken particular notice of what we have been shown on this occasion, *viz*. That the earth at a certain depth *may have been originally in a solid state*; and, that, where it is to be consolidated, this is done by the *mutual attraction of the stony particles*. Here is all that we have been shown to make subterraneous heat, for the consolidation of strata, unnecessary; and now I humbly submit, if this is sufficient evidence, that mineral heat is a gratuitous supposition.

Secondly, "*it is inconsistent with our author's own theory.*" Here I would beg the readers attention to the reasoning employed on this occasion. He says, "according to him these strata, which were consolidated by heat, were composed of materials gradually worn from a preceding continent, casually and successively deposited in the sea; Where then will he find, and how will he suppose, to have been formed those enormous masses of sulphur, coal, or bitumen, necessary to produce that immense heat necessary for the fusion of those vast mountains of stone now existing? All the coal, sulphur, and bitumen, now known, does not form the 100,000 part of the materials deposited within one quarter of a mile under the surface of the earth; if, therefore, they were, as his hypothesis demands, carried off and mixed with the other materials, and not formed in vast and separate collections, they could never occasion, by their combustion, a heat capable of producing the smallest effect, much less those gigantic effects which he requires."

Here is a comparative estimate formed between two things which have not any necessary relation; these are, the quantity of combustible materials found in the earth, on the one hand, and the quantity which is supposed necessary for hardening and consolidating strata, on the other. If this earth has been consolidated by the burning of combustible materials, there must have been a superfluity, so far as there is a certain quantity of these actually found unconsumed in the strata of the earth. Our author's conclusion is the very opposite; let us then see how he is to form his argument, by which he proves that the supposition of subterraneous heat for hardening bodies is gratuitous and unnecessary, as being inconsistent with my theory.

According to my theory, the strata of this earth are composed of the materials which came from a former earth; particularly these combustible strata that contain plants which must have grown upon the land. Let us then suppose the subterraneous fire supplied with its combustible materials from this source, the vegetable bodies growing upon the surface of the land. Here is a source provided for the supplying of mineral fire, a source which is inexhaustible or unlimited, unless we are to circumscribe it with regard to time, and the necessary ingredients; such as the matter of light, carbonic matter, and the hydrogenous principle. But it is not upon any deficiency of this kind that

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our author founds his estimate; it is upon the superfluity of combustible materials which is actually found in this earth, after it had been properly consolidated and raised above the surface of the sea. This is a method of reasoning calculated to convince only those who do not understand it; it is as if we should conclude that a person had died of want, because he had left provision behind him. Our author certainly means to employ nothing but the combustible minerals of the present earth, in feeding the subterraneous fire which is to concoct a future earth; in that case, I will allow that his provision is deficient; but this is not my theory.

I am not here to enter into any argument concerning subterraneous fire; the reader will find, in the foregoing theory, my reasons for concluding, That subterraneous fire had existed previous to, and ever since, the formation of this earth,—that it exists in all its vigour at this day,—that there is, in the constitution of this earth, a superfluity of subterranean heat,—and that there is wisely provided a proper remedy against any destructive effect to the system, that might arise from that superabundant provision of this necessary agent. Had our author attended to the ocular proof that we have of the actual existence of subterraneous fire, and to the physical demonstrations which I have given of the effects of heat in melting mineral bodies, he must have seen that those arguments of his, with regard to the difficulty or impossibility of procuring that fire, can only show the error of his reasoning. I am far from supposing that my theory may be free from inconsistency or error; I am only maintaining that, in all his confident assertions, this author has not hitherto pointed any of these out.

So far I have answered our author’s objections as to consolidation, and I have given a specimen of his reasoning upon that subject; but with regard to my Theory of the Earth, although simple fluidity, without heat, would have answered the purpose of consolidating strata that had been formed at the bottom of the sea, it was necessary to provide a power for raising those consolidated strata from that low place to the summits of the continents; now, in supposing heat to be the cause of that fluidity which had been employed in the consolidation of those submarine masses, we find a power capable of erecting continents, and the only power, so far as I see, which natural philosophy can employ for that purpose. Thus I was led, from the consolidation of strata, to understand the nature of the elevating power, and, from the nature of that power, again to understand the cause of fluidity by which the rocks and stones of this earth had been consolidated.

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Having thus, without employing the evidence of any fire or *burning*, been necessarily led to conclude an extreme degree of heat exerted in the mineral regions, I next inquire how far there are any appearances from whence we might conclude whether that active subterraneous power still subsists, and what may be the nature of that power. When first I conceived my theory, naturalists were far from suspecting that basaltic rocks were of volcanic origin; I could not then have employed an argument from these rocks as I may do now, for proving that the fires, which we see almost daily issuing with such force from volcanos, are a continuation of that active cause which has so evidently been exerted in all times, and in all places, so far as have been examined of this earth.

With regard to the degree of heat in that subterraneous fire, our author, after proving that combustible materials would not burn in the mineral regions, then says, that suppose they were to burn, this would be “incapable of forming a heat even equal to that of our common furnaces, as Mr Dolomieu has clearly shown to be the case with respect to volcanic heat.” The place to which he alludes, I believe to be that which I have quoted from the Journal de Physique (Part I. page 139) to which I here beg leave to refer the reader. After what I have already said, this subject will appear to be of little concern to me; but, it must be considered, that my object, in these answers, is not so much to justify the theory which I have given, as it is to remove that prejudice which, to those who are not master of chemical and mineral subjects, will naturally arise from the opinion or authority of a scientific man, and a chemist; therefore, I think it my business to show how much he has misconceived the matter which he treats of, and how much he misunderstands the subject of my theory.

Mr Dolomieu alleges that the volcanic fire operates in the melting of bodies, not by the intensity of its heat, which is the means employed by us in our operations, but in the long continuance of its action. But in that proposition, this philosopher is merely giving us his opinion; and, this opinion our author mistakes, I suppose, for the fact on which that opinion had been (perhaps reasonably) founded. The reader will see, in the place quoted, or in the *avant-propos* to his *Memoire sur les Iles Ponces*, the fact to be this; That the Chevalier Dolomieu finds those bodies which we either cannot melt in our fires, or which we cannot melt without changing them by calcination and vitrification, he finds, I say, these substances had actually been melted with his lavas; he also finds those substances, which are necessarily dissipated in our fires, to have been retained in those melted mineral substances. Had our author quoted the text, instead of giving us his own interpretation, he could not have offered a stronger confirmation of my theory; which certainly is not concerned with

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the particular intensity of volcanic fire, and far less with what may be the opinion of any naturalist with regard to that intensity, but only with the efficacy of that volcanic heat for the melting of mineral substances. Now this efficacy of volcanic fire, so far as we are to found upon the authority given on this occasion, is clearly confirmed by the observations of a most intelligent mineralist, and one who is actually a patron of the opposite theory to that which I have given. This being the state of the case, Must I not conclude, that our author has misunderstood the subject, and that he has been led to give a mutilated opinion of Mr Dolomieu, in order to refute my theory, when either the entire opinion, or the facts on which the opinion had been founded, would have confirmed it?

I have thus endeavoured to put in its true light a species of reasoning, which, while it assumes the air and form of that inductive train of thought employed by men of science for the investigation of nature, is only fit to mislead the unwary, and, when closely examined, will appear to be inconsequential or unfounded. How mortifying then to find, that one may be employed almost a lifetime in generalising the phenomena of nature, or in gathering an infinity of evidence for the forming of a theory, and that the consequence of this shall only be to give offence, and to receive reproach from those who see not things in the same light!—While man has to learn, mankind must have different opinions. It is the prerogative of man to form opinions; these indeed are often, commonly I may say, erroneous; but they are commonly corrected, and it is thus that truth in general is made to appear.

I wrote a general Theory for the inspection of philosophers, who doubtless will point out its errors; but this requires the study of nature, which is not the work of a day; and, in this political age, the study of nature seems to be but little pursued by our philosophers. In the mean time, there are, on the one hand, sceptical philosophers, who think there is nothing certain in nature, because there is misconception in the mind of man; on the other hand, there are many credulous amateurs, who go to nature to be entertained as we go to see a pantomime: But there are also superficial reasoning men, who think themselves qualified to write on subjects on which they may have read in books,—-subjects which they may have seen in cabinets, and which, perhaps, they have just learned to name; without truly knowing what they see, they think they know those regions of the earth which never can be seen; and they judge of the great operations of the mineral kingdom, from having kindled a fire, and looked into the bottom of a little crucible.

In the Theory of the Earth which was published, I was anxious to warn the reader against the notion that subterraneous heat and fusion could be compared with that which we induce by our chemical operations on mineral substances here upon the surface of the earth; yet, notwithstanding all the precaution I had taken, our author has bestowed four quarto pages in proving to me, that our fires have an effect upon mineral substances different from that of the subterraneous power which I would employ.

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He then sets about combining metals with sulphur in the moist way, as if that were any more to his purpose than is the making of a stalactite for the explanation of marble. Silver and lead may be sulphurated, as he says, with hepatic gas; but, Has the sulphurated solid ores of those metals, and that of iron, been formed in the moist way, as in some measure they may be by the fusion of our fires? But, even suppose that this were the case, Could that explain a thousand other appearances which are inconsistent with the operation of water? We see aerated lead dissolved in the excavations of our mines, and again concreted by the separation of the evaporated solvent, in like manner as stalactical concretions are made of calcareous earth; but, so far from explaining mineral appearances, as having had their concretions formed in the same manner, here is the most convincing argument against it; for, among the infinite variety of mineral productions which we find in nature, Why does no other example of aqueous concretion ever occur upon the surface of the earth except those which we understand so well, and which we therefore know cannot be performed in the bodies of strata not exposed to the evaporation of the solvent, a circumstance which is necessary.

I have given a very remarkable example of mineral fusion, in reguline manganese, (as the reader will see in page 68.) It is not that this example is more to the purpose of my theory than what may be found in every species of stone; but this example speaks so immediately to the common sense of mankind, (who are often convinced by a general resemblance of things, when they may not see the force of demonstration from an abstract principle) that I thought it deserved a place on that account, as well as being a curious example, But more particularly to my antagonist, who has been pleased (very improperly indeed) to try some part of my theory in the fire, here is an example which should have been absolutely in point, and without any manner of exception:—Has he acknowledged this?—No; he has, on the contrary, endeavoured to set this very example aside.

On this occasion, he says, “Manganese has been found in a reguline state by M. de la Peyrouse, and in small grains, as when produced by fire. True; but it was mixed with a large quantity of iron, which is often, found in that form without any suspicion of fusion. A fire capable of melting quartz might surely produce it in larger masses.” We have here a kind of two arguments, for removing the effect of this example; and I shall consider them separately.

The first of these is, the not being suspected of having been in fusion; now, if this were to be admitted as an argument against the igneous origin of stony substances, it might have superseded the adducing of any other, for it is applicable perhaps to every mineral; but we must here examine the case more minutely.

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This argument, of the manganese being in a mine of iron, if I understand it rightly, amounts to this, that, as iron ore is not suspected of having been melted, therefore, we should doubt the manganese having been so. If this be our author's meaning, it is not the fair conclusion which the case admits of; for, so far as the manganese appears evidently to have been in a melted state, the iron ore should be *suspected* of having been also in fusion, were there no other evidence of that fact. In science, however, it is not suspicion that should be employed in physical investigation; the question at present is; If the phenomena of the case correspond to the conclusion which the intelligent mineralist, who examined them, has formed? and, to this question, our author gives no direct answer. He says, *iron is often found in that form without any suspicion of fusion*. This is what I am now to answer.

The form in which the manganese appears is one of the strongest proofs of those masses having been in fusion; and, if iron should ever be found in that form, it must give the same proof of mineral fusion as this example of manganese; let us then see the nature of this evidence. The form of the manganese is that of a fluid body collecting itself into a spherical figure by the cohesion or attraction of its particles, so far as may be admitted by other circumstances; but, being here refilled by the solid part on which it rests, this spherical body is flattened by the gravitation of its substance. Now here is a regular form, which demonstrates the masses to have been in the state of fusion; for, there is no other way in which that form of those reguline masses could have been induced.

There now remains to be considered what our author has observed respecting the intensity of the fire and size of the masses. "A fire capable of melting quartz might surely produce it (meaning the manganese) in larger masses." M. de la Peyrouse says, that those masses were in all respects as if formed by art, only much larger, as the powers of nature exceed those of our laboratories. What then is it that is here meant to be disputed? We are comparing the operation of nature and that of art, and these are to be judged of by the product which we examine; but the quantity, in this case, or the size of the masses, makes no part of the evidence, and therefore is here most improperly mentioned by our author. With regard again to the nature of the fire by which the fusion had been produced, he is much mistaken if he imagines that the reduction of the reguline or metallic manganese depends upon the intensity of the heat; it depends upon circumstances proper for the separation of the oxygenating principle from the calx, in like manner as the calcination of calcareous spar must depend upon circumstances proper for allowing the separation of the carbonic acid or fixed air.

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But do not let us lose sight of our proper subject, by examining things foreign or not so immediately to the purpose. We are only inquiring if those flattened spheres of native manganese had been formed by water, or if it were by fusion; for, our author agrees that there is no other way. Why then does he endeavour to evade giving a direct answer, and fly away to consider the quantity of the product, as if that had any thing to do with, the question, or as if that quantity were not sufficient, neither of which is the case. In short, our author's whole observation, on this occasion, looks as if he were willing to destroy, by insinuation, the force of an argument which proves the theory of mineral fusion; and that he wishes to render doubtful, by a species of sophistry, what in fair reasoning he cannot deny.

Our author has written upon the subject of phlogiston; one would suppose that he should be well acquainted with inflammable bodies at least; let us see then what he has to observe upon that subject. He quotes from my Theory, that spar, quartz, pyrites, crystallised upon or near each other, and adhering to coal, or mixed with bitumen, *etc*. are found; circumstances that cannot be explained in the hypothesis of solution in the moist way.—He then answers;—"Not exactly, nor with certainty; which is not wonderful: But they are still less explicable in the hypothesis of dry solution, as must be apparent from what has been already said. How coal, an infusible substance, could be spread into strata by mere heat, is to me incomprehensible."—It is only upon the last sentence that I am here to remark: This, I believe, will be a sufficient specimen of our author's understanding, with regard at least to my Theory which he is here examining.

The reader will see what I have said upon the subject of coal, by turning back to the second section of the preceding chapter. I had given almost three quarto pages upon that subject, endeavouring to explain how all the different degrees of *infusibility* were produced, by means of heat and distillation, in strata which had been originally more or less oily, bituminous, and *fusible*; and now our author says, that it is incomprehensible to him, how coal, *an infusible substance*, could be spread into strata by mere heat.—So it truly may, either to him or to any other person; but, it appears to me almost as incomprehensible, how a person of common understanding should read my Dissertation, and impute to it a thing so contrary to its doctrine.

Nothing can better illustrate the misconceived view that our author seems to have taken of the two opposite theories, (*i. e*. of consolidation by means of heat, and by means of water alone,) than his observation upon the case of mineral alkali. To that irrefragable argument (which Dr Black suggested) in proof of this substance having been in a state of fusion in the mineral regions, our author makes the following reply;

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“What then will our author say of the vast masses of this salt which are found with their full quantity of water of crystallization?”—There is in this proposition, insignificant as it may seem, a confusion of ideas, which it certainly cannot be thought worth while to investigate; but, so far as the doctrine of the aqueous theory may be considered as here concerned, it will be proper that I should give some answer to the question so triumphantly put to me.

Our author is in a mistake in supposing that Dr Black had written any thing upon the subject; he had only suggested the argument of this example of mineral alkali to me, as I have mentioned; and, the use I made of that argument was to corroborate the example I had given of sal gem. If, therefore, our author does not deny the inference from the state of that mineral alkali, his observation upon it must refer to something which this other example of his is to prove on the opposite side, or to support the aqueous instead of the igneous theory; and, this is a subject which I am always willing to examine in the most impartial manner, having a desire to know the true effect of aqueous solution in the consolidation of mineral bodies, and having no objection to allow it any thing which it can possibly produce, although denying that it can do every thing, as many mineralists seem to think.

The question, with regard to this example of our author’s of a mineral alkali with its water of crystallization, must be this, Whether those saline bodies had been concreted by the evaporation of the aqueous solvent with which they had been introduced, or by the congelation of that saline substance from a fluid state of fusion; for, surely, we are not to suppose those bodies to have been created in the place and state in which we find them. With regard to the evaporation or separation of the aqueous solvent, this may be easily conceived according to the igneous theory; but, the aqueous theory has not any means for the producing of that effect in the mineral regions, which is the only place we are here concerned with. Therefore, this example of a concreted body of salt, whatever it may prove in other respects, can neither diminish the evidence of my Theory with regard to the igneous origin of stony substances, nor can it contribute to support the opposite supposition of an aqueous origin to them.

But to show how little reason our author had for exulting in that question which he so confidently proposed in order to defeat my argument, let us consider this matter a little farther. I will for a moment allow the aqueous theory to have the means for separating the water from the saline solution, and thus to concrete the saline substance in the bowels of the earth; this concretion then is to be examined with a view to investigate the last state of this body, which is to inform us with regard to those mineral operations. But, our author has not mentioned whether those masses appear to have been crystallised from the aqueous

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solution, or if they appear to have been congealed from the melted state of their *aqueous fusion*.—Has he ever thought of this? Now this is so material a point in the view with which that example has been held out to us, that, without showing that this salt had crystallised from the solution, he has no right to employ it as an example; and if, on the other hand, it should appear to have simply congealed from the state of aqueous fusion, then, instead of answering the purpose for which our author gave it, it would refute his supposition, as certainly as the example which I have given.

So far I have reasoned upon the supposition of this alkali, with its water of crystallization, being truly a mineral concretion; but, I see no authority for such a supposition: It certainly may be otherwise; and, in that case, our author would have no more right to give it as an example in opposition to Dr Black's argument, than he would have to give the crystallization of sea-salt, on Turk's Island, in opposition to the example which I had given, of the salt rock, at Northwych in Cheshire, having been in the state of fusion.

It certainly was incumbent on our author to have informed us, if those masses of salt were found in, what may be properly termed, their mineral state; or, if the state in which they are found at present had been produced by the influences of the atmosphere, transforming that saline substance from its mineral state, as happens upon so many other occasions; I am inclined to suspect that this last is truly the case. It may be thought illiberal in me to suppose a natural philosopher thus holding out an example that could only serve to lead us into error, or to mislead our judgment with regard to those two theories which is the subject of consideration. This certainly would be the case, almost on any other occasion; but, when I find every argument and example, employed in this dissertation, to be either unfounded or misjudged, Whether am I to conclude our author, on this occasion, to be consistent with himself, or not?

I have but one article more to observe upon. I had given, as I thought, a kind of demonstration, from the internal evidence of the stone, that granite had been in the fluid state of fusion, and had concreted by crystallization and congelation from that melted state. This no doubt must be a stumbling block to those who maintain that granite mountains are the primitive parts of our earth; and who, like our author, suppose that "things may have been originally, as at present, in a solid state." It must also be a great, if not an invincible obstacle in the way of the aqueous theory, which thus endeavours to explain those granite veins that are found traversing strata, and therefore necessarily of a posterior formation.

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To remove that obstacle in the way of the aqueous theory, or to carry that theory over the obstacle which he cannot remove, our author undertakes to refute my theory with regard to the igneous origin of stony substances, by giving an example of granite formed upon the surface of the earth by means of water, or in what is called the moist way; and he closes his Dissertation with this example as an *experimentum crucis*. It is therefore necessary that I take this demonstration of our author into particular consideration; for, surely, independent of our controversy, which is perhaps of little moment, here is the most interesting experiment, as it is announced, that mineralogy could be enriched with.

“To close this controversy,” says our author, “I shall only add, that granite, recently formed in the moist way, has been frequently found.”—Of that remarkable event, however, he has selected only one example. This is to be found upon the Oder; and the authority upon which our author has given it, is that of Lasius Hartz.

The formation of a granite stone, from granite sand, by means of water, is inconsistent with our chemical knowledge of those mineral substances which constitute that stone; it is repugnant to the phenomena which appear from the inspection of the natural bodies of this kind; and it is directly contrary to the universal experience in granite countries, where, instead of any thing concreting, every thing is going into decay, from the loose stones and sand of granite, to the solid rock and mountains which are always in a state of degradation. Therefore, to have any credit given to such a story, would require the most scientific evidence in its favour. Now, in order that others may judge whether this has been the case in this example, I will transcribe what our author has said upon the subject; and then I will give the view in which it appears to me.

He says, “a mole having been constructed in the Oder in the year 1723, 350 feet long, 54 feet in height, 144 feet broad at bottom, and 54 at the top, its sides only were granite, without any other cement than moss; the middle space was entirely filled with granite sand. In a short time this concreted into a substance so compact as to be impenetrable by water.”—Here is an example, according to our author, of *granite formed in the moist way*. But now, I must ask to see the evidence of that fact; for, from what our author has told us, I do not even see reason to conclude that there was the least concretion, or any stone formed at all. A body of sand will be *so compacted as to be impenetrable by water*, with the introduction of a very little mud, and without any degree of concretion; muddy water, indeed, cannot be made to pass through such a body without compacting it so; and this every body finds, to their cost, who have attempted to make a filter of that kind.

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But I shall suppose Lasius has informed our author that there had been a petrifaction in this case; and, before I admit this example of the formation of granite, I must ask what sort of a granite it was;—whether of two, three, or four ingredients; and, how these were disposed. If, again, it were not properly a granite, but a stone formed of granite sand, What is the cementing substance?—Is it quartz, felt-spar, mica, or schorl?—or, Was it calcareous? If our author knows any thing about these necessary questions, Why has he not informed us, as minutely as he has done with regard to the dimensions of the mole, with which we certainly are less concerned? If, again, he knows no more about the matter than what he has informed us of, he must have strangely imposed upon himself, to suppose that he was giving us an example of the *formation of granite in the moist way*, when he has only described an effectual way of retaining water, by means of sand and mud.

CHAP. III.

Of Physical Systems, and Geological Theories, in general.

In the first chapter I have given a general theory of the earth, with such proofs as I thought were sufficient for the information of intelligent men, who might satisfy themselves by examining the facts on which the reasoning in that theory had been founded.

In the second chapter, I have endeavoured to remove the objections which have been made to that theory, by a strenuous patron of the commonly received opinion of mineralogists and geologists,—an opinion which, if not diametrically opposite, differs essentially from mine. But now I am to examine nature more particularly, in order to compare those different opinions with the actual state of things, on which every physical theory must be founded. Therefore, the opinions of other geologists should be clearly stated, that so a fair comparison may be made of theories which are to represent the system of this earth.

Now, if I am to compare that which I have given as a theory of the earth, with the theories given by others under that denomination, I find so little similarity, in the things to be compared, that no other judgment could hence be formed, perhaps, than that they had little or no resemblance. I see certain treatises named Theories of the Earth; but, I find not any thing that entitles them to be considered as such, unless it be their endeavouring to explain certain appearances which are observed in the earth. That a proper theory of the earth should explain all those appearances is true; but, it does not hold, conversely, that the explanation of an appearance should constitute a theory of the earth. So far as the theory of the earth shall be considered as the philosophy or physical knowledge of this world, that is to say, a general view of the means by which the end or purpose is attained, nothing can be properly esteemed such a theory unless

it lead, in some degree, to the forming of that general view of things. But now, let us see what we have to examine in that respect.

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We have, first, Burnet's Theory of the Earth. This surely cannot be considered in any other light than as a dream, formed upon the poetic fiction of a golden age, and that of iron which had succeeded it; at the same time, there are certain appearances in the earth which would, in a partial view of things, seem to justify that imagination. In Telliamed, again, we have a very ingenious theory, with regard to the production of the earth above the surface of the sea, and of the origin of those land animals which now inhabit that earth. This is a theory which has something in it like a regular system, such as we might expect to find in nature; but, it is only a physical romance, and cannot be considered in a serious view, although apparently better founded than most of that which has been wrote upon the subject.

We have then a theory of a very different kind; this is that of the Count de Buffon. Here is a theory, not founded on any regular system, but upon an irregularity of nature, or an accident supposed to have happened to the sun. But, are we to consider as a theory of the earth, an accident by which a planetary body had been made to increase the number of these in the solar system? The circumvolution of a planetary body (allowing it to have happened in that manner) cannot form the system of a world, such as our earth exhibits; and, in forming a theory of the earth, it is required to see the aptitude of every part of this complicated machine to fulfil the purpose of its intention, and not to suppose the wise system of this world to have arisen from, the cooling of a lump of melted matter which had belonged to another body. When we consider the power and wisdom that must have been exerted in the contriving, creating, and maintaining this living world which sustains such a variety of plants and animals, the revolution of a mass of dead matter according to the laws of projectiles, although in perfect wisdom, is but like a unite among an infinite series of ascending numbers.

After the theory of that eloquent writer, founded on a mere accident, or rather the error of a comet which produced the beautiful system of this world, M. de Luc, in his Theory of the earth, has given us the history of a disaster which befell this well contrived world; —a disaster which caused the general deluge, and which, without a miracle, must have undone a system of living beings that are so well adapted to the present state of things. But, surely, general deluges form no part of the theory of the earth; for, the purpose of this earth is evidently to maintain vegetable and animal life, and not to destroy them.

Besides these imaginary great operations in the natural history of this earth, we have also certain suppositions of geologists and mineralists with regard to the effect of water, for explaining to us the consolidation of the loose materials of which the strata of the earth had been composed, and also for producing every other appearance, or any which shall happen to occur in the examination of the earth, and require to be explained. That this is no exaggerated representation, and that this is all we have as a theory, in the suppositions of those geologists, will appear from the following state of the case.

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They suppose water the agent employed in forming the solid bodies of the earth, and in producing those crystallised bodies which appear in the mineral kingdom. That this is a mere supposition will appear by considering; first, that they do not know how this agent water is to operate in producing those effects; nor have they any direct proof of the fact which is alleged, from a very fallaceous analogy; and, secondly, that they cannot tell us where this operation is to be performed. They cannot say that it is in the earth above the level of the sea: for, the same appearances are found as deep as we can examine below that level; besides, we see that water has the opposite effect upon the surface of the earth, through which it percolates dissolving soluble substances, and thus resolving solid bodies in preparing soil for plants. If, again, it be below the level of the sea, that strata of the earth are supposed to be consolidated by the infiltration of that water which falls from the heavens; this cannot be allowed, so far as whatever of the earth is bibulous, in that place, must have been always full of water, consequently cannot admit of that supposed infiltration.

But allowing those suppositions to be true, there is nothing in them like a theory of the earth,—a theory that should bring the operations of the world into the regularity of ends and means, and, by generalizing these regular events, show us the operation of perfect intelligence forming a design; they are only an attempt to show how certain things, which we see, have happened without any perceivable design, or without any farther design than this particular effect which we perceive. If we believe that there is almighty power, and supreme wisdom employed for sustaining that beautiful system of plants and animals which is so interesting to us, we must certainly conclude, that the earth, on which this system of living things depends, has been constructed on principles that are adequate to the end proposed, and procure it a perfection which it is our business to explore. Therefore, a proper system of the earth should lead us to see that wise contraction, by which this earth is made to answer the purpose of its intention and to preserve itself from every accident by which the design of this living world might be frustrated as this world is an active scene, or a material machine moving in all its parts, we must see how this machine is so contrived, as either to have those parts to move without wearing and decay, or to have those parts, which are wasting and decaying, again repaired.

A rock or stone is not a subject that, of itself, may interest a philosopher to study; but, when he comes to see the necessity of those hard bodies, in the constitution of this earth, or for the permanency of the land on which we dwell, and when he finds that there are means wisely provided for the renovation of this necessary decaying part, as well as that of every other, he then, with pleasure, contemplates this

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manifestation of design, and thus connects the mineral system of this earth with that by which the heavenly bodies are made to move perpetually in their orbits. It is not, therefore, simply by seeing the concretion of mineral bodies that a philosopher is to be gratified in his his intellectual pursuit, but by the contemplation of that system in which the necessary resolution of this earth, while at present it serves the purpose of vegetation, or the fertility of our soil, is the very means employed in furnishing the materials of future land.

It is such a view as this that I have endeavoured to represent in the theory which I have given. I have there stated the present situation of things, by which we are led to perceive a former state; and, from that necessary progress of actual things, I have concluded a certain system according to which things will be changed, without any accident or error. It is by tracing this regular system in nature that a philosopher is to perceive the wisdom with which this world has been contrived; but, he must see that wisdom founded upon the aptitude of all the parts to fulfil the intention of the design; and that intention is to be deduced from the end which is known to be attained. Thus we are first to reason from effect to cause, in seeing the order of that which has already happened; and then, from those known causes, to reason forwards, so as to conceive that which is to come to pass in time. Such would be the philosophy of this earth, formed by the highest generalisation of phenomena, a generalisation which had required the particular investigation of inductive reasoning.

That no such theory as this, founded upon water as an agent operating in the changes of this earth, has yet appeared, will, I believe be easily allowed. With regard again to fire as an agent in the mineral operations of this earth, geologists have formed no consistent theory. They see volcanoes in all the quarters of the globe, and from those burning mountains, they conjecture other mountains have been formed. But a burning mountain is only a matter of fact; and, they have not on this formed any general principle, for establishing what may be called a theory of the earth. Those who have considered subterraneous fires as producing certain effects, neither know how these have been procured, nor do they see the proper purpose for which they are employed in the system of this world. In this case, the agent fire is only seen as a destructive element, in like manner as deluges of water have been attributed by others to changes which have happened in the natural state of things. These operations are seen only as the accidents of nature, and not as part of that design by which the earth, which is necessarily wasted in the operations of the world, is to be repaired.

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So far from employing heat or subterraneous fire as an agent in the mineral operations of the earth, the volcanic philosophers do not even attempt to explain upon that principle the frequent nodules of calcareous, zeolite, and other spatose and agaty substances, in those basaltic bodies which they consider as lavas. Instead then of learning to see the operation of heat as a general principle of mineral consolidation and crystallization, the volcanic philosophers endeavour to explain those particular appearances, which they think inconsistent with fusion, by aqueous infiltration, no otherwise than other mineralists who do not admit the igneous origin of those basaltic bodies. Thus, that great agent, subterraneous heat, has never been employed by geologists, as a general principle in the theory of the earth; it has been only considered as an occasional circumstance, or as the accident of having certain mineral bodies, which are inflammable, kindled in the earth, without so much as seeing how that may be done.

This agent heat, then, is a new principle to be employed in forming a theory of the earth; a principle that must have been in the constitution of this globe, when contrived to subsist as a world, and to maintain a system of living bodies perpetuating their species. It is therefore necessary to connect this great mineral principle, subterraneous fire or heat, with the other operations of the world, in forming a general theory. For, whether we are to consider those great and constant explosions of mineral fire as a principal agent in the design, or only as a casual event depending upon circumstances which give occasion to an operation of such magnitude, here is an object that must surely have its place in every general theory of the earth.

In examining things which actually exist, and which have proceeded in a certain order, it is natural to look for that which had been first; man desires to know what had been the beginning of those things which now appear. But when, in forming a theory of the earth, a geologist shall indulge his fancy in framing, without evidence, that which had preceded the present order of things, he then either misleads himself, or writes a fable for the amusement of his reader. A theory of the earth, which has for object truth, can have no retrospect to that which had preceded the present order of this world; for, this order alone is what we have to reason upon; and to reason without data is nothing but delusion. A theory, therefore, which is limited to the actual constitution of this earth, cannot be allowed to proceed one step beyond the present order of things.

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But, having surveyed the order of this living world, and having investigated the progress of this active scene of life, death and circulation, we find ample data on which to found a train of the most conclusive reasoning with regard to a general design. It is thus that there is to be perceived another system of active things for the contemplation of our mind;—things which, though not immediately within our view, are not the less certain in being out of our sight; and things which must necessarily be comprehended in the theory of the earth, if we are to give stability to it as a world sustaining plants and animals. This is a mineral system, by which the decayed constitution of an earth, or fruitful surface of habitable land, may be continually renewed in proportion as it is wasted in the operations of this world.

It is in this mineral system that I have occasion to compare the explanations, which I give of certain natural appearances, with the theories or explanations which have been given by others, and which are generally received as the proper theory of those mineral operations. I am, therefore, to examine those different opinions, respecting the means employed by nature for producing particular appearances in the construction of our land, appearances which must be explained in some consistent mineral theory.

These appearances may all be comprehended under two heads, which are now to be mentioned, in order to see the importance of their explanation, or purpose which such an explanation is to serve in a theory of the earth. The first kind of these appearances is that of known bodies which we find composing part of the masses of our land, bodies whose natural history we know, as having existed in another state previous to the composition of this earth where they now are found; these are the relicts or parts of animal and vegetable bodies, and various stony substances broken and worn by attrition, all which had belonged to a former earth. By means of these known objects, we are to learn a great deal of the natural history of this earth; and, it is in tracing that history, from where we first perceive it, to the present state of things, that forms the subject of a geological and mineralogical theory of this earth. But, we are more especially enabled to trace those operations of the earth, by means of the second kind of appearances, which are now to be mentioned.

These again are the evident changes which those known bodies have undergone, and which have been induced upon such collected masses of which those bodies constitute a part. These changes are of three sorts; *first*, the solid state, and various degrees of it, in which we now find those masses which had been originally formed by the collection of loose and incoherent materials; *secondly*, the subsequent changes which have evidently happened to those consolidated masses which have been broken and displaced, and which have had other mineral substances introduced into those broken and disordered parts; and, *lastly*, that great change of situation which has happened to this compound mass formed originally at the bottom of the sea, a mass which, after being consolidated in the mineral region, is now situated in the atmosphere above the surface of the sea.

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In this manner we are led to the system of the world, or theory of the earth in general; for, that great change of situation, which our land has undergone, cannot be considered as the work of accident, or any other than an essential part in the system of this world. It is therefore a proper view of the necessary connection and mutual dependence of all those different systems of changing things that forms the theory of this earth as a world, or as that active part of nature which the philosophy of this earth has to explore. The animal system is the first or last of these; next comes the vegetable system, on which the life of animals depends; then comes the system of this earth, composed of atmosphere, sea, and land, and comprehending the various chemical, mechanical, and meteorologically operations which take place upon that surface where vegetation must proceed; and, lastly, we have the mineral system to contemplate, a system in which the wasting surface of the earth is employed in laying the foundation of future land within the sea, and a system in which the mineral operations are employed in concocting that future land.

Now, such must surely be the theory of this earth, if the land is continually wasting in the operations of this world; for, to acknowledge the perfection of those systems of plants and animals perpetuating their species, and to suppose the system of this earth on which they must depend, to be imperfect, and in time to perish, would be to reason inconsistently or absurdly. This is the view of nature that I would wish philosophers to take; but, there are certain prejudices of education or prepossession of opinion among them to be overcome, before they can be brought to see those fundamental propositions,—the wasting of the land, and the necessity of its renovation by the co-operation of the mineral system. Let us then consider how men of science, in examining the mineral state of things, and reasoning from those appearances by which we are to learn the physiology of this earth, have misled themselves with regard to physical causes, and formed certain mineralogical and geological theories, by which their judgment is so perverted, in examining nature, as to exclude them from the proper means of correcting their first erroneous notions, or render them blind to the clearest evidence of any other theory that is proposed.

When men of science reason upon subjects where the ideas are distinct and definite, with terms appropriated to the ideas, they come to conclusions in which there is no difference of opinion. It is otherwise in physical subjects, where things are to be assimilated, in being properly compared; there, things are not always compared in similar and equal circumstances or conditions; and there, philosophers often draw conclusions beyond the analogy of the things compared, and thus judge without data. When, for example, they would form the physical induction, with regard to the effect of fire or water upon certain substances

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in the mineral regions, from the analogy of such events as may be observed upon the surface of the earth, they are apt to judge of things acting under different circumstances or conditions, consequently not producing similar effects; in which case, they are judging without reason, that is, instead of inductive reasoning from actual data or physical truth, they are forming data to themselves purely by supposition, consequently, so far as these, imagined data may be wrong, the physical conclusion, of these philosophers may be erroneous.

It is thus that philosophers have judged, with regard to the effects of fire and water upon mineral substances below the bottom of the sea, from what their chemistry had taught them to believe concerning bodies exposed to those agents in the atmosphere or on the surface of the earth. If in those two cases the circumstances were the same, or similar, consequently the conditions of the action not changed, then, the inductive reasoning, which they employ in that comparison, would be just; but, so far as it is evidently otherwise, to have employed that inductive conclusion for the explanation of mineral appearances, without having reason to believe that those changed circumstances of the case should not make any difference in the action or effect, is plainly to have transgressed the rules of scientific reasoning; consequently, instead of being a proper physical conclusion, it is only that imperfect reasoning of the vulgar which, by comparing things not properly analysed or distinguished, is so subject to be erroneous. This vague reasoning, therefore, cannot be admitted as a part of any geological or mineral theory. Now I here maintain, that philosophers have judged in no other manner than by this false analogy, when they conclude that water is the agent by which mineral concretions have been formed. But it will be proper to state more particularly the case of that misunderstanding among mineral philosophers.

In forming a geological theory, the general construction of this earth, and the materials of which it is composed, are such visible objects, and so evident to those who will take the pains to examine nature, that here is a subject in which there cannot be any doubt or difference of opinion. Neither can there be any dispute concerning the place and situation of mass when it was first formed or composed; for, this is clearly proved, from every concomitant circumstance, to have been at the bottom of the sea. The only question in this case, that can be made, is, How that mass comes now to be a solid body, and above the surface of the sea in which it had been formed?

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With regard to the last, the opinions of philosophers have been so dissonant, so vague, and so unreasonable, as to draw to no conclusion. Some suppose the land to be discovered by the gradual retreat of the ocean, without proposing to explain to us from whence had come the known materials of a former earth, which compose the highest summits of the mountains in the highest continents of the earth. Others suppose the whole of a former earth to have subsided below the bottom even of the present sea, and together with it all the water of the former sea, from above the summits of the present mountains, which had then been at the bottom of the former sea. The placing of the bottom of the sea, or any part of it, in the atmosphere so as to be dry land, is no doubt a great operation to be performed, and a difficult task to be explained; but this is only an argument the more for philosophers to agree in adopting the most reasonable means.

But though philosophers differ so widely in that point, this is not the case with regard to the concretion of mineral bodies; here mineralists seem to be almost all of one mind, at the same time without any reason, at least, without any other reason than that false analogy which they have inconsiderately formed from the operations of the surface of this earth. This great misunderstanding of mineralists has such an extensive and baneful effect in the judging of geological theories, that it will be proper here to explain how that has happened, and to shew the necessity of correcting that erroneous principle before any just opinion can be formed upon the subject.

Fire and water are two great agents in the system of this earth; it is therefore most natural to look for the operation of those agents in the changes which are made on bodies in the mineral regions; and as the consolidated state of those bodies, which had been collected at the bottom of the sea, may have been supposed to be induced either by fusion, or by the concretion from a solution, we are to consider how far natural appearance lead to the conclusion of the one or other of those two different operations. Here, no doubt, we are to reason analogically from the known power and effects of those great agents; but, we must take care not to reason from a false analogy, by misunderstanding the circumstances of the case, or not attending to the necessary conditions in which those agents act.—We must not conclude that fire cannot burn in the mineral regions because our fires require the ventilation of the atmosphere; for, besides the actual exigence of mineral fire being a notorious matter of fact, we know that much more powerful means *may* be employed by nature, for that mineral purpose of exciting heat, than those which we practise.—We must not conclude that mineral marble is formed in the same manner as we see a similar stony substance produced upon the surface of the earth, unless we should have reason to suppose the analogy to be complete. But, this is the very error into which mineral philosophers have fallen; and this is the subject which I am now to endeavour to illustrate.

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The manner in which those philosophers have deceived themselves when reasoning upon the subject of mineral concretion, is this: They see, that by means of water a stony substance is produced; and, this stony body so much resembles mineral marble as to be hardly distinguishable in certain cases. These mineral philosophers then, reasoning in the manner of the vulgar, or without analysing the subject to its principle, naturally attribute the formation of the mineral marble to a cause of the same sort; and, the mineral marble being found so intimately connected with all other mineral bodies, we must necessarily conclude, in reasoning according to the soundest principles, that all those different substances had been concreted in the same manner. Thus, having once departed one step from the path of just investigation, our physical science is necessarily bewildered in the labyrinth of error. Let us then, in re-examining our data, point out where lies that first devious step which had been impregnated with fixed air, or carbonic acid gas, (as it is called), dissolves a certain portion of mild calcareous earth or marble; consequently such acidulated water, that is, water impregnated with this gas, will, by filtrating through calcareous substances, become saturated with that solution of marble; and, this solution is what is called a *petrifying water*. When this solution is exposed to the action of the atmosphere, the acid gas, by means of which the stony substance is dissolved, evaporates from the solution, in having a stronger attraction for the atmospheric air; it is then that the marble, or calcareous substance, concretes and crystallises, separating from the water in a sparry state, and forming a very solid stone by the successive accretion from the solution, as it comes to be exposed to the influence of the atmosphere in flowing over the accumulating body. Here is the source of their delusion; for, they do not distinguish properly the case of this solution of a stony substance concreting by means of the separation of its solvent, and the case of such a solution being in a place where that necessary condition cannot be supposed to exist; such as, *e.g.*, the interstices among the particles of sand, clay, *etc*. deposited at the bottom of the sea, and accumulated in immense stratified masses.

No example can better illustrate how pernicious it is to science to have admitted a false principle, on which a chain of reasoning is to proceed in forming a theory. Mineral philosophers have founded their theory upon that deceitful analogy, which they had concluded between the stalactical concretions of petrifying waters and the marble formed in the mineral regions; thus, blinded by prejudice, they shut the door against the clearest evidence; and it is most difficult to make them see the error of their principle. But this is not to be wondered at, when we consider how few among philosophising men remount to the first principles of their theory; and, unless they shall thus remount to that first step, in which the concreting operation of a dissolved stony substance is supposed to take place without the necessary conditions for the petrifying operation, it is impossible to be convinced that their theory, thus formed with regard to mineral concretion, is merely supposition, and has no foundation in matter of fact from whence it should proceed.

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But this is not all; for, even supposing their theory to be well founded and just, it is plainly contradicted by natural appearances. According to that theory of aqueous consolidation, all the stratified bodies, of which this earth in general consists, should be found in the natural order of their regular formation; but, instead of this, they are found every where disturbed in that order more or less; in many places this order and regularity is so disturbed as hardly to be acknowledged; in most places we find those stratified bodies broken, dislocated, and contorted, and this aqueous theory of mineralists has neither the means for attaining that end, were it required in their theory, nor have they any such purpose in their theory, were that end attainable by the means which they employ. Thus blinded by the prejudice of a false analogy, they do not even endeavour to gratify the human understanding (which naturally goes in quest of wisdom and design) by forming a hypothetical or specious theory of the mineral system; and they only amuse themselves with the supposition of an unknown operation of water for the explanation of their cabinet specimens, a supposition altogether ineffectual for the purpose of forming a habitable earth, and a supposition which is certainly contradicted by every natural appearance.

Thus, in examining geological and mineralogical theories, I am laid under the disagreeable necessity of pointing out the errors of physical principles which are assumed, the prejudices of theoretical opinions which have been received, and the misconceived notions which philosophers entertain with regard to the system of nature, in which may be perceived no ineffectual operation, nor any destructive intention, but the wise and benevolent purpose of preserving the present order of this world. But, though thus misled with regard to the cause of things, naturalists are every where making interesting observations in the mineral kingdom, I shall therefore avail myself of that instructive information, for the confirmation of my theory.

It may now be proper to consider what must be required, in order to have a geological and mineral theory established upon scientific principles, or on such grounds as must give conviction to those who will examine the subject; for, unless we may clearly see that there are means for attaining that desirable end, few philosophers will be persuaded to pursue this branch of knowledge.

A theory is nothing but the generalization of particular facts; and, in a theory of the earth, those facts must be taken from the observations of natural history. Nature is considered as absolutely true; no error or contradiction can be found in nature. For, if such contradiction were truly found, if the stone, for example, which fell to day were to rise again to-morrow, there would be an end of natural philosophy, our principles would fail, and we would no longer investigate the rules of nature from our observations.

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Every natural appearance, therefore, which is explained, *i.e.* which is made to come into the order of things that happen, must so far confirm the theory to which it then belongs. But is it necessary, that every particular appearance, among minerals, should be thus explained in a general theory of the earth? And, is any appearance, which is not explained by it, to be considered as sufficient to discredit or confute a theory which corresponded with every other appearance? Here is a question which it would require some accuracy to resolve.

If we knew all the powers of nature, and all the different conditions in which those powers may have their action varied, that is to say, if we were acquainted with every physical cause, then every natural effect, or all appearances upon the surface of this earth, might be explained in a theory that were just. But, seeing that this is far from being the case, and that there may be many causes of which we are as yet ignorant, as well as certain conditions in which the known action of powers may be varied, it must be evident, that a theory of the earth is not to be confuted by this argument alone, That there are, among natural bodies, certain appearances which are not explained by the theory. We must admit, that, not having all the data which natural philosophy requires, we cannot pretend to explain every thing which appears; and that our theories, which necessarily are imperfect, are not to be considered as erroneous when not explaining every thing which is in nature, but only when they are found contrary to or inconsistent with the laws of nature, which are known, and with which the case in question may be properly compared.

But we may have different theories to compare with nature; and, in that case, the question is not, How far any of those theories should explain all natural appearances? but, How far any one particular theory might explain a phenomenon better than another? In this case of comparison, it will be evident, that if one theory explains natural appearances, then the opposite to that theory cannot be supposed to explain the same appearances. If for example, granite, porphyry, or basaltes, should be found naturally formed by fusion, the formation of those stones could not be supposed in any case as formed by water, although it could not be demonstrated that water is incapable of forming those mineral productions.

In like manner, if those three bodies were proved to have been actually formed by water alone, then, in other cases where we should have no proof, they could not be supposed as having been formed by fire or fusion. It must be evident, that an equal degree of proof of those two different propositions would leave our judgment in suspence, unless that proof were perfect, in which case, we would have two different causes producing similar effects. But, if we shall have a sufficient proof upon the one side, and only a presumptive proof or probability upon the

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other, we must reject that probability or presumption, when opposed by a proof, although that proof were only an induction by reasoning from similar effects as following similar causes. *A fortiori*, if there be on one side a fair induction, without the least suspicion of error, and on the other nothing but a mere presumption founded upon a distant analogy, which could not even properly apply, then, the inductive proof would be as satisfactory as if there had not been any supposition on the opposite side.

So far as a theory is formed in the generalization of natural appearances, that theory must be just, although it may not be perfect, as having comprehended every appearance; that is to say, a theory is not perfect until it be founded upon every natural appearance; in which case, those appearances will be explained by the theory. The theory of gravitation, though no ways doubtful, was not so perfect before the shape of this globe had been determined by actual measurement, and before the direction of the plummet had been tried upon Shihallion, as after those observations had been made. But a theory which should be merely hypothetical, or founded upon a few appearances, can only be received as a theory, after it has been found to correspond properly with nature; it would then be held a proper explanation of those natural appearances with which it corresponded; and, the more of those phenomena that were thus explained by the theory, the more would that, which had been first conjectural, be converted into a theory legitimately founded upon natural appearances.

Matter of fact is that upon which science proceeds, by generalization, to form theory, for the purpose of philosophy, or the knowledge of all natural causes; and it is by the companion of these matters of fact with any theory, that such a theory will be tried. But, in judging of matter of fact, let us be cautious of deceiving ourselves, by substituting speculative reasoning in place of actual events.

Nature, as the subject of our observation, consists of two sorts of objects; for, things are either active, when we perceive change to take place in consequence of such action, or they are quiescent, when we perceive no change to take place. Now, it is evident, that in judging of the active powers of nature from the quiescent objects of our information, we are liable to error, in misinterpreting the objects which we see; we thus form to ourselves false or erroneous opinion concerning the general laws of action, and the powers of nature. In comparing, therefore, generalised facts, or theory, with particular observations, there is required the greatest care, neither, on the one hand, to strain the appearances, so as to bring in to the theory a fact belonging to another class of things; nor, on the other, to condemn a proper theory, merely because that theory has not been extended to the explanation of every natural appearance.

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But, besides the misinterpretation of matters of fact, we are also to guard against the misrepresentation of natural appearances. Whether warped by the prejudice of partial and erroneous theory, or deceived by the inaccuracy of superficial observation, naturalists are apt to see things in an improper light, and thus to reason from principles which cannot be admitted, and, which often lead to false conclusions. A naturalist, for example, comes to examine a cavity in the mines, he there finds water dropping down all around him, and he sees the cavity all hung with siliceous crystals; he then concludes, without hesitation, that here is to be perceived cause and effect, or that he actually sees the formation of those crystallizations from the operation of water. It is thus that I have been told by men of great mineral knowledge, men who must have had the best education upon that subject of mineralogy, and who have the superintendance of great mineral concerns in Germany, that they had actually seen nature at work in that operation of forming rock-crystal;—they saw what I have now described; they could see no more; but, they saw what had convinced them of that which, there is every reason to believe, never happened. With regard to my theory, I wish for the most rigorous examination; and do not ask for any indulgence whatever, whether with regard to the principles on which the theory is built, or for the application of the theory to the explanation of natural appearances. But, let not geologists judge my theory by their imperfect notions of nature, or by those narrow views which they take of the present state of things;—let not mineralogists condemn my theory, for no other reason but because it does not correspond with their false principles, and those gratuitous suppositions by which they had been pleased to explain to themselves every thing before. First let them look into their own theory, and correct that erroneous principle, with regard to the action of water, or the assumption of unknown causes, upon which they have reasoned in forming their vague notions of the mineral region, before they can be properly qualified to examine, impartially, a theory which employs another principle. Every thing which has come under my observation shall be, as far as I can, faithfully related; nor shall I withhold those which neither the present theory, nor any other that I am acquainted with, can, I think, explain.

Appearances cannot well be described except in relation to some theory or general arrangement of the subject; because the particular detail, of every part in a complicated appearance, would be endless and insignificant. When, however, any question in a theory depends upon the nature of an appearance, we cannot be too particular in describing that by which the question is to be decided. But though it be sometimes proper to be minute in a particular, it is always, and above all things, necessary to be distinct; and not to confound

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together things which are of different natures. For, though it be by finding similarity, in things which at first sight may seem different, that science is promoted and philosophy attained, yet, we must have a distinct view of those things which are to be assimilated; and surely the lowest state of knowledge in any subject, is the not distinguishing things which, though not to common observation different, are not truly the same.

To confound, for example one stone with another, because they were both hard, friable, and heavy, would be to describe, with the superficial views of vulgar observation; whereas science specifies the weight and hardness, and thus accurately distinguishes the stone.

Before naturalists had learned to distinguish what they saw, and to describe, in known terms, those natural appearances, a theorist must have generalised only from his proper observation. This has been my case. When I first conceived my theory, few naturalists could write intelligibly upon the subject; but that is long ago, and things are much altered since; now there are most enlightened men making observations, and communicating natural knowledge. I have the satisfaction, almost every day, to compare the theory, which I had formed from my proper observations, with the actual state of things in almost every quarter of the globe.

Whether, therefore, we mean to try a theory by its application to such phenomena as are well understood, or to learn something from the application of particular phenomena to a well established theory, we shall always find it interesting to have appearances described; particularly such as may be referred to some general rule, as circumscribing it to certain conditions, or as finding rule in rule, that is to say, discovering those particular conditions in which the general laws of action may be affected.

Instead, for example, of the rule which we find in the application of heat for the fusion and evaporation of mineral substances upon the surface of this earth, we may find it necessary to consider the effect which changed circumstances produce in the mineral regions, and occasion a change of that rule of action which we have learned from experience, when melting and evaporating those substances in the atmosphere or on the surface of the earth.

It is in this manner that a theory, which was formed by the generalization of particular facts, comes to be a source of information, by explaining to us certain appearances which otherwise we could not understand. Thus, it was not the appearance of the tides that taught the theory of gravitation; it was the theory of gravitation that made us understand the appearance of the tides. In like manner, the law of gravitation, which was demonstrated from the motion of the moon in her orbit round this earth, when applied to the paths of comets, explained that appearance. Our theory, of a central fire, has been formed upon the consolidation of the strata of this earth; but this theory is to

be applied for the explanation of various different appearances. In this manner, two different purposes will be served; the trying of the theory by its application to phenomena; and the explanation of phenomena by the principles laid open in the theory.

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I may repeat it; a theory of the earth must ultimately depend upon matter of fact or particular observation; but those observations must be distinct, and those distinguished things must be generalised. We have just now given for an example, a distinction among stones, in knowing them by their sensible qualities. But, besides distinguishing those objects, we are also to inquire into the origin and cause of those things which are distinguished. Here, again, we take into our aid the chemical as well as the mechanical properties of these several things; and hence learn to know on what their natural form and constitution may depend. Having thus attained the natural philosophy of stones, we next inquire into the place and application of those things in nature; and in this manner we acquire some knowledge with regard to the natural constitution of this earth. We find this earth composed of known things; it is therefore the operations, required in these compositions, which form the natural philosophy of this earth, considered as a body of solid land. But, the solid land is only one part of the globe; therefore, the philosophy of the globe proceeds still farther by knowing the constitution of this planetary body, as consisting of different parts united for a purpose, which is that of a world.

The general theory of this earth as a world, will thus appear to be a complex thing, which however founded upon simple principles, contains many subjects of discussion, and requires attention to a variety of particulars. For, not only the great features of this earth are to be explained by the theory, but also the most minute appearance, such as are to be found, even with microscopic observation, in every particular part.

Thus the nature, constitution, and cause of every particular appearance in the construction of this earth, are to be investigated in a geological theory, as well as that general constitution of the world in which all the particular parts are to be employed for a purpose.

If the subject here examined shall be found properly explained, there will remain little doubt with regard to the justness of the theory, which will then be applicable to other appearances that may occur; although every appearance is not to be explained, in a manner equally satisfactory, by any theory which is not perfect.

The first subject to be examined is the modern theory of primitive mountains. I have written several chapters upon that subject, having successively acquired more light in this interesting part of the theory, by observations of my own in several places of this country, as well as from the natural history of other countries. I shall give these nearly in the order in which they occurred, or had been written.

CHAP. IV.

The Supposition of Primitive Mountains refuted.

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In the theory now given, the earth has been represented as a composition of different materials, which had existed in another form, and as the effect of natural operations; therefore, however various may be found the structure of our earth, and however dissimilar some parts of its composition may be in comparison with others, no part should be considered as original, in relation to the globe, or as primitive, in relation to second causes, *i.e.* physical operations by which those parts should have been formed. But it is pretended by naturalists, that there are certain primitive mountains in the earth, bodies which have had another origin than that of the general strata of the globe and subsequent masses; an origin, therefore, which cannot be considered as having been produced from natural operations, or as effected in the course of known causes. Now, if it can be made to appear, that there is no solid ground for this distinction; and if it can be shown, that there is truly no mineral body in this earth which may not have been produced by operations natural to the globe, we should thus procure a certain confirmation of the doctrine. This also will be the more interesting, in being deduced from a part of natural appearances, which seemed to be inconsistent with the theory.

Certain masses or mountains of granite, are the only bodies of this earth which have apparently a certain pretension to this species of originality. These, therefore, must be now the subject of our examination.

Granite, considered by itself, does not appear to have any claim to originality in its nature. It is composed of bodies which are capable of being analyzed; and these are then found to be compositions of different substances, which are also sometimes variously proportioned. The feldspar and the mica, for example, as well as the schorl, are found variously coloured in different granites, and coloured in various proportions. Besides the variety in the composition, or chemical mixture of the different bodies which compose granite, this rock admits of a great diversity, from the variety of its mechanical mixture, or from the different species of bodies which are its constituent parts. M. de Saussure, who has examined this subject perhaps more than any other person, and who has had the very best opportunities for this purpose, says, that this composition may be found in all the different combinations which may be produced by every possible composition of 7 or 8 different kinds of stone, (page 108, Voyage dans les Alpes, *etc*.). Neither does this fill up the measure of its variety; for, another source of change is found in the grain of this rock stone; I have a specimen of this variety from the size almost of sand to that of some inches.

Were granite, therefore, to be supposed as in the original state of its creation, nature would be considered as having operated in an indefinite diversity of ways, without that order and wisdom which we find in all her works; for here would be change without a principle, and variety without a purpose. There is no reason, however, to suppose granite original, more than any other composite rock, although we may be ignorant of the particular process in which it is formed, and although, comparatively in relation to certain other rocks, granite, or certain masses of this composition, may be found of a more ancient date.

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If granite be truly stratified, and those strata connected with the other strata of the earth, it can have no claim to originality; and the idea of primitive mountains, of late so much employed by natural philosophers, must vanish, in a more extensive view of the operations of the globe; but it is certain that granite, or a species of the same kind of stone, is thus found stratified. It is the *granit feuilletee* of M. de Saussure, and, if I mistake not, what is called *gneis* by the Germans. We have it also in our north alpine country of Scotland; of this I have specimens, but have not seen it in its place.

Granite being thus found stratified, the masses of this stone cannot be allowed to have any right of priority over the schistus, its companion in the alpine countries, although M. de Saussure, whose authority I would revere, has given it for the following reason; that it is found the most centrical in the chains of high mountains, or in alpine countries. Now, supposing this fact to be general, as he has found it in the Alps, no argument for the priority of those masses can be founded either upon the height or the situation of those granite mountains; for the height of the mountain depends upon the solidity and strength of the stone. Now though it is not to be here maintained that granite is the most durable of those alpine rocks, yet as a mountain, either granite in general, or in particular, certain species of it, may be esteemed such, consequently, this massy stone, remaining highest in the mountainous region, will naturally be considered as the centre, and according to this rule, as having the pre-eminence in point of seniority.

The rock which stands in competition with granite for the title of primitive in the order of mountains, is that micaceous stratified stone which is formed chiefly of quartz, but which admits of great variety like the granite. The difference between those two bodies does not consist in the materials of which they are composed, for, in their varieties, they may be in this respect the same, but in a certain regularity of composition, in this alpine stone, which evidently arises from stratification or subsidence in water.

If we shall thus consider all the varieties of this alpine stone as being of one kind, and call it granite, then we shall distinguish in this body two different species, from whence perhaps some interesting conclusion may be formed with regard to the operations of the globe. These two species are, *first*, granite regular in its composition, or stratified in its construction; and, *secondly*, granite in mass, or irregular in its construction. Let us now endeavour to make use of these generalizations and distinctions.

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In examining the great diversity of our whinstone, trap, or basaltes, it is found at last to granulate into granite; at the same time those two different species of rock-stone may be distinguished. A perfect granite has not in its composition necessarily any argillaceous earth, farther than may be in the natural constitution of its distinct parts; whereas, a perfect basalt may have abundance of this substance, without any quartz or any siliceous body. A perfect granite, is, therefore, an extremely hard stone, having quartz and feldspar for its basis; but a perfect whin or basaltes may be extremely soft, so as to cut easily with a knife. In like manner granite is a composition which graduates into porphyry; but porphyry is only whinstone of a harder species. Therefore, though perfectly distinct, those three things graduate into each other, and may be considered as the same.

Granite and whinstone, or basaltes, though distinct compositions, thus graduating into each other; and whinstone, as well as porphyry, being without doubt a species of lava, we may consider the granite which is found in mass without stratification, in like manner as we do the masses of whinstone, basaltes, or Swedish trap, as having flowed in the bowels of the earth, and thus been produced by the chance of place, without any proper form of its own, or in an irregular shape and construction. In this manner would be explained the irregular shape or structure of those granite masses; and thus great light would be thrown upon the waved structure of the stratified alpine stone, which, though it has not been made to flow, has been brought to a great degree of softness, so as to have the original straight lines of its stratification changed to those undulated or waving lines which are in some cases extremely much incurvated.

It remains only to confirm this reasoning, upon our principles, by bringing actual observation to its support; and this we shall do from two of the best authorities. The Chevalier de Dolomieu, in describing the volcanic productions of Etna, mentions a lava which had flowed from that mountain, and which may be considered as a granite. But M. de Saussure has put this matter out of doubt by describing most accurately what he had seen both in the Alps and at the city of Lyons. These are veins of granite which have flowed from the contiguous mass into the stratified stone, and leave no doubt with regard to this proposition, that the granite had flowed in form of subterranean lava, although M. de Saussure has drawn a very different conclusion from this appearance. I have also a specimen from this country of a vein of granite in a granite stone, the vein being of a smaller grain than that of the rock which it traverses.[20]

[Note 20: This is what I had wrote upon, the subject of granite, before I had acquired such ample testimony from my own observations upon that species of rock. I have given some notice, in the 3d vol. of the Transactions of the Edinburgh R.S. concerning the general result of those observations, which will be given particularly in the course of this work.]

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It will thus appear, that the doctrine which of late has prevailed, of primitive mountains, or something which should be considered as original in the construction of this earth, must be given up as a false view of nature, which has formed the granite upon the same principle with that of any other consolidated stratum; so far as the collection of different materials, and the subsequent fusion of the compound mass, are necessary operations in the preparation of all the solid masses of the earth. Whatever operations of the globe, therefore, may be concluded from the composition of granite masses, as well as of the alpine strata, these must be considered as giving us information with regard to the natural history of this earth; and they will be considered as important, in proportion as they disclose to us truths, which from other strata might not be so evident, or at all made known.

Let us now examine the arguments, which, may be employed in favour of that supposition of primitive mountains.

The observations, on which naturalists have founded that opinion of originality in some of the component parts of our earth, are these; *first*, They observe certain great masses of granite in which stratification is not to be perceived; this then they say is an original mass, and it is not to be derived from any natural operation of the globe; *secondly*, They observe considerable tracts of the earth composed of matter in the order of stratification as to its general composition, but not as to its particular position, the vertical position here prevailing, instead of the horizontal which is proper to strata formed in water; this, therefore, they also term primitive, and suppose it to be from another origin than that of the subsidence of materials moved in the waters of the globe; *lastly*, They observe both strata and masses of calcareous matter in which they cannot distinguish any marine body as is usual in other strata of the same substance; and these calcareous masses being generally connected with their primitive mountains, they have also included these collections of calcareous matter, in which marine bodies are not observed, among the primitive parts which they suppose to be the original construction of this globe.

It may be proper to see the description of a calcareous alpine mountain. M. de Saussure gives us the following observations concerning a mountain of this kind in the middle of the Alps, where the water divides in running different ways towards the sea. It is in describing the passage of the Bon-Homme, (Tom. 2. V. dans les Alpes).

“Sec. 759. Sur la droite ou au couchant de ces rochers, on voit une montagne calcaire etonnante dans ce genre par la hardiesse avec laquelle elle eleve contre le ciel ses cimes aigues et tranchantes, taillees a angles vifs dans le costume des hautes cimes de granit. Elle est pourtant bien surement calcaire, je l’ai observee de pres, et on rencontre sur cette route les blocs qui s’en detachent.

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“Cette pierre porte les caracteres des calcaires les plus anciennes; sa couleur est grise, son grain assez fin, on n’y appercoit aucun vestige de corps organises; ses couches sont peu epaisses, ondees et coupees frequemment par des fentes paralleles entr’elles et perpendiculaires a leurs plans. On trouve aussi parmi ces fragmens des breches calcaires grises.”

Here is a mountain which will rank with the most primitive of the earth; But why? only because it is extremely consolidated without any mark of organised body. Had there been in this mountain but one single shell, we should not then have scrupled to conclude that the origin of this lofty mountain had been the same with every marble or limestone in the earth. But though, from the structure of this stone, there is no mark of its having been formed immediately of the calcareous parts of animals, there is every mark of those calcareous strata having been formed like other marbles by deposit in the waters of the globe.

These two things are also homologated by the equal or perfect consolidation of their substance; for, as it is to be proved that all stratified marbles have been consolidated by the fusion of their substance, we must attribute the same consolidating cause to those alpine masses; the frequent veins that divide those calcareous strata which M. de Saussure has here described, also prove the nature of the consolidating cause, (see Chap. 1. page 111.).

This mountain, considered by itself, may perhaps afford no data by which a naturalist might read the circumstances of its origin. But, Is a theory of the earth to be formed upon such a negative observation? and, Is there any particular in this mountain, that may not be shown in others of which the origin is not in any degree doubtful?

It is not to be disputed, that there are parts of the solid body of our earth which may be considered as primary or prior, compared with others that are posterior, in relation to the time of their formation, and much less changed with regard to the state in which they had been originally formed:—But it is here denied, that there are any parts of the earth which do not appear to have had the same origin with all the rest, so far as this consists in the collection of materials deposited at the bottom of the waters[21]; for there is no solid mass of land that may not be traced to this origin, either from its composition, or from its local connection with other masses, the nature of which in this respect are known. We have already given examples of this from sufficient authority. The evidence, therefore, of those primary masses being original in relation to the natural operations of the globe, is reduced to this assertion, that there are no vestiges of organised bodies to be found in those primary masses. Let us now examine how far this testimony for the originality of those masses is to be admitted in fact and sound reasoning.

[Note 21: There are no collection of those alpine masses in which may not be found in some of them sand, mica, and gravel; but these materials prove the existence of an

earth, on which those fragments of greater masses had been formed, and more or less worn by attrition.]

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The matter in question at present is this, that there are certain tracts of countries in which no vestige of organised bodies are found; now, let us suppose the fact to be true or well grounded, Can we conclude from this that there had been originally no organised bodies in the composition of those masses?—Such a conclusion could only be formed in making a supposition, that every organised body deposited in a mass of matter, whether homogeneous or heterogeneous, should be preserved without change, while the collected mass, in which it had been deposited, changes as much as possible by the operation both of fire and water. But this supposition is erroneous, and cannot be admitted; and the study of marbles will demonstrate this truth, that the calcareous relics of organised bodies are changed, in the consolidating operations of the globe, in every degree, from the smallest alteration to the greatest, when they become indistinguishable any farther to our sight.

Therefore, from the supposition of no appearance of marine bodies in the pretended primitive masses, there is no sufficient evidence or reason to conclude, that those masses have not had a marine origin; because, the traces of organised bodies may be obliterated by the many subsequent operations of the mineral region; and which operations, the present state of those masses certify beyond dispute.

We are now to examine the fact, how far the ground on which that false reasoning had been founded is strictly true.

In the first place, then, it must be considered, that the alledged fact is nothing but a negative assertion, importing that no mark of organised bodies had been observed, in certain stones and strata which some naturalists have examined with that view. But, though many naturalists have looked for them without success, it does not follow that such marks may not be found; it indeed proves that such a task is difficult, and the success of it, to a particular, most precarious; accident, however, may bring about what the greatest industry has not been able to attain. Secondly, there is good reason to believe that this asserted negation is not absolutely true; for I have in my possession what I consider as proof of the contrary; I found it in Wales, and I think it is in what may be considered as primitive mountains;—it is the mark of shells in a stone of that kind.

Thus, I had formed my opinion with regard to this alleged fact, long before I had seen any description either of the Alps or Pyrennean mountains; and now I have no reason to change that opinion. It may indeed be alleged, that the strata of marble or limestone, containing marine bodies found in those mountains, are secondary strata, and not the primitive. To this I can give no reply, as the descriptions given of those strata do not enable me to decide this point.

At the village of Mat, under the Mont Blatten for example, there is a quarry of schistus or black slate, in which are often found the print and the bones of fishes. (Discours sur l’Histoire Naturelle de la Suisse, page 225.). If this may be considered as an alpine or primitive schistus, it would be decisive of the question: But it would require to have it

well ascertained that this schistus is truly one of those which are esteemed primitive, or that it is properly connected with them.

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But though I cannot find in those interesting descriptions which we now have got, any one which is demonstrative of this truth, that calcareous marine objects are found in the primitive strata, this is not the case with regard to another object equally important in deciding this question, Whether the primitive strata are found containing the marks of organised bodies?

M. de Dellancourt, in his *Observations Mineralogiques*, Journal de Physique Juillet 1786, in describing the mountains of Dauphine, gives us the following fact with regard to those alpine vertical strata.

“La pierre constituante de la montagne d’Oris est en general le *Kneifs* ou la roche feuilletee mica et quartz a couches plus ou moins ferrees quelquefois le schorl en roche penetre de steatite. Les couches varient infiniment quant a leur direction et a leur inclinaisons. Cette montagne est cultivee et riche dans certain cantons, surtout autour du village d’Oris, mais elle est tres-escarpee dans beaucoup d’autres. Entre le village d’Oris et celui du Tresnay est une espece de combe assez creuse formee par la chute des eaux des cimes superieures des rochers. Cette combe offre beaucoup de schiste dont les couches font ou tres-inclinees ou perpendiculaires. Entre ces couches il s’en est trouve de plus noires que les autres et capable de bruler, mais difficilement. Les habitans ont extrait beaucoup de cette matiere terreuse, et lui ont donne le nom de charbon de terre. Ils viennent meme a bout de la faire bruler, et de s’en servir l’hiver en la melant avec du bois. Ce schiste noir particulier m’a paru exister principalement dans les endroits ou les eaux se sont infiltrees entre les couches perpendiculaires, et y ont entraine diverse matieres, et sur-tout des debris de vegetaux que j’ai encore retrouves a demi-noirs, pulverulens et comme dans un etat charbonneux.”

This formation of coal, by the infiltration of water and carrying in of vegetable bodies, certainly cannot be admitted of; consequently, from this description, there would seem to be strata of coal alternated with the alpine schisti. But the formation of mineral coal requires vegetable matter to have been deposited along with those earthy substances, at the bottom of the sea. The production of vegetable bodies, again, requires the constitution of sea and land, and the system of a living world, sustaining plants at least, if not animals.

In this natural history of the alpine schisti, therefore, we have a most interesting fact, an example which is extremely rare. Seldom are calcareous organised bodies found among those alpine strata, but still more rarely, I believe, are the marks of vegetable bodies having contributed in the formation of those masses. But however rare this example, it is equally decisive of the question, Whether the alpine schisti have had a similar origin as the other strata of the globe, in which are found abundance of animal and vegetable bodies, or their relics? and we are authorised to say, that since those perfect alpine strata of Dauphine have had that origin, all the alpine schisti of the globe have been originally formed in a similar manner. But to put this matter out of doubt:

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In this summer 1788, coming from the Isle of Man, Mr Clerk and I traveled through the alpine schistus country of Cumberland and Westmoreland. We found a limestone quarry upon the banks of Windermere, near the Low-wood Inn. I examined this limestone closely, but despaired of finding any vestige of organised body. The strata of limestone seem to graduate into the slate or schistus strata, between which the calcareous are placed. Fortunately, however, I at last found a fragment in which I thought to perceive the works of organised bodies in a sparry state; I told Mr Clerk so, and our landlord Mr Wright, who had accompanied us. I have brought home this specimen, which I have now ground and polished; and now it is most evidently full of fragments of entrochi. Mr Wright then told me he had seen evident impressions of marine objects, as I understood from the description, in the slate of those mountains; and he was to send me specimens so soon as he could procure them.

Here is one specimen which at once overturns all the speculations formed upon that negative proposition. The schistus mountains of Cumberland were, in this respect, as perfect primitive mountains as any upon the earth, before this observation; now they have no claim upon that score, no more than any limestone formed of shells.

When I first announced my belief that such objects in natural history might be found, I little thought to have seen it realised, to such a degree as has now happened in the little circle of my knowledge. In the summer 1791,

Professor Playfair was to pass through Cumberland. I begged that he would inquire of Mr Wright, at the Low-wood Inn, for those objects which he was to endeavour to procure for me, and to examine the limestone quarry in which I had found the specimen with entrochi. He went through another part of those primary mountains, and has found examples of this kind in the schisti; concerning which he has written me the following account.

“In a visit which I made to the Lakes of Cumberland in September 1791, in company with the Hon. Francis Charteris, I met with a limestone full of marine objects, though from its position it is certainly to be reckoned among the primary strata. The place where we found this stone was in the district of Lancashire, that is west of Windermere Lake, on the road from Ambleside to the north end of Coniston Lake, and not far from the point when you come in sight of the latter. Just about this spot we happened to meet with one of those people who serve as guides to travelers in those parts, and who told us, among other things, that stones with shells in them were often found not far from where we were then walking. We immediately began to look about for specimens of that kind, and soon met with several; the most remarkable of which was in a rock that rose a little above the surface, about 300 or 400 yards to the right of the road. It was a part of a limestone stratum, nearly vertical,

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and was full of bivalves with the impressions as strong as in a common secondary limestone. The strata on both sides had the same inclination, and were decidedly primary, consisting of the ordinary micaceous schistus. This however I need not remark to you, who know so well from your own observations that the whole of the country I am now speaking of has every character of a primary one. I, only mention it, that it may not be supposed that the rock in question was some fragment of a secondary stratum that remained, after the rest was washed away, superincumbent on the primary.

“After I had seen this rock, I recollected that you had told me of something of the same kind that you saw in a quarry at Low-wood Inn; and it may be that both belonged to the same stratum or body of strata; for the direction of the strata, as nearly as I could observe, was from S.W. to N.E.; and this also is nearly the bearing of Low-wood from the place where we now were. I send you a specimen, which you can compare with those you brought from the lime quarry at Low-wood.”

I have examined this specimen, and find it to be the common schistus of that country, only containing many bivalve shells and fragments of entrochi and madrapore bodies, and mixed with pyrites.

I have already observed that one single example of a shell, or of its print, in a schistus, or in a stone stratified among those vertical or erected masses, suffices to prove the origin of those bodies to have been, what I had maintained them to be, water formed strata erected from the bottom of the sea, like every other consolidated stratum of the earth. But now, I think, I may affirm, that there is not, or rarely, any considerable extent of country of that primary kind, in which some mark of this origin will not be found, upon careful examination; and now I will give my reason for this assertion. I have been examining the south alpine country of Scotland, occasionally, for more than forty years back, and I never could find any mark of an organised body in the schistus of those mountains. It is true that I know of only one place where limestone is found among the strata; this is upon Tweed-side near the Crook. This quarry I had carefully examined long ago, but could find no mark of any organised body in it. I suppose they now are working some other of the vertical strata near those which I had examined; for, in the summer 1792, I received a letter from Sir James Hall, which I shall now transcribe. It is dated at Moffat, June 2. 1792.

“As I was riding yesterday between Noble-house and Crook, on the road to this place, I fell in with a quarry of alpine limestone; it consists of four or five strata, about three feet thick, one of them single, and the rest contiguous; they all stand between the strata of slate and schist that are at the place nearly vertical. In the neighbourhood, a slate quarry is worked of a pure blue slate; several of the strata of slate near the limestone are filled with fragments of limestone scattered about like the fragments of schist in the sandstone in the neighbourhood of the junction on our coast.[22]

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[Note 22: This has a reference to very curious observations which we made upon the east coast where these mountains terminate, and which I am to describe in the course of this work.]

“Among the masses of limestone lately broken off for use, and having the fractures fresh, I found the forms of cockles quite distinct; and in great abundance.—I send you three pieces of this kind,” *etc*.

It may perhaps be alleged that those mountains of Cumberland and Tweedale are not the primary mountains, but composed of the secondary schistus, which is every where known to contain those objects belonging to a former earth. Naturalists who have not the opportunity of convincing themselves by their proper examination, must judge with regard to that geological fact by the description of others. Now it is most fortunate for natural history, that it has been in this range of mountains that we have discovered those marks of a marine origin; for, I shall afterwards have occasion to give the clearest light into this subject, from observations made in other parts of those same mountains of schist, by which it will be proved that they are the primary strata; and thus no manner of doubt will then remain in the minds of naturalists, who might otherwise suspect that we were deceiving ourselves, by mistaking the secondary for the primitive schistus.

I have only farther to observe, that those schisti mountains of Wales, of Cumberland, and of the south alpine part of Scotland, where these marine objects have been found, consist, of that species of stone which in some places makes the most admirable slate for covering houses; and, in other parts, it breaks into blocks that so much resemble wood in appearance, that, without narrow inspection, it might pass for petrified wood.

We are therefore to conclude that the marks of organised bodies in those primary mountains are certainly found; at the same time the general observation of naturalists has some foundation, so far as the marks of organised bodies are both rarely to be met with in those masses, and not easily distinguished as such when they are found.

But this scarcity of marine objects is not confined to those primary mountains, as they are called; for among the most horizontal strata, or those of the latest production, there are many in which, it is commonly thought, no marine calcareous objects are to be found; and this is a subject that deserves to be more particularly considered, as the theory may thus receive some illustration.

Sandstone, coal, and their accompanying strata, are thought to be destitute of calcareous marine productions, although many vestiges of plants or vegetable productions are there perceived. But this general opinion is neither accurate nor true; for though it be true that in the coal and sandstone strata it is most common to find marks of vegetable production, and rarely those calcareous bodies which are so frequent in the

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limestone, yet it is not unusual for coal to be accompanied with limestone formed of shells and corals, and also with ironstone containing many of those marine objects as well as wood. Besides, sandstone frequently contains objects which have been organised bodies, but which do not belong to the vegetable kingdom, at least to no plant which grows upon the land, but would seem to have been some species of zoophite perhaps unknown.

I have also frequently seen the vestige of shells in sandstone, although in these strata the calcareous bodies are in general not perceived. The reason of this is evident. When there is a small proportion of the calcareous matter in the mass of sand which is pervious to steam and to the percolation of water, the calcareous bodies may be easily dissolved, and either carried away or dispersed in the mass; or even without being thus dispersed by means of solution, the calcareous matter may be absorbed by the siliceous substance of the stratum by means of fusion, or by heat and cementation. The fact is, that I have seen in sandstone the empty mould of marine shells with some siliceous crystallization, so far as I remember, which corresponded perfectly with that idea. The place I saw this was in a fine white sandstone accompanying the coal, upon the sea side at Brora in Sutherland.

Mineralogy is much indebted to Mr Pallas for the valuable observations which he has given of countries so distant from the habitations of learned men. The physiology of the globe has also been enriched with some interesting observations from the labours of this learned traveller. But besides giving us facts, Mr Pallas has also reasoned upon the subject, and thus entered deep into the science of Cosmogeny; here it is that I am afraid he has introduced some confusion into the natural history of the earth, in not properly distinguishing the mineral operations of the globe, and those again which belong entirely to the surface of the earth; perhaps also in confounding the natural effects of water upon the surface of the earth, with those convulsions of the sea which may be properly considered as the accidental operations of the globe. This subject being strictly connected with the opinions of that philosopher with regard to primitive mountains, I am obliged to examine in this place matters which otherwise might have come more properly to be considered in another.

M. Pallas in his *Observations sur la formation des montagnes*, (page 48) makes the following observations.

“J’ai deja dit que *la bande de montagnes primitives schisteuses* heterogenes, qui, par toute la terre, accompagne les chaines granitiques, et comprend les roches quartzeuses et talceuses mixtes, trapezoides, serpentines, le schiste corne, les roches spathiques et cornees, les grais purs, le porphyre et le jaspre, tous rocs feles en couches, ou presque perpendiculaires, ou du moins tres-rapidement inclinees, (les plus favorables

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a la filtration des eaux), semble aussi-bien que le granit, anterieure a la creation organisee. Une raison tres-forte pour appuyer cette supposition, c'est que la plupart de ces roches, quoique lamelleuse en facon d'ardoise, n'a jamais produit aux curieux la moindre trace de petrifactions ou empreintes de corps organises. S'il s'en est trouve, c'est apparemment dans des fentes de ces roches ou ces corps ont ete apportes par un deluge, et encastrees apres dans une matiere infiltree, de meme qu'on a trouve des restes d'Elephans dans le filon de la mine d'argent du Schlangenberg.[23] Les caracteres par lesquels plusieurs de ces roches semblent avoir souffert des effets d'un feu-tres-violent, les puissantes veines et amas des mineraux les plus riches qui se trouvent principalement dans la bande qui en est composee, leur position immediate sur le granit, et meme le passage, par lequel on voit souvent en grand, changer le granit en une des autres especes; tout cela indique une origine bien plus ancienne, et des causes bien differentes de celles qui ont produit les montagnes secondaires."

[Note 23: This is a very natural way of reasoning when a philosopher finds a fact, related by some naturalists, that does not correspond with his theory or systematic view of things. Here our author follows the general opinion in concluding that no organised body should be found in their primitive strata; when, therefore, such an object is said to have been observed, it is supposed that there may have been some mistake with regard to the case, and that all the circumstances may not have been considered. This caution with regard to the inaccurate representation of facts, in natural history, is certainly extremely necessary; the relicts of an elephant found in a mineral vein, is certainly a fact of that kind, which should not be given as an example in geology without the most accurate scientifical examination of the subject.]

Here M. Pallas gives his reason for supposing those mountains primitive or anterior to the operations of this globe as a living world; *first*, because they have not, in general, marks of animals or plants; and that it is doubtful if they ever properly contain those marks of organised bodies; *secondly*, because many of those rocks have the appearance of having suffered the effects of the most violent fire. Now, What are those effects? Is it in their having been brought into a fluid state of fusion. In that case, no doubt, they may have been much changed from the original state of their formation; but this is a very good reason why, in this changed state, the marks of organised bodies, which may have been in their original constitution, should be now effaced.

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The *third* reason for supposing those mountains primitive, is taken from the metallic veins, which are found so plentifully in these masses. Now, had these masses been the only bodies in this earth in which those mineral veins were found, there might be some species of reason for drawing the conclusion, which is here formed by our philosopher. But nothing is so common (at least in England) as mineral veins in the strata of the latest formation, and in those which are principally formed of marine productions; consequently so far from serving the purpose for which this argument was employed, the mineral veins in the primitive mountains tend to destroy their originality, by assimilating them in some respect with every other mass of strata or mountain upon the globe.

Lastly, M. Pallas here employs an argument taken from an appearance for which we are particularly indebted to him, and by which the arguments which have been already employed in denying the originality of granite is abundantly confirmed. It has been already alleged, that granite, porphyry, and whinstone, or trap, graduate into each other; but here M. Pallas informs us that he has found the granite not only changed into porphyry, but also into the other alpine compositions. How an argument for the originality of these mountains can be established upon those facts, I am not a little at a loss to conceive.

The general mineralogical view of the Russian dominions, which we have, in this treatise, may now be considered with regard to that distinction made by naturalists, of primitive, secondary, and tertiary mountains, in order to see how far the observations of this well informed naturalist shall be found to confirm the theory of the earth which has been already given, or not.

The Oural mountains form a very long chain, which makes the natural division betwixt Europe and Asia, to the north of the Caspian. If in this ridge, as a centre of elevation, and of mineral operations, we shall find the greatest manifestation of the violent exertion of subterraneous fire, or of consolidating and elevating operations; and if we shall perceive a regular appearance of diminution in the violence or magnitude of those operations, as the places gradually recede from this centre of active force; we may find some explanation of those appearances, without having recourse to conjectures which carry no scientific meaning, and which are more calculated to confound our acquired knowledge, than to form any valuable distinction of things. Let us consult M. Pallas how far this is the case, or not.

After having told us that all those various alpine schisti, jaspers, porphyries, serpentines, *etc*. in those mountains, are found mutually convertible with granite, or graduating into each other, our author thus continues, (p. 50).

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“On entrevoit de certaines loix a l’egard de l’arrangement respectif de cet ordre secondaire d’anciennes roches, par tous les systemes de montagnes qui appartiennent a l’Empire Russe. La chaine Ouralique, par exemple, a du cote de l’Orient sur tout sa longueur, une tres-grande abondance de schistes cornes, serpentins et talceux, riches en filons de cuivre, qui forment le principal accompagnement du granite, et en jaspres de diverses couleurs plus exterieurs et souvent comme entrelaces avec les premiers, mais formant des suites de montagnes entieres, et occupant de tres-grands espaces. De ce meme cote, il y parait beaucoup de quartz en grandes roches toutes pures, tant dans la principale chaine que dans le noyau des montagnes de jaspre, et jusques dans la plaine. Les marbres spateux et veines, percent en beaucoup d’endroits. La plupart de ces especes ne paraissent point du tout a la lisiere occidentale de la chaine, qui n’est presque que de roche melangee de schistes argileux, alumineux, phlogistique, *etc*. Les filons des mines d’or melees, les riches mines de cuivre en veines et chambrees, les mines de fer et d’aimant par amas et montagnes entieres, sont l’apanage de la bande schisteuse orientale; tandis que l’occidentale n’a pour elle que des mines de fer de depots, et se montre generalement tres-pauvre en metaux. Le granit de la chaine qui borde la Siberie, est recouvert du cote que nous connaissons de roches cornees de la nature des pierres a fusil, quelquefois tendant a la nature d’un grais fin et de schistes tres-metallieres de differente composition. Le jaspre n’y est qu’en filons, ou plans obliques, ce qui est tres-rare pour la chaine Ouralique, et s’observe dans la plus grande partie de la Siberie, a l’exception de cette partie de sa chaine qui passe pres de la mer d’Okhotsk, ou le jaspre forme derechef des suites de montagnes, ainsi que nous venons de le dire des monts Ourals; mais comme cette roche tient ici le cote meridionale de la chaine Siberienne, et que nous ne lui connaissons point ce cote sur le reste de sa longueur, il se pourrait que le jaspre y fut aussi abondant. Il faudrait, au reste, bien plus de fouilles et d observations pour etablir quelque chose de certain sur l’ordre respectif qu’observent ces roches.”

I would now ask, if in all this account of the gradation of rock from the Oural mountains to the sandy coast of the Baltic, there is to be observed any clear and distinctive mark of primitive, secondary, and tertiary, mountains, farther than as one stratum may be considered as either prior or posterior to another stratum, according to the order of superposition in which they are found. We have every where evident marks of the formation of strata by materials deposited originally in water; for the most part, there is sufficient proof that this water in which those materials had been deposited was the sea; we are likewise assured that the operations of this living world producing animals, must have, for a course of time, altogether inconceivably been exerted, in preparing materials for this mass; and, lastly, from the changed constitution of those masses, we may infer certain mineral operations that melt the substance and alter the position of those horizontal bodies. Such is the information which we may collect from this mineral description of the Russian Dominions.

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If we compare some of the Oural mountains with the general strata of the Russian plains, then, as to the contained minerals, we may find a certain diversity in those two places; at the same time, no greater perhaps than may be found betwixt two different bodies in those same plains, for example, chalk and flint. But when we consider those bodies of the earth, or solid strata of the globe, in relation to their proper structure and formation, we surely can find in this description nothing on which may be founded any solid opinion with regard to a different original, however important conclusions may perhaps be formed with regard to the operations of the globe, from the peculiar appearances found in alpine.

From this detail of what is found in the Oural mountains, and in the gradation of country from those mountains to the plains of Russia, we have several facts that are worthy of observation. First extensive mountains of jasper. I have a specimen of this stone; it is striped red and green like some of our marly strata. It has evidently been formed of such argillaceous and siliceous materials, not only indurated, so as to lose its character, as an argillaceous stone, but to have been brought into that degree of fusion which produces perfect solidity. Of the same kind are those hornstein rocks of the nature of flint, sometimes tending to the nature of a fine sandstone. Here is the same induration of sandstone by means of fusion, that in the argillaceous strata has produced jasper. But oblique veins of jasper are represented as traversing these last strata; now this is a fact which is not conceivable in any other way, than by the injection or transfusion of the fluid jasper among those masses of indurated strata.

All this belongs to the east side of the mountains. On the west, again, we find the same species of strata; only these are not changed to such a degree as to lose their original character or construction, and thus to be termed differently in mineralogy.

Our author then proceeds. (p. 53.)

“Nous pourrons parler plus decisivement sur les *montagnes secondaires et tertiaires* de l’Empire, et c’est de celles-la, de la nature, de l’arrangement et du contenu de leurs couches, des grandes inegalites et de la forme du continent d’Europe et d’Asie, que l’on peut tirer avec plus de confiance quelques lumieres sur les changemens arrives aux terres habitables. Ces deux ordres de montagnes presentent la chronique de notre globe la plus ancienne, la moins sujette aux falsifications, et en meme-tems plus lisible que le caractere des chaines primitives; ce font les archives de la nature, anterieures aux lettres et aux traditions les plus reculees, qu’il etoit reserve a notre siecle observateur de feuiller, de commenter, et de mettre au jour, mais que plusieurs siecles apres le notre n’epuiseront pas.

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“Dans toute l’etendue de vastes dominations Russes, aussi bien que dans l’Europe entiere, les observateurs attentifs ont remarque que generalement la band schisteuse des grandes chaines se trouve immediatement recouverte ou cottee par la *bande calcaire*. Celle-ci forme deux ordres de montagnes, tres-differentes par la hauteur, la situation de leurs couches, et la composition de la pierre calcaire qui les compose; difference qui est tres-evidente dans cette bande calcaire qui forme la lisiere occidentale de toute la chaine Ouralique, et dont le plan s’etend par tout le plat pays de la Russie. L’on observerait la meme chose a l’orient de la chaine, et dans toute l’etendue de la Siberie, si les couches calcaires horizontales n’y etaient recouvertes par les depots posterieures, de facon qu’il ne parait a la surface que les parties les plus faillantes de la bande, et si ce pays n’etoit trop nouvellement cultive et trop peu exploite par des fouilles et autres operations, que des hommes industrieux ont pratique dans les pays anciennement habites. Ce que je vais exposer sur les deux ordres de montagnes calcaires, se rapportera donc principalement a celles qui sont a l’occident de la chaine Ouralique.

“Ce cote de la dite chaine consiste sur cinquante a cent verstes de largeur, de roche calcaire solide, d’un grain uni, qui tantot ne contient aucune trace de productions marines, tantot n’en conserve que des empreintes aussi legeres qu’eparses. Cette roche s’eleve en montagnes d’une hauteur tres-considerable, irregulieres, rapides, et coupees de vallons escarpes. Ses couches, generalement epaisses, ne sont point de niveau, mais tres-inclinees a l’horizon, paralleles, pour la plupart, a la direction de la chaine, qui est aussi ordinairement celle de la bande schisteuse;—au lieu que du cote de l’orient les couches calcaires sont au sens de la chaine en direction plus ou moins approchante de l’angle droite. L’on trouve dans ces hautes montagnes calcaires de frequentes grottes et cavernes tres-remarquables, tant par leur grandeur que par les belles congelations et crystallizations stalactiques dont elles s’ornent. Quelques-unes de ces grottes ne peuvent etre attribuees qu’a quelque bouleversement des couches; d’autres semblent devoir leur origine a l’ecoulement des sources souterraines qui ont amolli, ronge et charrie une partie de la roche qui en etoit susceptible.

“En s’eloignant de la chaine, on voit les couches calcaires s’aplanir assez rapidement, prendre une position horizontale, et devenir abondantes en toute forte de coquillages, de madrepores, et d’autres depouilles marines. Telles on les voit par-tout dans les vallees les plus basses qui se trouvent aux pieds des montagnes (comme aux environs de la riviere d’Oufa); telles aussi, elles occupent tout l’etendue de la grande Russie, tant en collines qu’en plat pays; solides tantot et comme semees de productions marines; tantot toutes composees de coquilles et madrepores brisees, et de ce gravier calcaire qui se trouve toujours sur les parages ou la mer abonde en pareilles productions; tantot, enfin, dissoutes en craie et en marines, et souvent entremelees de couches de gravier et de cailloux roules.”

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How valuable for science to have naturalists who can distinguish properly what they see, and describe intelligibly that which they distinguish. In this description of the strata, from the chain of mountains here considered as primitive, to the plains of Russia, which are supposed to be of a tertiary formation, our naturalist presents us with another species of strata, which he has distinguished, on the one hand, in relation to the mountains at present in question, and on the other, with regard to the strata in the plains, concerning which there is at present no question at all. Now, let us see how these three things are so connected in their nature, as to form properly the contiguous links of the same chain.

The primary and tertiary masses are bodies perfectly disconnected; and, without a medium by which they might be approached, they would be considered as things differing in all respects, consequently as having their origins of as opposite a nature as are their appearances. But the nature and formation of those bodies are not left in this obscurity; for, the secondary masses, which are interposed, participate so precisely of what is truly opposite and characteristic in the primary and tertiary masses, that it requires nothing more than to see this distinction of things in its true light, to be persuaded, that in those three different things we may perceive a certain gradation, which here takes place among the works of nature, and forms three steps distinguishable by a naturalist, although in reality nothing but the variable measure of similar operations.

We are now to assimilate the primary and tertiary masses, which are so extremely different, by means of the secondary masses, which is the mean. The primary and tertiary differ in the following respects: The one of these contains the relicts of organised bodies which are not observed in the other. But in the species containing these distinguishable bodies, the natural structure and position of the mass is little affected, or not so much as to be called into doubt. This, however, is not the case with the other; the species in which organised bodies do not appear, is in general so indurated or consolidated in its structure, and changed in its position, that this common origin of those masses is by good naturalists, who have also carefully examined them, actually denied. Now, the secondary masses may be considered, not only as intermediate with respect to its actual place, as M. Pallas has represented it, but as uniting together the primary and tertiary, or as participating of the distinguishing characters of the other two. It is homologated with the primitive mountains, in the solidity of its substance and in the position of its strata; with the tertiary species, again, in its containing marks of organised bodies. How far this view of things is consistent with the theory of the earth now given, is submitted to the consideration of the unprejudiced.

Let us see what our learned author has said farther on this subject, (page 65).

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“Je dois parler d’un ordre de montagnes tres-certainement posterieur aux couches marines, puisque celles-ci, generalement lui servent de base. On n’a point jusqu’ici observe une suite de ces *montagnes tertiaires*, effet des catastrophes les plus modernes de notre globe, si marquee et si puissante, que celle qui accompagne la chaine Ouralique ou cote occidentale fur tout la longueur. Cette suite de montagnes, pour la plupart composees de grais, de marnes rougeatres, entremelees de couches diversement mixtes, forme une chaine par-tout separee par une vallee plus ou moins large de la bande de roche calcaire, dont nous avons parle. Sillonnee et entrecoupee de frequens vallons, elles s’eleve souvent a plus de cent toises perpendiculaires, se repand vers les plaines de la Russie en trainees de collines, qui separent les rivieres, en accompagnant generalement la rive boreale ou occidentale, et degenere enfin en deserts sableux qui occupent de grands espaces, et s’etendent surtout par longues bandes paralleles aux principales traces qui suivent les cours des rivieres. La principale force de ces montagnes tertiaires est plus pres de la chaine primitive par-tout le gouvernement d’Orenbourg et la Permie, ou elle consiste principalement en grais, et contient un fond inepuisable de mines de cuivre sableuses, argileuses, et autres qui se voient ordinairement dans les couches horizontales. Plus loin, vers la plaine, sont des suites de collines toutes marneuses, qui abondent autant en pierres gypseuses, que les autres en minerais cuivreux. Je n’entre pas dans le detail de celles-ci, qui indiquent sur-tout les sources salines; mais je dois dire des premieres, qui abondent le plus et dont les plus hautes elevations des plaines, meme celle de Moscou, sont formees, qu’elles contiennent tres-peu de traces de productions marines, et jamais des amas entiers de ces corps, tels qu’une mer reposee pendant des siecles de suite a pu les accumuler dans les bancs calcaires. Rien, au contraire, de plus abondant dans ces montagnes de grais stratifie sur l’ancien plan calcaire, que des troncs d’arbres entieres et des fragmens de bois petrifie, souvent mineralise par le cuivre ou le fer; des impressions de troncs de palmires, de tiges de plantes, de roseau, et de quelques fruits etrangers; enfin des ossemens d’animaux terrestres, si rares dans les couches calcaires. Les bois petrifies se trouvent jusques dans les collines de sable de la plaine; l’on en tire, entr’autres, des hauteurs sablonneuses aux environs de Sysran sur la Volga, changes en queux tres-fin, qui a conserve jusqu’a la texture organique du bois, et remarquables sur-tout par les traces tres-evidentes de ces vers rongeurs qui attaquent les vaisseaux, les pilotis et autres bois trempes dans la mer, et qui sont proprement originaires de la mer des Indes.”

This philosopher has now given us a view of what, according to the present fashion of mineral philosophy, he has termed *montagnes primitives, secondaires, et tertiaires*. The first consists in masses and strata, much indurated and consolidated, and greatly displaced in their position; but the character of which is chiefly taken from this, that they contain not any visible mark of animal or vegetable bodies.

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The second are formed in a great measure of marine productions, are often no less consolidated than those of the first class, and frequently no less changed in their natural shape and situation.

The third again have for character, according to this learned theorist, the containing of those organised bodies which are proper to the earth, instead of those which in the second class had belonged to the sea; in other respects, surely there is no essential difference. It is not pretended that these tertiary strata had any other origin, than that of having been deposited in water; it is not so much as suspected, that this water had been any other than that of the sea; the few marine bodies which M. Pallas here acknowledges, goes at least to prove this fact: and with regard to the mineral operations which had been employed in consolidating those water formed strata, it is impossible not to be convinced that every effect visible in the other two are here also to be perceived.

From this view of mineral bodies, taken from the extensive observations of the Russian dominions, and from the suppositions of geologists in relation to those appearances, we should be led to conclude that the globe of this earth had been originally nothing but an ocean, a world containing neither plant nor animal to live, to grow and propagate its species. In following a system founded on those appearances, we must next suppose, that to the sterile unorganised world there had succeeded an ocean stored with fish of every species. Here it would be proper to inquire what sustained those aquatic animals; for, in such a system as this, there is no provision made for continuing the life even of the individuals, far less of feeding the species while, in an almost infinite succession of individuals, they should form a continent of land almost composed of their *exuviae*.

If fish can be fed upon water and stone; if siliceous bodies can, by the digesting powers of animals, be converted into argillaceous and calcareous earths; and if inflammable matter can be prepared without the intervention of vegetable bodies, we might erect a system in which this should be the natural order of things. But to form a system in direct opposition to every order of nature that we know, merely because we may suppose another order of things different from the laws of nature which we observe, would be as inconsistent with the rules of reasoning in science, by which the speculations of philosophy are directed, as it would be contrary to common sense, by which the affairs of mankind are conducted.

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Still, however, to pursue our visionary system, after a continent had been formed from the relicts of those animals, living, growing, and propagating, during an indefinite series of ages, plants at last are formed; and, what is no less wonderful, those animals which had formed the earth then disappear; but, in compensation, we are to suppose, I presume, that terrestrial animals began. Let us now reason from those facts, without either constraining nature, which we know, or forming visionary systems, with regard to things which are unknown. It would appear, that at one period of time, or in one place, the matter of the globe may be deposited, in strata, without containing any organised bodies; at another time, or in another place, much animal matter may be deposited in strata, without any vegetable substance there appearing; but at another period, or at another time, strata may be formed with much vegetable matter, while there is hardly to be observed any animal body. What then are we to conclude upon the whole? That nature, forming strata, is subject to vicissitudes; and that it is not always the same regular operation with respect to the materials, although always forming strata upon the same principles. Consequently, upon the same spot in the sea, different materials may be accumulated at different periods of time, and, conversely, the same or similar materials may be collected in different places at the same time. Nothing more follows strictly from the facts on which we now are reasoning; and this is a conclusion which will be verified by every appearance, so far as I know.

Of this I am certain, that in a very little space of this country, in many places, such a course of things is to be perceived. Nothing so common as to find alternated, over and over again, beds of sand-stone without animal bodies, beds of coal and schistus abounding with vegetable bodies, beds of lime-stone formed of shells and corals, and beds or particular strata of iron-stone containing sometimes vegetable sometimes animal bodies, or both. Here, indeed, the strata are most commonly inclined; it is seldom they are horizontal; consequently, as across the whole country, all the strata come up to the day, and may be seen in the beds of our rivers, we have an opportunity of observing that great variety which is in nature, and which we are not able to explain. This only is certain, from what we see, that there is nothing formed in one epoch of nature, but what has been repeated in another, however dissimilar may be the operations which had intervened between those several epochs.

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It must not be alleged, that the heights of the Oural mountains, or the hardness of their rocks, make an essential distinction between them and the argillaceous or arenaceous strata of the plains; solidity and hardness, as well as changes in their height and natural position, has been superinduced in operations posterior to the collection of those masses,—operations which may be formed in various degrees, even in the different parts of the same mass. If this is the case, there can be no difficulty in conceiving a stratum, which appears to be argillaceous or marly in the plains, to be found jasper in the Oural mountains. But there is nothing in the Oural mountains, that may not be found some where or other in the plains, although the soft and easily decomposing argillaceous strata be not found upon the Oural mountains, or the Alps, for this reason, that had those mountains been formed of such materials, there had not been a mountain there at this day.

But surely the greatest possible error, with regard to the philosophy of this earth, would be to confound the sediment of a river with the strata of the globe; bodies deposited upon the surface of the earth, with those sunk at the bottom of the sea; and things which only form the travelled or transported soil, with those which constitute the substratum or the solid earth. How far M. Pallas has committed this oversight, I leave others to determine. After mentioning those strata in which wood is found petrified, and metallic minerals formed, he thus proceeds, (page 69).

“Dans ces memes depots sableux et souvent limoneux, gisent les restes des grands animaux de l’Inde: ces ossemens d’elephans, de rhinoceros, de buffles monstrueux, dont on deterre tous les jours un si grand nombre, et qui font l’admiration des curieux. En Siberie, ou l’on a decouvert le long de presque toutes les rivieres ces restes d’animaux etrangers, et l’ivoire meme bien conserve en si grande abondance, qu’il forme un article de commerce, en Siberie, dis je, c’est aussi la couche la plus moderne de limon sablonneux qui leur sert de sepulture, et nulle part ces monumens etrangers sont si frequens, qu’aux endroits ou la grande chaine, qui domine surtout la frontiere meridionale de la Siberie, offre quelque depression, quelque ouverture considerable.

“Ces grands ossemens, tantot epars tantot entasses par squelettes, et meme par hecatombes, consideree dans leurs sites naturels, m’ont sur-tout convaincu de la realite d’un deluge arrive sur notre terre, d’une catastrophe, dont j’avoue n’avoir pu concevoir la vraisemblance avant d’avoir parcouru ces places, et vu, par moi-meme, tout ce qui peut y servir de preuve a cet evenement memorable[24]. Une infinite de ces ossemens couches dans des lits meles de petites tellines calcinees, d’os de poissons, de glossopetres, de bois charges d’ocre, *etc*. prouve deja qu’ils ont ete transportes par des inondations. Mais la carcasse d’un rhinoceros, trouve avec sa peau entiere, des restes

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de tendons, de ligamens, et de cartilages, dans les terres glacees des bords du Viloui, dont j'ai depose les parties les mieux conservees au cabinet de l'Academie, forme encore une preuve convaincante que ce devait etre un mouvement d'inondation des plus violens et des plus rapides, qui entraina jadis ces cadavres vers nos climats glaces, avant que la corruption eut le tems, d'en detruire les parties molles. Il seroit a souhaiter qu'un observateur parvint aux montagnes qui occupent l'espace entre les fleuves Indighirka et Koylma ou selon le rapport des chasseurs, de semblables carcasses d'elephans et d'autres animaux gigantesques encore revetues de leurs peaux, ont ete remarquees a plusieurs reprises."

[Note 24: Voyez le Memoire, imprime dans le XVII. volume des nouveaux Commentaires de l'Academie Imperiale de Petersbourgh.]

The question here turns upon this, Are the sea shells and glossopetrae, which are thus found deposited along with those skeletons, in their natural state, or are they petrified and mineralised. If the productions of the sea shall here be found collected along with bodies belonging to the surface of the earth, and which had never been within the limits of the sea, this would surely announce to us some strange catastrophe, of which it would be difficult, perhaps, to form a notion; if, on the contrary, those marine productions belong to the solid strata of the earth, in the resolution or decay of which they had been set at liberty, and were transported in the floods, our author would have no reason from those appearances to conclude, there had existed any other deluge than those produced by the waters of the land[25].

[Note 25: Since writing this, I find my doubts in a great measure resolved, in reading M. Pallas's Journal, translated from the German by M. Gauthier de la Peyronie. What I had suspected is, I think, confirmed in the distinct account which M. Pallas has given of those occasions in which the bones of land animals and marine objects are found buried together. The marine objects are mineralised; consequently, they have proceeded from the decomposition of the solid strata; and, having been travelled in the running water of the surface of the earth, they must have been deposited in those beds of rivers, which now are dry, alongst with the bones, or the entire bodies of terrestrial animals, the remains of which are now found there. This argument, from the state of those marine bodies will not be allowed, perhaps by the generality of mineralists, who attribute to the operations of water every species of petrifaction or mineralisation; but, until some species of proof be given with regard to the truth of that theory, which vulgar error first suggested, I must reason from a theory, in proof of which I have given clear examples, and, I think, irrefragable arguments, which shall be more and more illustrated. Thus may be removed the necessity of a general deluge, or any great catastrophe, in order to bring together things so foreign to each other; but at the same time we would ascertain this fact, That formerly the Elephant and Rhinoceros had lived in Siberia. (See Voyage de Pallas, Tom. II. p. 377 and 403.)]

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Having thus endeavoured to remove this prevailing prejudice, of there being primitive parts in this earth, parts of which the composition and constitution are not to be explained upon the principles of natural philosophy, it will be proper to inquire, how far there may be in the theory, which has now been given, principles by which may be explained those appearances that have led natural philosophers to form conclusions, of there being in this earth parts whose origin may not be traced; and of there being parts whose origin may not be explained upon the same principles which apply so well to all the rest.

CHAP. V.

Concerning that which may be termed the Primary Part of the Present Earth.

In the present theory, it is maintained, that there is no part of the earth which has not had the same origin, so far as this consists in that earth being collected at the bottom of the sea, and afterwards produced, as land, along with masses of melted substances, by the operation of mineral causes. But, though all those things be similar, or equal, as to the manner of their production, they are far from being so with regard to the periods of their original composition, or to the subsequent operations which they may have undergone.

There is a certain order established for the progress of nature, for the succession of things, and for the circulation of matter upon the surface of this globe; and, the order of time is associated with this change of things. But it is not in equal portions that time is thus combined with dissimilar things, nor always found, in our estimation, as equally accompanying those which we reckon similar. The succession of light and darkness is that which, in those operations, appears to us most steady; the alternation of heat and cold comes next, but not with equal regularity in its periods. The succession of wet and dry upon the surface of the earth, though equally the work of nature and the effect of regular causes, is often to us irregular, when we look for equal periods in the course of things which are unequal. It is by equalities that we find order in things, and we wish to find order every where.

The present object of our contemplation is the alternation of land and water upon the surface of this globe. It is only in knowing this succession of things, that natural appearances can be explained; and it is only from the examination of those appearances, that any certain knowledge of this operation is to be obtained. But how shall we acquire the knowledge of a system calculated for millions, not of years only, nor of the ages of man, but of the races of men, and the successions of empires? There is no question here with regard to the memory of man, of any human record, which continues the memory of man from age to age; we must read the transactions of time past, in the present state of natural bodies; and, for the reading of this character, we

have nothing but the laws of nature, established in the science of man by his inductive reasoning.

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It has been in reasoning after this manner, that I have endeavoured to prove, that every thing which we now behold, of the solid parts of this earth, had been formerly at the bottom of the sea; and that there is, in the constitution of this globe, a power for interchanging sea and land. If this shall be admitted as a just view of the system of this globe, we may next examine, how far there are to be found any marks of certain parts of our earth having more than once undergone that change of posture, or vicissitude of things, and of having had reiterated operations of the mineral kingdom changing their substance, as well as altering their positions in relation to the atmosphere and sea.

Besides the gradual decay of solid land, exposed to the silent influences of the atmosphere, and to the violent operations of the waters moving upon the surface of the earth, there is a more sudden destruction that may be supposed to happen sometimes to our continents of land. In order to see this, it must be considered, that the continents of our earth are only raised above the level of the sea by the expansion of matter, placed below that land, and rarified in that place: We may thus consider our land as placed upon pillars, which may break, and thus restore the ancient situation of things when this land had been originally collected at the bottom of the ocean. It is not here inquired by what mechanism this operation is to be performed; it is certainly by the exertion of a subterranean power that the land is elevated from the place in which it had been formed; and nothing is more natural than to suppose the supports of the land in time to fail, or be destroyed in the course of mineral operations which are to us unknown. In that case, whatever were remaining of that land, which had for millions of ages past sustained plants and animals, would again be placed at the bottom of the sea; and strata of every different species might be deposited again upon that mass, which, from an atmospheric situation, is now supposed to be lower than the surface of the sea.

Such a compound mass might be again resuscitated, or restored with the new superincumbent strata, consolidated in their texture and inclined in their position. In that case, the inferior mass must have undergone a double course of mineral changes and displacement; consequently, the effect of subterranean heat or fusion must be more apparent in this mass, and the marks of its original formation more and more obliterated.

If, in examining our land, we shall find a mass of matter which had been evidently formed originally in the ordinary manner of stratification, but which is now extremely distorted in its structure, and displaced in its position,—which is also extremely consolidated in its mass, and variously changed in its composition,—which therefore has the marks of its original or marine composition extremely obliterated, and many subsequent veins of melted mineral

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matter interjected; we should then reason to suppose that here were masses of matter which, though not different in their origin from those that are gradually deposited at the bottom of the ocean, have been more acted upon by subterranean heat and the expanding power, that is to say, have been changed in a greater degree by the operations of the mineral region. If this conclusion shall be thought reasonable, then here is an explanation of all the peculiar appearances of the alpine schistus masses of our land, those parts which have been erroneously considered as primitive in the constitution of the earth.

We are thus led to suppose, that some parts of our earth may have undergone the vicissitudes of sea and land more than once, having been changed from the summit of a continent to the bottom of the sea, and again erected, with the rest of that bottom, into the place of land. In that case, appearances might be found to induce natural philosophers to conclude that there were in our land primary parts, which had not the marine origin which is generally to be acknowledged in the structure of this earth; and, by finding other masses, of marine origin, superincumbent upon those primary mountains, they might make strange suppositions in order to explain those natural appearances.

Let us now see what has been advanced by those philosophers who, though they term these parts of the earth *primordial*, and not *primitive*, at the same time appear to deny to those parts an origin analogous to that of their secondary mountains, or strata that are aquiform in their construction.

M. de Luc, after having long believed that the strata of the Alps had been formed like those of the low countries, at the bottom of the sea, gives an account of the occasion by which he was first confirmed in the opposite opinion.[26] Like a true philosopher, he gives us the reason of this change.

[Note 26: Lettres Physique et Morales sur l'Histoire de la Terre, tom. 2. pag. 206.]

"Ce fut une espece de *montagne* tres commune, et que j'avois souvent examinee qui dessilla mes yeux. La pierre qui la compose est de la classe appellee *schiste*; son caractere generique est d'etre *feuilletee*; elle renferme *l'ardoise* dont on couvre les toits. Ces *feuillets* minces, qu'on peut prendre pour des *couches*, et qui le font en effet dans quelques pierres de ce genre, rappelloient toujours l'idee vague de depots des eaux. Mais il y a des masses dont la composition est plutot par fibres que par feuillets, et dont le moellon ressemble aux copeaux de bois d'un chantier. Le plus souvent aussi les feuillets sont situes en toute suite de sens dans une meme *montagne*, et quelquefois meme verticalement, Enfin il s'en trouve de si tortilles, qu'il est impossible de les regarder comme des depots de l'eau.

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“Ce fut donc cette espece de montagne qui me persuada la premiere que toutes les montagnes n’avoient pas une meme origine. Le lieu ou j’abjurai mon erreur, etoit un de ces grands *chantiers* petrifies, qui, par la variete du tortillement, et des zig-zags des fibres du moellon qui le composoit, attira singulierement mon attention. C’etoit un sort grand talus qui venoit d’une face escarpee; j’y montai pour m’approcher du rocher, et je remarquai, avec etonnement, des multitudes de paquets enchevetres les uns dans les autres, sans ordre ni direction fixe; les uns presqu’en rouleaux; les autres en zig-zag; et meme ce qui, separe de la montagne, eut peu etre pris pour des *couches*, le trouvoit incline de toute maniere dans cette meme face de rocher. *Non*, me dis-je alors a moi-meme; *non, l’eau n’a pu faire cette montagne.... Ni celle-la donc*, ajoutai-je en regardant ailleurs.... *Et pourquoi mieux celle-la? Pourquoi toutes les montagnes devroient-elles etre le produit des eaux, seulement parce qu’il y en a quelques-unes qui annoncent cette origine*? En effet, puis qu’on n’a songe aux eaux, comme cause des montagnes, que par les preuves evidentes que quelques-unes offroient de cette formation; pourquoi etendre cette consequence a toutes, s’il y en a beaucoup qui manquent de ces caracteres? C’est comme le dit Mr. d’Alembert, qu’on generalise ses premieres remarques l’instant d’apres qu’on ne remarquoit rien.”

Science is indebted to this author for giving us so clear a picture of natural appearances, and of his own reasoning upon those facts, in forming his opinion; he thus leads astray no person of sound judgment, although he may be in error. The disposition of things in the present case are such, that, reasoning from his principles, this author could not see the truth; because he had not been persuaded, that aquiform strata could have been so changed by the chemical power of fusion, and the mechanical force of bending while in a certain state of softness.

But though, in this case, the reasoning of this philosopher is to be justified, so far as he proceeded upon principles which could not lead him to the truth, his conduct is not so irreproachable in applying them to cases by which their fallacy might have been detected. This author acknowledges calcareous strata to be aquiform in their original; but, in those mountains which he has so much examined, he will find those aquiform bodies have undergone the same species of changes, which made him conclude that those schistus mountains had not been truly aquiform, as he at first had thought them. This would have led him to reason back upon his principles, and to say, *If one species of strata may be thus changed in its texture, and its shape, may not another be equally so? Therefore, may not the origin of both be similar*?

But least I should do injustice to this author, to whom we are indebted for many valuable observations in natural history, I shall transcribe what he has said upon the subject, being persuaded that my readers will not think this improper in me, or impertinent to the argument.

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“Quand nous fumes une fois persuades que la mer n’avoit pas fait toutes les *montagnes*, nous entreprimes de decouvrir les caracteres distinctifs de celles qui lui devoient leur origine; et s’il etoit, par exemple, des matieres qui leur fussent propres. Mais nous y trouvames les memes difficultes qu’on rencontre dans tout ce qu’on veut classer dans la nature. On peut bien distinguer entr’elles les choses qui ont fortement l’empreinte de leur classe; mais les confins echappent toujours.

“C’est la, pour le dire en passant, ce qui a pu conduire quelques philosophes a imaginer cette *chaine des etres* ou ils supposent, que, de la pierre a l’homme et plus haut, les nuances sont reellement imperceptibles. Comme si, quoique les limites soyent cachees a nos sens, notre intelligence ne nous disoit pas, qu’il y a un *saut*, une distance meme infinie, entre le plus petit degre d’organization *propageante*, et la matiere unie par la simple cohesion: entre le plus petit degre de *sensibilite*, et la matiere insensible: entre la plus petite capacite d’observer et de transmettre ses observations, et l’instinct constamment le meme dans l’espece. Toutes ces differences tranchees existent dans la nature; mais notre incapacite de rien connoitre a fond, et la necessite ou nous sommes de juger de tout sur des apparences, nous fait perdre presque toutes les limites, parce que sur ces bords, la plupart des phenomenes sont equivoques. Ainsi la plante nous paroit se rapprocher de la pierre, mais n’en approche jamais reellement.

“On eprouve la meme difficulte a classer les montagnes; et quoique depuis quelque tems plusieurs naturalistes ayent aussi observe qu’elles n’ont pas toutes la meme origine, je ne vois pas qu’on soit parvenu a fixer des caracteres infaillibles, pour les placer surement toutes dans leurs classes particulieres.

“Apres avoir examine attentivement cet objet, d’apres les phenomenes que j’ai moi-meme observes, et ce que j’ai appris par les observations des autres; j’ai vu que c’etoit la un champ tres vaste, quand on vouloit l’embrasser en entier, et trop vaste pour moi, qui n’etoit pas libre d’y consacrer tout le tems qu’il exige. Je me suis donc replie sur mon objet principal, savoir *la cause qui a laisse des depouilles marines dans nos continens*, et l’examen des hypotheses sur cette matiere.

“Les phenomenes ainsi limites, se reduisent a ceci: qu’il y a dans nos continens des montagnes visiblement formees par des *depots successifs de la mer* et a l’egard des quelles il n’y a besoin de rien imaginer, si ce n’est la maniere dont elles en sont sorties: qu’il y en a d’autres au contraire, qui ne portent aucun des caracteres de cette cause, et qui, si elles ont ete produites dans la *mer*, doivent etre l’effet de toute autre cause que de simples depots successifs, et avoir meme precede l’existence des animaux marins. J’abandonne donc les classes confuses ou ces caracteres sont equivoques, jusqu’a ce qu’elles servent a fonder quelque hypothese; ayant assez de ces deux classes tres distinctes pour examiner d’apres elles tous les systemes qui me sont connus.

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“La ou ces deux classes de montagnes sont melees, on remarque que celles qui sont formees par *couches*, et qui renferment des *corps marins*, recouvrent souvent celles de l’autre classe, mais n’en sont jamais recouvertes. On a donc naturellement conclu, que lors meme que la *mer* auroit en quelque part a la formation des montagnes ou l’on ne reconnoit pas son caractere, celles auxquelles elle a travaille seule, en enlevant des matieres dans certaines parties de son fond et les deposant dans d’autres, font au moins les dernieres formees. On les a donc nommees *secondaires*, et les autres *primitives*.

“J’adopterai la premiere de ces expressions; car c’est la meme qui nous etoit venu a l’esprit a mon frere, et a moi longtemps avant que nous l’eussions vue employer; mais je substituerai celle de *primordiales a primitives* pour l’autre classe de *montagnes*, afin de ne rien decider sur leur origine. Il est des *montagnes*, dont jusqu’a present on n’a pu demeler la cause: voila le fait. Je ne dirai donc pas qu’elles ont ete creees ainsi, parce qu’en physique je ne dois pas employer des expressions sur lesquelles on ne s’entend pas. Sans doute cependant, que l’histoire naturelle ni la physique ne nous conduisent nullement a croire que notre globe ait existe de toute eternite; et lorsqu’il prit naissance, il fallut bien que la matiere qui le composa fut de quelque nature, ou sous quelque premiere forme integrante. Rien donc jusqu’ici n’empeche d’admettre que ces *montagnes* que je nommerai *primordiales*, ne soient reellement *primitives*; je penche meme pour cette opinion a l’egard de quelques unes. Mais il y a une tres grande variete entr’elles; et quoiqu’elles soyent toutes egalement exclues de la classe *secondaire*, elles ne sont pas toutes semblables: il y en a meme un grand nombre dont les matieres ont une certaine configuration qui semble annoncer qu’elles ayent ete molles et durcies ensuite, quoique par une toute autre cause que celle qui a agi pour former les montagnes secondaires.”

Here I would beg leave to call the attention of philosophers to this observation of a naturalist who explains all petrification, and the consolidation of strata by aqueous infiltration. If he has here found reason to conclude that, in those primordial parts of the earth, there are a great number which, from their present configuration, must have been in a soft state and then hardened, and this by a quite different cause from that which he supposes had produced the consolidation and hardness of the secondary parts; this is entering precisely into my views of the subject, in ascribing all the consolidation of the earth, whether primary or secondary, to one general cause, and in tracing this cause, from its effects, to be no other than the fusion of those bodies. It must be evident, that if this philosopher has seen good reason for concluding such a softening cause, which had operated upon the primary parts, to be quite different from that which he ascribes to the consolidation of the secondary, which is the effect of water, it must then, I say, be evident that the softening cause of the primary parts, if not heat, by which every degree of fusion may be produced, must be an occult cause, one which cannot be admitted into natural philosophy.

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By thus choosing to consider mountains as of two distinct kinds, one aquiform which is understood, and the other primordial which is not to be known, we supersede the necessity of reconciling a theory with many appearances in nature which otherwise might be extremely inconvenient to our explanation, if not inconsistent with our system. Our author no doubt has thus relieved himself from a considerable difficulty in the philosophy of this earth, by saying here is a great part which is not to be explained. But I would beg leave to observe, that this form of discussion, with regard to a physical subject, is but a mere confession of our ignorance, and has no tendency to clear up another part of the subject of which one treats, however it may impress us with a favourable opinion of the theorist, in allowing him all the candour of the acknowledgement.

The general result of the reasoning which we now have quoted, and what follows in his examination, seems to terminate in this; that there are various different compositions of mountains which this author cannot allow to be the production of the sea; but it is not upon account of the matter of which they are formed, or of the particular mixture and composition of those species of matter, of which the variety is almost indefinite. According to this philosopher, the distinction that we are to make of those primordial and secondary competitions, consists in this, that the first are in such a shape and structure as cannot be conceived to be formed by subsidence in water.

M. de Saussure has carefully examined those same objects; and he seems inclined to think that they must have been the operation of the ocean; not in the common manner of depositing strata, but in some other way by crystallization. The present theory supposes all those masses formed originally in the ordinary manner, by the deposits or subsidence of materials transported in the waters, and that those strata were afterwards changed by operations proper to the mineral regions.

But the subject of the present investigation goes farther, by inquiring if, in the operations of the globe, a primary and secondary class of bodies may be distinguished, so far as the one may have undergone the operations of the globe, or the vicissitudes of sea and land, oftener than the other, consequently must be anterior to the later productions both in time and operation, although the original of all those bodies be the same, and the operations of the earth, so far as we see in the effects, always proceed upon the same principles. This is an extensive view of nature to which few have turned their thoughts. But this is a subject to which the observations described by this author have evidently a reference.

In his 113th letter, he has given us a view of one of those parts of the earth that are proper to be examined in determining this question so important in the genealogy of land, although no ways concerned in altering the principles upon which nature in forming continents must proceed.

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It is in describing the nature of the mountains about *Elbingerode*; and he begins in ascending from Hefeld.

“Cette partie exterieure de la chaine est *primordiale*: c’est du *granit* a *Hereld* et au commencement de la route; puis quand on passe dans d’autres vallees, on trouve les *schistes* et la *roche grise* dans tout le pied des montagnes: mais des qu’on est arrive a une certain hauteur, on voit de la *pierre a chaux* par couches etendue sur ces matieres; et c’est elle qui forme le sommet de ces memes montagnes; tellement que la plaine elevee, qui conduit a *Elbingerode*, est entierement de *pierre a chaux*, excepte dans sa partie la plus haute ou cette pierre est recouverte des memes *gres* et sables *vitrescibles* qui sont sur le schiste du Bruchberg et sur la *pierre a chaux* dans la *Hesse* et le pays de Gottingue.

“Les environs d’Elbingerode etant plus bas que ces parties recouvertes de matieres vitrescibles, montrent la *pierre a chaux* a nud; et l’on y trouve de tres beaux marbres, dont les nuances jaunes, rouges et vertes sont souvent tres vives, et embellies par les coupes des *corps marins*.

“Cependant le schiste n’est pas enseveli partout sous ces depots de la mer; on le retrouve en quelques endroits, et meme avec de *filons*.

“Ainsi au milieu de ces matieres *calcaires* qui forment le sol montueux des environs *d’Elbingerode*, paroit encore le *schiste* sur lequel elles ont ete deposees: Et en montant a la partie la plus elevee de ces memes environs, on trouve que la *pierre a chaux* est recouverte elle-meme d’une *pierre sableuse* grise par couches, dans laquelle on voit quantite de petits fragmens de *schiste* poses de plat. C’est la que se trouve une des mines de *fer* dont le minerai va en partie a la *Koningshutte*, mais en plus grande partie a la *Rothechutte*, qui n’est qu’a une lieue de distance. On perce d’abord la couche sableuse; sous elle se trouve de la *pierre a chaux* grise; puis une couche de *pierre a chaux ferrugineuse*, remplie de *corps marins*, et surtout *d’entroques*: C’est cette *couche* qui est ici le *minerai*; et elle appartient a la formation de cette eminence comme toutes les autres *couches*. Cette mine se nomme *bomshey*: elle n’est pas riche; mais elle sert de *fondant* aux matieres ferrugineuses tirees des filons des montagnes primordiales en meme tems qu’elle leur ajoute son *fer* dans la fonte. A quelque distance de la on a perce un autre puits; qui a transverse d’abord une sorte de pierre, que je ne saurois nommer, mais qui ressemble fort a une *lave* poreuse. Au dessous de cette couche on a retrouve la *pierre a chaux* ordinaire; puis la *couche ferrugineuse* y continue; mais elle differe un peu de ce qu’elle est dans l’autre mine, une partie de sa substance etant convertie en *jaspe*.

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“Mais ce qui est digne de la plus grande attention dans cette contree, est un filon peu distant nomme *Buchenberg*, qui appartient en partie au Roi, et en partie a Mr. le Comte de *Wernigerode*. La montagne en cette endroit montre une vallee artificielle de 70 a 80 pieds de profondeur, de 20 a 30 de largeur dans le haut, et de 400 toises en etendue. C’est le creusement qu’on a deja fait en suivant ce *filon* de *fer*, que l’on continue a exploiter de la meme maniere sur les terres de Mr. le Comte de *Wernigerode*. La matiere propre de la montagne *est* de *schiste*; et la vallee qui se forme de nouveau a mesure qu’on enleve la *gangue* du *filon*, a surement deja existe dans la mer sous la forme d’une *fente*, qui a ete remplie, et en particulier des ingrediens dont on fait aujourd’hui le *fer*.”

Here is a supposition of our author that corresponds to nothing which has yet been observed any where else, so far as I know. It is concerning a mineral vein, one which does not appear to differ in any respect from other mineral veins, except in being worked in that open manner which has given our author an idea of its being a valley. He then supposes that valley (or rather empty vein) to have been in this mountain when at the bottom of the sea, and that this mineral vein had then been filled with those materials which now are found in that space between the two sides of the separated rock. This is a very different operation from that of infiltration, which is commonly supposed to be the method of filling mineral veins; but, we shall soon see the reason why our author has here deserted the common hypothesis, and has adopted another to serve the occasion, without appearing to have considered how perfectly inconsistent those two suppositions are to each other. That mineral veins have been filled with matter in a fluid state, is acknowledged by every body who has either looked at a mineral vein in the earth, or in a cabinet specimen; mineralists and geologists, in general, suppose this to have been done by means of solutions and concretions, a supposition by no means warranted by appearances, which, on the contrary, in general demonstrate that the materials of those veins had been introduced in the fluid state of fusion. But here is a new idea with regard to the filling of those veins; and, I would now beg the reader’s attention to the facts which follow in this interesting description, and which have suggested that idea to our author.

“Quand cette matiere accidentelle est enlevee, on voit la coupe du *schiste* des deux cotes de la *fente*, faisant un *toit* et un *mur*, parce que la *fente* n’est pas absolument verticale: des qu’il y a un peu d’inclinaison, on distingue un *toit* et un mur, comme j’ai l’honneur de l’expliquer a V.M. On ne connoit point encore l’etendue de ce filon, ni dans sa profondeur, ou l’on ne peut pas s’enfoncer beaucoup de cette maniere, ni dans la longueur, selon laquelle on continue a l’exploiter.

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“Voila donc un *filon*, a la rigueur de la definition que j’en ai donne a V.M. c’est a dire, une *fente* dans la montagne naturelle, *comblee* de *matiere* etrangere. Mais ce qu’il y a d’extraordinaire ici, c’est que cette *matiere* vient de la *mer*: ce sont differentes *couches aquiformes*, dont quelques unes sont remplies de *corps marins*. Il y a des *couches* d’une *terre martiale* fort brune et sans liaison: d’autres, au contraire toujours *martiales*, sont tres dures et renferment de tres beau jaspe sanguin: d’autres enfin sont de vrai *marbre* gris veinees de rouge. C’est dans ce marbre que font les *corps marins*, savoir des coquillages et des spongites; et il est lui-meme martial comme tout le reste: les mineurs le nomment *Kubrimen*, et ne l’employent que comme un *fondant* pour d’autres *mineraux de fer*.

“A ce *filon*, s’en joignent d’autres plus embarrassans. Ils viennent du *toit*, qu’ils divisent par de larges *fentes* comblees, aboutissantes au *filon* principale. Ils font de meme *calcaires* et marins faits par *couches*; mais ces *couches* ont une si grande inclinaison, que je ne puis les comprendre: il faut qu’il y ait eu d’etranges bouleversemens dans ces endroits-la[27].

[Note 27: Here, no doubt, are appearances which it is impossible to explain by the theory of infiltration; it is the filling of mineral veins, and their branches or ramifications, with marble containing marks of marine objects. But, if we shall suppose this marble to have been in the fluid state of fusion, as well as the iron-ore and jasper, we may easily conceive it introduced into the principal vein and its branches. The description here given of those appearances is by no means such as to enable us to judge particularly of this case, which surely merits the most accurate investigation, and which, I doubt not, will give physical demonstration of the fusion of those mineral substances. I know that shells have been found within the body of veins in Germany; but, a stratification of those materials in a vein was never heard of before, so far as I know.]

“Ces *fentes* se sont faites, et ont ete remplies, dans la *mer*; puisque les matieres qui les remplissent sont de la classe de ses depots tres connoissables, et qu’il contiennent des *depouilles marines*. Mais ce qui embarrasse alors c’est que les autres *filons* ne soyent pas dans le meme cas. N’est ce point la encore un indice, que ces *fentes* out ete d’abord et principalement remplies de matieres, poussees du fond par la meme force qui secouoit les montagnes[28].

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[Note 28: But what is this power by which matter is to be forced from the bottom of the sea to the top of the mountains? For, unless we can form some idea of that power which, as a cause, we ascribe to the perceived effect, we either say nothing to the purpose, or we employ a preternatural cause. It is not sufficient to imagine a power capable of raising from the bottom of the sea the materials deposited in the abyss; it is also necessary to find a power capable of softening bodies which are hard, and of thus consolidating those masses which are formed of loose or unconnected materials. Such a power, indeed, the present theory assumes; and, so far as this shall be implied in the supposition of our author, it will thus have received a certain conformation.]

“Ce *filon* n’est pas le seul dans le *Hartz* qui donne des signes *marins*. Il y en a un autre, qui meme se rapproche davantage de la nature du commun des *filons*, et ou l’on trouve aussi des *coquillages*. C’est celui de *Haus-Hartzbergerzug*, pres de *Clausthal*, ou, dans les *Halles* de quelques mines de plomb abandonnees, et dans une forte *d’ardoise*, on trouve de petites *moules* ou *tellines* striees, d’une espece particuliere que j’ai vue dans des *ardoises secondaires d’Arotzen* en *Waldek* et de *Sombernon* en *Bourgogne*. Il y a donc certainement quelques *filons* faits par les depots de la *mer* dans les *fentes* de montagnes *primordiales*; comme au contraire il y a des *filons* metalliques sans indices *marins*, dans des montagnes evidemment *secondaires*, telles que celles de *Derbyshire*, ou les *filons* de *plomb* traversent des couches de *pierre a chaux*.”

Here again our author seems to me to refute his own supposition, That a chasm in the schistus rock may have existed at the bottom of the sea, and been then filled from above with such materials as were transported by the moving water to that place, is not impossible; but nobody, who knows the nature of a common metallic vein, can ever suppose it to have been filled in that manner. Our author then adds, “On ne fait reellement que commencer dans ce genre d’observations, considerees quant a la Cosmologie; ainsi il ne faut point desesperer que tout cela ne se devoile un jour, et que nous n’acquerrions ainsi un peu plus de connoissance sur ce qui se passoit dans la *mer ancienne*.

“En revenant vers *Elbingerode*, nous retrouvames ces *schistes*, qui paroissent au travers des *marbres:* ils sont donc la continuation de la masse *schisteuse* a laquelle appartient le *filon*, dont je viens de parler. Ce *filon* a ete forme dans une *fente*, restee ouverte et vide: les depots de la *mer* l’ont comblee, en meme tems qu’ils formoient les couches de *marbre*, qui sont a l’exterieur. En effet, ce *filon* contient des *couches marines ferrugineuses*, de la meme nature que celles des collines calcaires voisines formees sur le schiste.

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“Nous partimes *d’Elbingerode* dans l’apres midi pour nous rapprocher de Clausthal. Notre chemin fut encore quelque tems sur des sommites *calcaires*; et avant que d’en sortir, nous trouvames une autre mine singuliere a *Arenfeld*. C’est encore un vrai *filon*; mais dans une montagne de *pierre a chaux:* C’est a-dire, que cette montagne a aussi ete *fendue*, et que la *fente* a ete remplie d’une *gangue*. La matiere de ce *filon* est encore *calcaire* en plus grande partie; mais cette *pierre a chaux* distincte est *ferrugineuse*, et parsemee de concretions de *jaspe* comme celles *d’Elbingerode:* on y trouve aussi une matiere verdatre, qui, comme le *jaspe*, ne fait pas effervescence avec l’eau forte.”

Here is a phenomenon which is altogether incompatible with the theory that this author has given us for the explanation of those appearances. He supposes empty crevices in the schistus mountains at the bottom of the sea; these crevices he supposes filled by the deposits of the sea, at the same time, and with the same materials with which the lime-stone strata were formed above the schistus mountains; but we find one of those same veins in these secondary calcareous strata. Now, tho’ we should be disposed to allow, that, in the primordial mountain, of which we are supposed not to know the origin, there might have been empty crevices which were afterwards filled with materials transported by the sea, this cannot be admitted as taking place in the loose or incoherent materials deposited above the schistus. Consequently, this theory of our author, which is evidently erroneous with regard to the veins in the lime-stone, must, in the other case, be at least examined with a jealous eye.

“Le haut de cette partie des montagnes *calcaires* etoit encore recouvert de *sable* et de gres *vitrescibles*: et continuant a marcher, sans aucune inflexion sensible, nous nous trouvames subitement sur les *schistes*; d’ou nous montames plus rapidement. Puis traversant quelques petites vallees nous arrivames sur les montagnes qui appartiennent au prolongement du *Brocken* ou *Blocksberg*. La matiere dominante est alors le *granit*; mais il est tout en blocs le long de cette route, et ces blocs se trouvent a une telle distance de tout sommite intacte de cette pierre, qui est aise de juger non seulement qu’ils ne sont pas dans leur place originaire, mais encore qu’il ne sont arrives la par aucune des causes naturelles qui agissent dans les montagnes; savoir, la pesanteur, la pente, et le cours des eaux. Ce sont donc de violentes explosions qui ont disperse ces blocs; et alors ils deviennent un nouveau trait cosmologique de quelque importance: car rien ne se meut, ni ne paroit s’etre mu depuis bien des siecles, dans ces lieux qui montrent tant de desordre: un tapis de verdure couvre tout, en conservant les contours baroques du sol. Le betail ne sauroit paturer dans de telles prairies; mais l’industrieux montagnard fait y faucher[29].

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[Note 29: M. de Saussure endeavours to explain those appearances of transported blocks of granite by another cause; this is a certain *debacle* of the waters of the earth, which I do not understand. M. de Luc again attempts to explain it by violent explosions; I suppose he means those of a volcano. But he has not given us the evidence upon which such an opinion may be founded, farther than by saying that those blocks could not have come there by the natural operations of the surface. By this must be meant, that, from the nearest summit of granite, there is not, at present, any natural means by which these blocks might be transported to that place. But it is not with the present state of things that we are concerned, in explaining the operations of a distant period. If the natural operations of the surface change the shape of things, as is clearly proved by every natural appearance, Why form an argument against a former transaction, upon the circumstances of the present state of things? Our author does not seem to perceive, that, from this mode of reasoning, there is is an insuperable objection to his violent explosions having been employed in producing those effects. For, had there been such a cause, the evidence of this must have remained; if the surface of the earth does not undergo great changes: If, again, this surface be in time much changed, How can we judge from the present shape, what might have been the former posture of things?

This author, indeed, does not allow much time for the natural operations of the globe to change its surface; but, if things be not greatly removed from the state in which the violent operations of the globe had placed them, Why does he not point out to us the source of this great disorder which he there perceives? From what explosion will be explained the blocks of granite which are found upon the Jura, and which must have come from the mass of *Mont Blanc*? If these dispersed blocks of stone are to be explained by explosion, there must: have been similar explosions in other countries where there is not the smallest appearance of volcanic eruptions; for, around all our granite mountains, and I believe all others, there are found many blocks of granite, traveled at a great distance, and in all directions.]

“*Oberbruck*, ou nous avions ete la precedente fois, se trouva sur notre route, et nous y passames aussi la nuit, dans l’esperance de pouvoir monter le lendemain sur le *Brocken*; mais il fut encore enveloppe de nuages; ainsi nous continuames a marcher vers *Clausthal*, passant de nouveau par le *Bruchberg*, ou le *sable* et ses gres recouvrent le *schiste*; puis arrivant a une autre sommite, nous y trouvames la meme pierre *sableuse* par couches, melee de parcelles de *schiste*, que nous avions vue sur les montagnes *calcaires d’Elbingerode*. Il est donc toujours plus certain que le sol primordial de toutes ces montagnes existoit sous les eaux de

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l'ancienne mer; puisqu'il est recouvert de diverses fortes de depots, connus pour appartenir a la *mer*; et que les *fentes* des *filons* existoient dans cette *mer ancienne*; puisqu'elle en a rempli elle-meme quelques unes, et qu'elle a recouvert de ses depots quelques autres *filons* tout formes. Quant a celles des matieres de ces *filons*, qui ne paroissent pas etre *marines* (et c'est de beaucoup la plus grande quantite), j'ai toujours plus de penchant d'en attribuer une partie a l'operation des *feux souterreins*, a mesure que je vois diminuer la probabilite de les assigner entierement a *l'eau*. Mais quoi-qu'il en soit, ces gangues ne font pas de meme date que les montagnes[30].

[Note 30: I most willingly admit the justness of our author's view, if he thus perceives the operation of fire in the solids of our earth; but it is not for the reasons he has given us for discovering it here more than in other places; for there is not a mineral vein, (so far at least as I have seen), in which the appearances may be explained by any thing else besides the operation of fire or fusion. It is not easy to conceive in what manner our author had conceived the opinions which he has displayed in these letters. He had no opinion of this kind, or rather he was persuaded that subterraneous fire had no hand in the formation of this earth before he came to this place of the Hartz; here he finds certain appearances, by which he is confirmed in his former opinion, that water had operated in forming mineral veins; and then he forms the idea that subterraneous fire may have operated also. But, before the discovery of the chasms in the schistus mountains having been filled with the stratified materials of the sea, How had he supposed veins to be filled? If this philosopher had before no opinion of subterraneous fire, as instrumental in that operation, How comes he now to change that former opinion? For, unless it be the extraordinary manner of filling these open crevices in the mountains by matter deposited immediately from the sea, there is certainly no other appearance in this mineral country of the Hartz, that may not be found in any other, only perhaps upon a smaller scale.]

"Le lendemain de notre arrivee a *Clausthal*, qui etoit le 13e, nous allames visiter d'autres mines de *fer* en montagnes secondaires, situees au cote oppose du Hartz. Elles sont aupres de *Grund* l'une des *villes de mines*, et pres du lieu ou sortira la nouvelle *galerie d'ecoulement* a laquelle on travaille, *etc*.

"Arrives a *Grund* les officiers mineurs vinrent, comme a l'ordinaire, accompagner *Mons*. de *Reden* aux *mines* de leur departement. Celles-ci, sans etre plus extraordinaires que celles qui nous avions vues a *Elbingerod*, ou sans aider mieux jusqu'ici a expliquer ce qu'elles ont toutes d'extraordinaire, nous donnent au moins des indices probables de grands accidens.

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Ces montagnes de *Grund* sont encore de l'espece remarquable, dont la base est de *schiste*, et le haut de *pierre a chaux*. Les mines qu'on y exploit sont de *fer*, et se trouvent dans cette matiere *calcaire*; mais elles y sont sous des apparences tout-a-fait etranges. La montagne ou nous les vimes principalement le nomme *Iberg*. On y poursuit des masses de *pierre a fer*, de l'ensemble desquelles les mineurs ne peuvent encore se rendre compte d'une maniere claire. Ils ont trouve dans cette montagne des *ca*_vernes_, qui ressemblent a l'encaissement de *sillons* deja exploites, ou non formes; c'est-a-dire, que ce sont des *fentes* presque verticales, et vides, Le *minerai* qu'ils poursuivent est en *Rognons*; c'est a dire, en grandes masses sans continuite decidee. Cependant ces masses semblent se succeder dans la montagne suivant une certaine direction; tellement que les mineurs savent deja les chercher, par des indices d'habitude. La substance de cette *pierre a fer* particuliere renferme des crystallizations de diverses especes. Il y a des *druses de quartz*, ou de petits cristaux de quartz qui tapissent des cavites; il y a aussi du *spath* commun, et de celui qu'on nomme pesant; on y trouve enfin une forte de crystallization nommee *Eisenman* (*homme de fer*) par les mineurs; se sont des amas de cristaux noir-atres, qui ressemblent a des groupes de grandes lentilles plattes, et ces cristaux sont *ferrugineux*.

"Entre les signes de bouleversement que renferme ce lieu, est un rocher nomme *Gebichensten*, qui est en *pierre a chaux*, ce que *l'Ebrenbreitstein* de *Coblentz* est en pierre sableuse: c'est-a-dire, que ses *couches*, remplies de *corps marins*, sont presque verticales; ceux de ces corps qu'on y trouve en plus grande quantite, sont des *madrepores*. Ce rocher s'eleve comme un grand obelisque, au-dessus des *cavernes*, dont j'ai parle; montrant par le cote ses *couches*, qui se trouvent, comme je l'ai dit, dans une situation presque verticale. Sa base est deja bien minee, tant par les *cavernes*, que par la *pierre a fer* qu'on en tire; et je ne me hasardai dessus, que parce que je me dis, qu'il y a des millions contre un a parier, que ce n'est pas le moment ou il s'enfoncerait. Mais je n'en dirois pas autant, s'il s'agissoit de m'y loger a demeure.

"Quoique tout ce lieu la soit fort remarquable, il se pourrait que ce ne fut qu'un phenomene particulier. Les *cavernes* peuvent devoir leur origine a la meme cause que celle de Schartzfeld; et le derangement des rochers superieurs a des enfoncemens occasionnes par ces *cavernes*. Rien n'est si difficile que de retracer aujourd'hui ces fortes d'accidens a cause des changemens que le tems y a operes. S'ils sont arrives sous les eaux de la *mer*, on concoit aisement les alterations qui ont du succeder; et si c'est depuis que nos continens sont a sec, les eaux encore, tant interieures qu'exterieures, et la vegetation, en ont beaucoup change l'aspect."

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This author has a theory by which he explains to himself the former residence of the sea, above the summits of our mountains; this, however, is not the theory by which we are now endeavouring to explain appearances; we must therefore be allowed to reason from our own principles, in considering the facts here set forth by our author.

Nothing, I think, is more evident, than that in this mineral country of the Hartz, we may find the clearest marks of fracture, elevation, and dislocation of the strata, and of the introduction of foreign matter among those separated bodies. All those appearances, our author would have to be nothing but some particular accident, which is not to enter into the physiology of the earth. I wish again to generalise these facts, by finding them universal in relation to the globe, and necessarily to be found in all the consolidated parts of our land.

It was not to refute our author's reasoning that I have here introduced so much of his observations, but to give an extensive view of the mineral structure of this interesting country. This therefore being done, we now proceed to what is more peculiarly our business in this place, or the immediate subject of investigation, *viz*. the distinction of primary and secondary strata.

"Dans le voisinage de cette montagne, il y a une autre fort interessante, que je vis le jour suivant. Quoiqu'en traitant des volcans, j'aie demontre que la formation des montagnes, par soulevement, etoit sans exemple dans les faits, et sans fondement dans la theorie, je ne laisseroi pas de m'arreter au phenomene que presente cette montagne; parce qu'il prouvera directement que les *couches calcaires* au moins, ont ete formees *a la hauteur ou elles sont*; c'est-a-dire qu'elles n'ont pas ete soulevees.

"Voulant prendre l'occasion de mon retour a *Hanovre*, pour traverser les avant-corps du *Hartz*, dans quelque nouvelle direction; je resolus de faire ce voyage a cheval, et de prendre ma route droite vers *Hanovre*, au-travers des collines; ce qui me conduisit encore a *Grund* puis a *Muenchehof Brunshausen, Engelade, Winsenburg* et *Alfeld*, ou enfin, traversant la *Leine* j'entrai dans la grande route.

"Je quittai donc *Clausthal* (et avec bien du regret) le 14 au matin; et revenant d'abord a *Grund*, je le laissai sur ma droite, ainsi que *l'Iberg*; et plus loin, du meme cote, une autre montagne nommee *Winterberg* dont la base est *schiste*, et le sommet plus haut que Clausthal, entierement compose de *couches calcaires*. De *Grund* je montai vers une montagne nommee *Ost Kamp*; et je commencai la a donner une attention particuliere au sol. Le long de mon chemin, je ne trouvai longtemps que des schistes, qui montroient leurs points en haut, comme a l'ordinaire, et avec tous leurs tortillemens de feuillets. Mais arrive au haut de la montagne, j'y vis des carrieres

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de *pierre a chaux*, ou les couches absolument regulieres, et qui ont peu d'epaisseur sur le *schiste* suivent parfaitement les contours du *sommet*. Ces lits de *pierre a chaux* n'ont certainement pas ete souleves du fond de la *mer* sur le dos des schistes; lors meme qu'a cause de la grande inclinaison des feuillets de ceux-ci on voudroit le attribuer a quelque revolution telle que le *soulevement*; (ce que je n'admettrois point). Car si ces lits *calcaires*, ayant ete faits au fond de la *mer*, avoyent ete souleves avec les schistes, ne feroient-ils pas brises et bouleverses comme eux? Il est donc evident, que quoiqu'il soi arrive au schiste qui les porte, ces lits, et tous les autres de meme genre qui sont au haut de ces montagnes, ont ete deposees au niveau ou ils sont; et que par consequent la *mer* les surpassoit alors. Ainsi le systeme de soulevement perd son but, s'il tend a expliquer pourquoi nous avons des *couches*, formees par la mer, qui se trouvent maintenant si fort au dessus de son niveau. Il est evident que ces *couches* n'ont pas ete soulevees; mais que la *mer* s'est *abaissee*. Or c'est la le grand point cosmologique a expliquer: tous les autres, qui tiennent a la structure de certaines montagnes inintelligibles, n'appartiendront qu'a *l'histoire naturelle*, tant qu'ils ne se lieront pas avec celui-la."

Here are two things to be considered; the interesting facts described by our author, and the inference that he would have us draw from those facts. It would appear from the facts, that the body of schistus below, and that of lime-stone above, had not undergone the same disordering operations, or by no means in the same degree. But our author has formed another conclusion; he says, that these lime-stone strata must have been formed precisely in the place and order in which they lie at present; and the reason for this is, because these strata appeared to him to follow perfectly the contour of the summit of this mountain. Now, had there been in the top of this mountain a deep hollow encompassed about with the schistus rock; and had this cavity been now found filled with horizontal strata, there might have been some shadow of reason for supposing those strata to have been deposited upon the top of the mountain. But to suppose, *first*, that shells and corals should be deposited upon the convex summit of a mountain which was then covered by the sea; *secondly*, that these moveable materials should remain upon the summit, while the sea had changed its place; and, *lastly*, that those shells and corals left by the sea upon the top of a mountain should become strata of solid limestone, and have also metallic veins in it, certainly holds of no principle of natural philosophy that I am acquainted with. If, therefore, such an appearance as this were to be employed either in illustration or confirmation of a theory, it would itself require to be explained; but this is a task that this cosmologists does not seem willing to undertake.

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He has formed a hypothesis for explaining the general appearance of that which was once the bottom of the sea being now found forming the summits of our mountains; but surely this philosopher will acknowledge, that those natural appearances, in any particular place, will be the same, whether we suppose the bottom of the sea to have been raised, as in the present theory, or the surface of the sea to have sunk according to his hypothesis. For, it is equally easy to suppose a portion of the earth to have been raised all this height, as to suppose all the rest of the surface of the globe to have sunk an equal space, while a small portion of the bottom of the sea, remaining here and there fixed in its place, became the highest portion of the globe. Consequently, whatever evidence this philosopher shall find in support of his theory of the present earth, (a subject which it is not our purpose to examine) it cannot be allowed that he has here brought any argument capable of disproving the elevation of the bottom of the sea; a supposition which other theories may require.

I would now observe, in relation to the present theory, that so far as this author has reasoned justly from natural appearances, his conclusions will be found to confirm the present supposition, that there is to be perceived the distinction of primordial, and that of secondary, in the masses of this earth, without altering the general theory either with respect to the original formation of those masses, or to their posterior production.

Here one of two things must be allowed; either that those strata of schistus had been broken and distorted under a mass of other superincumbent strata; or that those superincumbent strata had been deposited upon the broken and distorted strata at the bottom of the sea. Our author, who has examined the subject, inclines to think, that this last has been the case. If, therefore, strata had been deposited upon broken and bare rocks of schistus, it is probable that these had been sunk in the sea after having been exposed to the atmosphere, and served the purpose of land upon the globe.[31]

[Note 31: This is also supported by another very interesting observation contained in this letter. M. de Luc observes, that in this country the schistus is generally covered by strata of lime-stone, and that these lime-stone strata are again covered with those of sand-stone, in which are found a great many fragments of schistus lying flat. Therefore, while those sand-stone strata were collecting at the bottom of the sea, there had been rocks of schistus in some other place, from whence those fragments bad been detached.]

An example of the same kind also occurs in the *Discours sur l'Histoire Naturelle de la Suisse*; and this author of the *Tableaux de la Suisse* has given a very distinct description of that appearance, which is perhaps the more to be valued as a piece of natural history, as this intelligent author does not pretend to any geological theory, but simply narrates what he has seen, with such pertinent observations on the subject as naturally must occur to a thinking person on the spot.—(Discours, *etc*. page 228. Entree au pays de Grisons).

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“Du village d’Elen on continue a monter le reste du petit vallon pendant une lieue et demie parmi les memes especes de pierres qu’on vient de decrire; en passant au travers de bois et de forets de sapins et de quelques paturages dont ce haut est couvert, on parvient au pied du Bundnerberg, montagne des grisons, qui forme la tete du vallon. On laisse a droite un fond ou espece d’entonnoir, entoure de tres-hautes montagnes inaccessibles, pour s’enfourrer a gauche entre des rochers qui font fort resserres, ou coule un torrent. Ce lieu seroit horreur si on ne se trouvoit accoutume, par degres, a voir de ces positions effrayantes: tout y est aride, il n’y a plus d’arbres ni de vegetaux ce sont des rochers entasses les un sur les autres; ce lieu paroit d’autant plus affreux que le passage a ete subit, et qu’en sortant de bois et des forets, on se trouve tout-a-coup parmi ces rochers qui s’elevent comme des murailles, et dont on ne voit pas la cime; cette gorge ou cette entree qui se nomme Jetz, est la communication du Canton du Glaris aux Gritons; on a dit precedemment qu’il y en avoit une plus aisee par le Gros-Thal ou le grand vallon. Ce passage est tres-curieux pour la Lithogeognosie, il est rare de trouver autant de phenomenes interessans rassembles, et des substances aussi variees par rapport a leurs positions; c’est le local qui merite le plus d’etre examine en Suisse, et la plus difficile que nous ayons parcouru. On se souviendra que nous avons continuellement monte depuis Glaris, et que nous nous trouvons au pied de ces montagnes ou de ces pics etonnans qui dominent les hautes Alpes; on trouve ici la facilite peu commune de pouvoir examiner, et voir le pied ou les fondemens de ces colosses qui couronnent le globe, parce qu’ils sont ordinairement entoures de leurs debris et de leurs eboulemens qui en cachent le pied. Ici c’est une roche de schiste bleuatre, dure et compact, traversee de filons de quartz blanc, et quelquefois jaunatre, dans laquelle on a taille un sentier pour pouvoir en franchir le pied. Cette roche s’eleve a une hauteur prodigieuse, est presque verticale, et ces couches sont a quatre-vingt degres d’inclinaison. L’imagination est effrayee de voir que de pareilles masses ayent pu etre ebranlees et deplacees au point d’avoir fait presque un quart de conversion. Apres avoir monte et suivi cette roche parmi les pierres et les decombres, une heure et demie, on trouve cette roche de schiste surmontee d’autres rochers fort hauts qui sont calcaires, et dont les lits sont fort horizontaux. Les schistes, qui sont directement sous les roches calcaires, conservent la meme inclinaison qu’elles ont a leur pied.”

Here is an observation which certainly agrees with that given by M. de Luc, and would seem to confirm this conclusion, that strata had been deposited upon those *schisti* after they had been changed from their natural or horizontal position, and become vertical; at the same time, this conclusion is not of necessary consequence, without examining concomitant appearances, and finding particular marks by which this operation might be traced; for the simply finding horizontal strata, placed above vertical or much inclined schiste, is not sufficient, of itself, to constitute that fact, while it is acknowledged that every species of fracture, dislocation, and contortion, is to be found among the displaced strata of the globe.

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Since writing this chapter, I am enabled to speak more decisively upon that point, having acquired more light upon the subject, as will appear in the next chapter.

CHAP. VI.

The Theory of interchanging Sea and Land illustrated by an Investigation of the Primary and Secondary Strata.

SECT. I.—A distinct View of the Primary and Secondary Strata.

Having given a view of what seems to be the primary and secondary strata, from the observations of authors, and having given what was my opinion when I first wrote that chapter, I am now to treat of this subject from observations of my own, which I made since forming that opinion.

From Portpatrick, on the west coast, to St Abb's Head, on the east, there is a tract of schistus mountains, in which the strata are generally much inclined, or approaching to the vertical situation; and it is in these inclined strata that geologists allege that there is not to be found any vestige of organised body. This opinion, however, I have now proved to be erroneous.

There cannot be any doubt with regard to the original formation of those stratified bodies, as having been formed of the materials that are natural to this earth, *viz*. the detritus of former bodies; and as having been deposited in water, like the horizontal strata: For the substances and bodies of which they are visibly composed are no other than those which form the most regular horizontal strata, and which are continually traveling, or transported at the bottom of the sea, such as gravel, and sand, argillaceous and micaceous bodies.

On each side of this ridge of mountains, which towards the east end is but narrow, there is a lower country composed of strata in general more horizontal; and among which strata, besides coal, there are also found the relics of organised bodies.

Abstracting at present from any consideration of organised bodies among the materials of those strata, it may be affirmed, that the materials which form the strata in the mountains and in the low country, are similar, or of the same nature; that they have, in both places, been consolidated by the same means, *viz*. heat and fusion; and that the same or similar accidents have happened to them, such as change from their original position, and mineral veins traversing them in various shapes. Yet still there is a distinctive character for those two bodies, the alpine and the horizontal strata; for, while the horizontal position appears natural to the one, and the changes from that particular state to be only an accident, the vertical position appears to be more natural to the other, which is seldom found horizontal.

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Therefore, altho' it is unquestionable that the strata in the alpine and low countries had the same or a similar original, yet, as the vertical position, which is the greatest possible change in that respect, is more natural to the alpine strata, or only necessary in the natural order of those bodies, we are to consider this great disorder or change from the natural state of their original formation, as the proper character of those alpine strata. But then it is also necessary to include in this character a general hardness and solidity in those vertical strata, otherwise they would not have been properly alpine, or have resisted the wearing and washing powers of the globe, so as to have remained higher than the others; for, the vertical position, or great inclination of those strata, should rather have disposed them the more to dissolution and decay. Let us now see how far we shall be justified in that general conclusion, by the examination of those bodies.

The fact is certain, that those alpine bodies are much harder, or less subject to dissolution and decay, than the horizontal strata. But this must be taken in the general, and will by no means apply to particular cases which might be compared. Nothing, for example, more solid than the lime-stones, or marbles, and iron-stones; nothing more hard or solid than the chirt or flint; and all these are found among the horizontal strata. But, while some strata among those horizontal beds are thus perfectly solid, others are found with so slight degrees of consolidation, that we should not be able to ascribe it to the proper cause, without that gradation of the effect, which leads us to impute the slightest degree of consolidation to the same operations that have produced the complete solidity. While, therefore, the most perfect solidity is found in certain strata, or occasionally among the horizontal bodies, this forms no part of their character in general, or cannot be considered as a distinctive mark, as it truly is with regard to the alpine strata. These last have a general character of consolidation and indissolubility, which is in a manner universal. We are, therefore, now to inquire into the cause of this distinction, and to form some hypothesis that may be tried by the actual state of things, in being compared with natural appearances.

As the general cause of consolidation among mineral bodies, formed originally of loose materials, has been found to consist in certain degrees of fusion or cementation of those materials by means of heat; and as, in the examination of the horizontal strata we actually find very different degrees of consolidation in the several strata, independent of their positions in relation to height or depth, we have reason to believe that the heat, or consolidating operation, has not been equally employed in relation to them all.

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We are not now inquiring how an inferior stratum should have been heated in a lesser degree, or not consolidated, while a superior stratum had been consolidated in the most perfect manner; we are to reason upon a fact, which is, that the horizontal strata in general appear not to have been equally or universally consolidated; and this we must attribute to an insufficient exertion of the consolidating cause. But, so far as the erecting cause is considered as the same with that by which the elevated bodies were consolidated, and so far as the vertical situation is a proof of the great exertion of that subterraneous power, the strata which are most erected, should in general be found most consolidated.

Nothing more certain than that there have been several repeated operations of the mineralising power exerted upon the strata in particular places; and all those mineral operations tend to consolidation: Therefore, the more the operations have been repeated in any place, the more we should find the strata consolidated, or changed from their natural state. Vertical strata have every appearance from whence we should be led to conclude, that much of the mineral power had been exerted upon them, in changing their original constitution or appearance. But the question now to be considered is this, How far it may appear that these masses of matter, which now seem to be so different from the ordinary strata of the globe, had been twice subjected to the mineral operations, in having been first consolidated and erected into the place of land, and afterwards sunk below the bottom of the sea, in order a second time to undergo the process of subterraneous heat, and again be elevated into the place where they now are found.

It must be evident, here is a question that may not be easy to decide. It is not to the degree of any change to which bodies may be subject, that we are to appeal, in order to clear up the point in question, but to a regular course of operations, which must appear to have been successively transacted, and by which the different circumstances or situations of those masses are to be discovered in their present state. Now, though it does not concern the present theory that this question be decided, as it is nothing but a repetition of the same operations that we look for; nevertheless, it would be an interesting fact in the natural history of this earth; and it would add great lustre to a theory by which so great, so many operations were to be explained. I am far from being sanguine in my expectations of giving all the satisfaction in relation to this subject that I could wish; but it will be proper to state what I have lately learned with regard to so curious a question, that others, who shall have the opportunity, may be led to inquire, and that thus the natural history of the earth may be enlarged, by a proper investigation of its mineral operations.

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With this view I have often considered our schistus mountains, both in the north and south; but I never found any satisfactory appearance from whence conclusions could be formed, whether for the question or against it. The places I examined were those between the alpine countries and the horizontal strata; here, indeed, I have frequently found a confused mass, formed of the fragments of those alpine strata mixed with the materials of the horizontal bodies; but not having seen the proper shape and connection of those several deposits, I always suspended my judgment with regard to the particular operations which might have been employed in producing those appearances.

I had long looked for the immediate junction of the secondary or low country strata with the alpine schistus, without finding it; the first place in which I observed it was at the north end of the island of Arran, at the mouth of Loch Ranza; it was upon the shore, where the inclined strata appeared bare, being; washed by the sea. It was but a very small part that I could see; but what appeared was most distinct. Here the schistus and the sandstone strata both rise inclined at an angle of about 45 deg.; but these primary and secondary strata were inclined in almost opposite directions; and thus they met together like the two sides of a *lambda*, or the rigging of a house, being a little in disorder at the angle of their junction. From this situation of those two different masses of strata, it is evidently impossible that either of them could have been formed originally in that position; therefore, I could not here learn in what state the schistus strata had been in when those of the sand-stone, &c, had been superinduced.

Such was the state of my mind, in relation to that subject:, when at Jedburgh upon a visit to a friend, after I had returned from Arran, and wrote the history of that journey; I there considered myself as among the horizontal strata which had first appeared after passing the Tweed, and before arriving at the Tiviot. The strata there, as in Berwickshire, which is their continuation to the east, are remarkably horizontal for Scotland; and they consist of alternated beds of sand-stone and marl, or argillaceous and micaceous strata. These horizontal strata are traversed in places with small veins of whin-stone, as well as greater masses forming rocks and hills of that material; but, except it be these, (of which there are some curious examples), I thought there could be nothing more of an interesting nature to observe. Chance, however, discovered to me what I could not have expected or foreseen.

The river Tweed, below Melrose, discovers in its bed the vertical strata of the schistus mountains, and though here these indurated bodies are not veined with quartz as in many places of the mountains, I did not hesitate to consider them as the same species, that is to say, the marly materials indurated and consolidated in those operations by which they had been so much changed in their place and natural position. Afterwards in travelling south, and seeing the horizontal softer strata, I concluded that I had got out of the alpine country, and supposed that no more of the vertical strata were to be observed.

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The river Tiviot has made a wide valley as might have been expected, in running over thole horizontal strata of marly or decaying substances; and the banks of this river declining gradually are covered with gravel and soil, and show little of the solid strata of the country. This, however, is not the case with the Jed, which is to the southward of the Tiviot; that river, in many places, runs upon the horizontal strata, and undermines steep banks, which falling shows high and beautiful sections of the regular horizontal strata. The little rivulets also which fall into the Jed have hollowed out deep gullies in the land, and show the uniformity of the horizontal strata.

In this manner I was disposed to look for nothing more than what I had seen among those mineral bodies, when one day, walking in the beautiful valley above the town of Jedburgh, I was surprised with the appearance of vertical strata in the bed of the river, where I was certain that the banks were composed of horizontal strata. I was soon satisfied with regard to this phenomenon, and rejoiced at my good fortune in stumbling upon an object so interesting to the natural history of the earth, and which I had been long looking for in vain.

Here the vertical strata, similar to those that are in the bed of the Tweed, appear; and above those vertical strata, are placed the horizontal beds, which extend along the whole country.

The question which we would wish to have solved is this; if the vertical strata had been broken and erected under the superincumbent horizontal strata; or if, after the vertical strata had been broken and erected, the horizontal strata had been deposited upon the vertical strata, then forming the bottom of the sea. That strata, which are regular and horizontal in one place, should be found bended, broken, or disordered at another, is not uncommon; it is always found more or less in all our horizontal strata. Now, to what length this disordering operation might have been carried, among strata under others, without disturbing the order and continuity of those above, may perhaps be difficult to determine; but here, in this present case, is the greatest disturbance of the under strata, and a very great regularity among those above. Here at least is the most difficult case of this kind to conceive, if we are to suppose that the upper strata had been deposited before those below had been broken and erected.

Let us now suppose that the under strata had been disordered at the bottom of the sea, before the superincumbent bodies were deposited; it is not to be well conceived, that the vertical strata should in that case appear to be cut off abruptly, and present their regular edges immediately under the uniformly deposited substances above. But, in the case now under consideration, there appears the most uniform section of the vertical strata, their ends go up regularly to the horizontal deposited bodies. Now, in whatever state the vertical

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strata had been in at the time of this event, we can hardly suppose that they could have been so perfectly cut off, without any relict being left to trace that operation. It is much more probable to suppose, that the sea had washed away the relics of the broken and disordered strata, before those that are now superincumbent had been begun to be deposited. But we cannot suppose two such contrary operations in the same place, as that of carrying away the relics of those broken strata, and the depositing of sand and subtile earth in such a regular order. We are therefore led to conclude, that the bottom of the sea, or surface of those erected strata, had been in very different situations at those two periods, when the relics of the disordered strata had been carried away, and when the new materials had been deposited.

If this shall be admitted as a just view of the subject, it will be fair to suppose, that the disordered strata had been raised more or less above the surface of the ocean; that, by the effects of either rivers, winds, or tides, the surface of the vertical strata had been washed bare; and that this surface had been afterwards sunk below the influence of those destructive operations, and thus placed in a situation proper for the opposite effect, the accumulation of matter prepared and put in motion by the destroying causes.

I will not pretend to say that this has all the evidence that should be required, in order to constitute a physical truth, or principle from whence we were to reason farther in our theory; but, as a simple fact, there is more probability for the thing having happened in that manner than in any other; and perhaps this is all that may be attained, though not all that were to be wished on the occasion. Let us now see how far any confirmation may be obtained from the examination of all the attending circumstances in those operations.

I have already mentioned, that I had long observed great masses of *debris*, or an extremely coarse species of pudding-stone, situated on the south as well as north sides of those schistus mountains, where the alpine strata terminate in our view, and where I had been looking for the connection of those with the softer strata of the low country. It has surely been such appearances as these which have often led naturalists to see the formation of secondary and tertiary strata formed by the simple congestion of *debris* from the mountains, and to suppose those masses consolidated by the operation of that very element by which they had been torn off from one place and deposited in another. I never before had data from whence to reason with regard to the natural history of those masses of gravel and sand which always appeared to me in an irregular shape, and not attended with such circumstances as might give light into their natural history; but now I have found what I think sufficient to explain those obscure appearances, and which at the same time will in some respect illustrate or confirm the conjecture which has now been formed with regard to the operations of the globe in those regions.

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In describing the vertical and horizontal strata of the Jed, no mention has been made of a certain pudding-stone, which is interposed between the two, lying immediately upon the one and under the other. This puddingstone corresponds entirely to that which I had found along the skirt of the schistus mountains upon the south side, in different places, almost from one end to the other. It is a confused mass of stones, gravel, and sand, with red marly earth; these are consolidated or cemented in a considerable degree, and thus form a stratum extremely unlike any thing which is to be found either above or below.

When we examine the stones and gravel of which it is composed, these appear to have belonged to the vertical strata or schistus mountains. They are in general the hard and solid parts of those indurated strata, worn and rounded by attrition; particularly sand or marl-stone consolidated and veined with quartz, and many fragments of quartz, all rounded by attrition. In this pudding-stone of the Jed, I find also rounded lumps of porphyry, but have not perceived any of granite.[32] This however is not the case in the pudding-stone of the schistus mountains, for, where there is granite in the neighbourhood, there is also granite in the pudding-stone.

[Note 32: A view of this object is seen in plate 3d. It is from a drawing taken by Mr Clerk of Eldin.]

From this it will appear, that the schistus mountains or the vertical strata of indurated bodies had been formed, and had been wasted and worn in the natural operations of the globe, before the horizontal strata were begun to be deposited in those places; the gravel formed of those indurated broken bodies worn round by attrition evince that fact. But it also appears that the mineral operations of the globe, melting and consolidating bodies, had been exerted upon those deposited strata above the vertical bodies.

This appears evidently from the examination of our pudding-stone. The vertical strata under it are much broken and injected with ferruginous spar; and this same spar has greatly penetrated the pudding-stone above, in which are found the various mineral appearances of that spar and iron ore.

But those injecting operations reach no farther up among the marl strata in this place; and then would appear to have been confined to the pudding-stone. But in another place, about half a mile farther up the river, where a very deep section of the strata is discovered, there are two injections from below; the one is a thin vein of whin-stone or basaltes, full of round particles of steatites impregnated with copper; it is but a few inches wide, and proceeds in a kind of zigzag. The other appears to have been calcareous spar, but the greatest part of it is now dissolved out. The strata here descend to the bottom of the river, which is above the place of the pudding-stone and vertical strata. Neither are these last discoverable below the town of Jedburgh, at least so far as I have seen; and the line of division, or plane of junction of the vertical and horizontal strata, appears to decline more than the bed of the river.

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But it may be asked, how the horizontal strata above, among which are many very strong beds, have been consolidated. The answer to this question is plain. Those strata have been indurated or consolidated in no other manner than the general strata of the earth; these being actually the common strata of the globe; while the vertical or schistus strata are the ordinary strata still farther manufactured, (if we may be allowed the expression) in the vicissitude of things, and by the mineral operations of the globe. That those operations have been performed by subterraneous heat has been already proved; but I would now mention some particular appearances which are common or general to those strata, and which can only be explained upon that principle.

The red marly earth is prevalent among those strata; and it is with this red ferruginous substance that many of the sand-stone strata are tinged. It is plain that there had been an uniform, deposits of that sand and tinging earth; and that, however different matter might be successively deposited, yet that each individual stratum should be nearly of the same colour or appearance, so far as it had been formed uniformly of the same subsiding matter. But, in the most uniform strata of red sand-stone, the fracture of the stone presents us with circular spots of a white or bluish colour; those little spheres are in all respects the same with the rest of the stone, they only want the tinging matter; and now it may be inquired how this has come about.

To say that sphericles of white sand should have been formed by subsiding along with the red sand and earth which composed the uniform stratum whether of sand-stone or marl, (for it happens equally in both,) is plainly impossible, according to our notion of that operation in which there is nothing mysterious. Those foliated strata, which are of the most uniform nature, must have been gradually accumulated from the subsiding sand and earth; and the white or colourless places must have had their colour destroyed in the subsequent cementing operations. It is often apparent, that the discharging operation had proceeded from a centre, as some small matter may be perceived in that place. I know not what species of substance this has been, whether saline or phlogistic, but it must have had the power of either volatilising or changing the ferruginous or red tinging substance so as to make it lose its colour.

I have only mentioned spherical spots for distinctness sake; but this discharging operation is found diversifying those strata in various ways, but always referable to the same or similar causes. Thus, in many of the veins or natural cracks of those strata, we find the colour discharged for a certain space within the strata; and we often see several of those spots united, each of them having proceeded from its own centre, and uniting where they approached. In the two veins above mentioned, of whin-stone and spar traversing the strata, the colour of the strata is, discharged more or less in the places contiguous with the veins.

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I am now to mention another appearance of a different kind. Those strata of marl are in general not much consolidated; but among, them there are sometimes found thin calcareous strata extremely consolidated, consequently much divided by veins. It is in the solid parts of those strata, perfectly disconnected from the veins, that there are frequent cavities curiously lined with crystals of different sorts, generally calcareous, sometimes containing also those that are siliceous, and often accompanied with pyrites. I am persuaded that the origin of those cavities may have been some hollow shells, such as *echini* or some marine object; but that calcareous body has been so changed, that it is not now distinguishable; therefore, at present, I hold this opinion only as conjecture.

Having, in my return to Edinburgh, traveled up the Tiviot, with a view to investigate this subject of primary and secondary operations of the earth, I found the vertical strata, or alpine schistus, in the bed of the river about two miles below Hawick. This was the third time I had seen those vertical bodies after leaving the mountains of Lauderdale. The first place was the bed of the river Tweed, at the new bridge below Melrose; but here no other covering is to be seen above those vertical strata besides the soil or traveled earth which conceals every thing except the rock in the bed of the river. The second place was Jedburgh, where I found the vertical strata covered with the horizontal sandstone and marl, as has been now described. The third place was the Tiviot, and this is that which now remains to be considered.

Seeing the vertical strata in the bed of the river, I was desirous to know if those were immediately covered with the horizontal strata. This could not be discovered in the bed of the river where the rock was covered upon the banks with travelled earth. I therefore left the river, and followed the course of a brook which comes from the south side. I had not gone far up the bank, or former boundary of the Tiviot, when I had the satisfaction to find the vertical strata covered with the pudding-stone and marly beds as in the valley of the Jed.

It will now be reasonable to suppose that all the schistus which we perceive, whether in the mountains or in the valleys, exposed to our view had been once covered with those horizontal strata which are observed in Berwickshire and Tiviotdale; and that, below all those horizontal strata in the level country, there is at present a body or basis of vertical or inclined schistus, on which the horizontal strata of a secondary order had been deposited. This is the conclusion that I had formed at Jedburgh, before I had seen the confirmation of it in the Tiviot; it is the only one that can be formed according to this view of things; and it must remain in the present state until more evidence be found by which the probability may be either increased or diminished.

Since writing this, I have read, in the Esprit de Journaux, an abstract of a memoir of M. Voigt, upon the same subject, which I shall now transcribe.

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“La mer a commence par miner les montagnes primitives dont les debris se sont precipites au fond. Ces debris forment la premiere couche qui est posee immediatement sur les montagnes primitives. D’apres l’ancien langage de mineurs, nous avons jusqu’aujourd’hui appelle cette couche *le sol mort rouge*, parce qu’il y a beaucoup de rouge dans son melange, qu’elle forme le sol ou la base d’autres couches, et peut-etre de toutes, qu’elle est entierement inutile et, en quelque facon, morte pour l’exploitation des mines. Plusieurs se sont efforces de lui donner un nom harmonieux; mais ils ne l’ont pu sans occasionner des equivoques. Les mots *Breche Puddinstone Conglomerations*, &_c_. designent toujours des substances autres que cette espece de pierre.

“Il est tres agreable de l’examiner dans les endroits ou elle forme des montagnes entieres. Cette couche est composee d’une quantite prodigieuse de pierres arrondies, agglutinees ensemble par une substance argileuse rouge et meme grise, et le toute a acquis assez de durete. On ne trouve dans sa composition aucune espece de pierre qui, a en juger par les meilleures observations, puisse avoir ete formee plus tard qu’elle; on n’y voit par-tout que des parties et des produit des montagnes primitives principalement de celles qui abondent le plus dans ces contrees. Le sol mort, par exemple, qui compose les montagnes des environs de Walbourg, pres d’Eisenach, contient une quantite de gros morceaux de granit et de schiste micace; c’est vraisemblablement parce que les montagnes primitives les plus voisines de Rhula, *etc*. sont, pour la plus part, formees de ces deux especes de pierres. Pres de Goldlauter, le sol mort consiste presque tout en porphyre, substance dont sont formees les montagnes primitives qui y dominent; et le Kiffauserberg dans la Thuringe a probablement recu ces morceaux arrondis de schiste argileux des montagnes voisine du Hartz. Vous trouverez ici que le schiste argileux existoit deja lorsque la mer a jette les premiers fondemens de nos montagnes stratifiees. Je serois fort etonne que quelqu’un me montrat un sol mort qui contint un morceaux de gypse, de marne, de pierre puante et autres. Quoiqu’il en soit il n’est pas aise d’expliquer pourquoi on ne trouve point de corps marins petrifies dans cette espece de pierre. C’est peut-etre que, par l’immense quantite de pierres dures roulees dans le fond de la mer, ils ont ete brises avant qu’ils aient commence de s’agglutiner ensemble. Mais on rencontre sur-tout au Kiffhauserberg des troncs d’arbres entiers petrifies; preuve qu’il y avoit deja ou de la vegetation avant que l’ocean destructeur se fut empare de ces cantons, ou du moins que quelques isles avoient existe au-dessus de la surface.”

Here we find the same observations in the mountains of Germany that I have been making with regard to those of Scotland. I have formerly observed masses of the same kind in the west of England, to the east of the Severn; but I could not discover any proper connection of that mass with the regular strata. I have also long observed it in many parts of Scotland, without being able to attain a sufficiently satisfactory idea with regard to those particulars by which the alternation of land and water, of the superficial and internal mineral operations of the globe, might be investigated.

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It will be very remarkable if similar appearances are always found upon the junction of the alpine with the level countries. Such an appearance, I am inclined to think, may be found in the Val d'Aoste, near Yvree. M. de Saussure describes such a stone as having been employed in building the triumphal arch erected in honour of Augustus. "Cet arc qui etoit anciennement revetu de marbre, est construit de grands quartiers d'une espece assez singuliere de poudingue ou de gres a gros grains. C'est une assemblage de fragmens, presque touts angulaires, de toutes sortes de roches primitives feuilletees, quartzeuses, micacees; les plus gros de ces fragmens n'atteignent pas le volume, d'une noisette. La plupart des edifices antiques de la cite l'Aoste et de ses environs, sont construits de cette matiere; et les gens du pays sont persuades que c'est une composition; mais j'en ai trouve des rochers en place dans les montagnes au nord et au-dessus de la route d'Yvree."

We may now come to this general conclusion, that, in this example of horizontal and posterior strata placed upon the vertical *schisti* which are prior in relation to the former, we obtain a further view into the natural history of this earth, more than what appears in the simple succession of one stratum above another. We know, in general, that all the solid parts of this earth, which come to our view, have either been formed originally by subsidence at the bottom of the sea, or been transfused in a melted state from the mineral regions among those solid bodies; but here we further learn, that the indurated and erected strata, after being broken and washed by the moving waters, had again been sunk below the sea, and had served as a bottom or basis on which to form a new structure of strata; and also, that those new or posterior strata had been indurated or cemented by the consolidating operations of the mineral region, and elevated from the bottom of the sea into the place of land, or considerably above the general surface of the waters. It is thus that we may investigate particular operations in the general progress of nature, which has for object to renovate the surface of the earth necessarily wasted in the operation of a world sustaining plants and animals.

It is necessary to compare together every thing of this kind which occurs; it is first necessary to ascertain the fact of their being a prior and posterior formation of strata, with the mineral operations for consolidating those bodies formed by collection of the moveable materials; and, secondly, it is interesting to acquire all the data we can in order to form a distinct judgment of that progress of nature in which the solid body of our land is alternately removed from the bottom of the sea into the atmosphere, and sunk again at the bottom of the sea.

I shall now transcribe what M. Schreiber has wrote in relation to this subject. It is in a memoir concerning the gold mine of Gardette, published in the Journal de Physique.

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“Avant de quitter la montagne de la Gardette qu’il me soit permis de rapporter une observation qui peut-etre n’est pas denuee de tout interet pour les naturalistes; je l’ai faite dans une galerie a environ cinquante-trois toises a l’ouest du principal puit laquelle a ete poussee sur la ligne de reunion de la pierre calcaire, et du granit feuillete ou gneiss pour fonder le filon dans cet endroit. Ce filon a six pouces d’epaisseur, et consiste en quartz entre-mele d’ochre martiale, de pyrite cuivreuse et galene. Cette derniere est souvent recouverte de chaux de plomb grise, et de petits cristaux de mine de plomb jaune donnant dans l’analyse un indice d’or. Ce filon finit a la reunion de la pierre calcaire au gneiss. Cette reunion se fait ici dans la direction d’une heure 6/8 de la boussole de raineur, et sous un inclinaison, occidentale de 26 degres.

“Mais ce qu’il y a de remarquable, c’est que le gneis ne participe en rien de la pierre calcaire quoiqu’il n’en soit separe que par une couche d’une pouce d’epaisseur de terre argileuse et calcaire, tandis que le rocher calcaire renferme beaucoup de fragmens de granit et de gneis, dans le voisinage de cette reunion.

“Cette observation prouve incontestablement que le granit et le gneis avoient deja acquis une durete capable de resister aux infiltration des parties calcaire, et qu’ils existoient a-peu-pres tels qu’ils sont aujourd’hui lorsque la pierre calcaire commenca a se former; autrement elle n’auroit pu saisir et envelopper des morceaux detaches de ces rochers auxquels on donne avec raison l’epithete de primitif ou de premiere formation.”

M. Schreiber continues his reasoning upon those mineral appearances, in adducing another argument, which I do not think equally conclusive. He says, “Le filon de la Gardette devoit pareillement exister avant la montagne calcaire, car s’il s’etoit forme apres, je ne voit pas la raison pour laquelle il s’y seroit arrete court, et pourquoi il ne se seroit pas prolonge dans cette espece de rocher.” It is not necessary, in the formation of a vein, that it should proceed in traversing all the strata which then are superincumbent; it is reasonable to suppose, and consistent with observation to find them stop short in proceeding from one stratum to another. Had M. Schreiber found any pieces of the vein contained in the calcareous rock, he would have had good reason for that assertion; but, to conclude that fact from grounds which do not necessarily imply it, is not to be permitted in sound reasoning, if certainty is the object, and not mere probability.

SECT. II.—The Theory confirmed from Observations made on purpose to elucidate the subject.

Having got a distinct view of the primary and secondary mineral bodies or strata of the globe, and having thus acquired a particular object to inquire after, with a view to investigate or illustrate this piece of natural history, I was considering where we might most probably succeed in finding the junction of the low country strata and alpine schistus. I inquired of Mr Hall of Whitehall, who had frequent opportunities of traversing

those mountains which lie between his house in the Merse and Edinburgh; and I particularly entreated him to examine the bed of the Whittater, which he executed to my satisfaction.

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Mr Hall having had occasion to examine the Pease and Tour burns, in planning and superintending the great improvement of the post road upon Sir James Hall's estate while Sir James was abroad, he informed me that the junction of the schistus and sand-stone strata was to be found in the Tour burn. Professor Playfair and I had been intending a visit to Sir James Hall at Dunglass; and this was a motive, not so much to hasten our visit, as to chose the most proper time for a mineral expedition both upon the hills and along the sea shore.

It was late in the spring 1788 when Sir James left town, and Mr Playfair and I went to Dunglass about the beginning of June. We had exceeding favourable weather during the most part of our expedition; and I now propose to give an account of the result of our observations.

Dunglass burn is the boundary between the counties of East Lothian and Berwickshire; and it is almost the boundary between the vertical and horizontal strata. To the north-west of this burn and beautiful dean are situated the coal, lime-stone, marl, and sand-stone strata; they are found stretching away along the shore in a very horizontal direction for some time, but become more and more inclined as they approach the schistus of which the hills of Lammermuir to the south are composed.

Though the boundary between the two things here in question be easily perceivable from the nature of the country at the first inspection, by the rising of the hills, yet this does not lead one precisely to the junction; and in the extensive common boundary of those two things, the junction itself is only to be perceived in few places, where the rock is washed bare by the rivers or the sea, and where this junction is exposed naked to our view. The sea is here wearing away the coast; and the bank, about 200 feet high, is gradually falling down, making in some places a steep declivity, in others a perpendicular cliff. St Abb's Head and Fast Castle are head lands projecting into the sea, and are the bulwarks of this shore, which is embayed to the westward, where the sea preys upon the horizontal strata. The solid strata are every where exposed either in the cliff or on the shore; we were therefore certain of meeting with the junction in going from Dunglass to Fast Castle, which is upon the schistus. But this journey can only be made by sea; and we first set out to examine the junction in the Tour and Pease burns, where we had been informed it was to be found.

In the bottom of those rivulets the sand-stone and marly strata appear pretty much inclined, rising towards the schistus country. The two burns unite before they come to the shore; and it is about midway between this junction and the bridges which are thrown over those two hollows, that the junction is to be found.

The schistus strata here approach towards vertical; and the sand-stone strata are greatly inclined. But this inclination of those two different strata are in opposite directions; neither does the horizontal section of those two different strata run parallel to

the junction; that is to say, the intersection of those two different strata is a line inclined to the horizon.

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At Jedburgh the schistus was vertical, and the strata horizontal; and there was interposed a compound bed of pudding-stone, formed of various water-worn bodies, the gravel of the schistus strata, and porphyries. Here again, though we have not a regular pudding-stone, we have that which corresponds to it, as having been the effect of similar circumstances. These are the fracture and detritus of the schistus, while the strata were deposited upon the broken ends of the schistus at the bottom of the sea. Most of the fragments of the schistus have their angles sharp; consequently, they had not travelled far, or been much worn by attrition. But more or less does not alter the nature of an operation; and the pudding-stone, which at Jedburgh is interposed between the vertical schistus and horizontal strata, is here properly represented by the included fragments of schistus in the inclined strata.

The line of this junction running, on the one hand, towards Fast Castle eastward, and, on the other, towards the head of Dunglass burn westward, our business was to pursue this object in those two different directions. But it was chiefly in the sea coast that was placed our expectations, having recollection of the great banks of gravel under which the strata are buried about Oldhamstocks, near which, from all appearances, the junction was to be expected.

Having taken boat at Dunglass burn, we set out to explore the coast; and, we observed the horizontal sand-stone turn up near the Pease burn, lifting towards the schistus. We found the junction of that schistus with the red sand-stone and marly strata on the shore and sea bank, at St. Helens, corresponding in general with what we had observed in the burns to the westward. But, at Siccar Point, we found a beautiful picture of this junction washed bare by the sea. The sand-stone strata are partly washed away, and partly remaining upon the ends of the vertical schistus; and, in many places, points of the schistus strata are seen standing up through among the sand-stone, the greatest part of which is worn away. Behind this again we have a natural section of those sand-stone strata, containing fragments of the schistus.

After this nothing appears but the schistus rocks, until sand-stone and marl again are found at Red-heugh above the vertical strata. From that bay to Fast Castle we had nothing to observe but the schistus, which is continued without interruption to St Abb’s Head. Beyond this, indeed, there appears to be something above the schistus; and great blocks of a red whin-stone or basaltes come down from the height and lie upon the shore; but we could not perceive distinctly how the upper mass is connected with the vertical schistus which is continued below.

Our attention was now directed to what we could observe with respect to the schisti, of which we had most beautiful views and most perfect sections. Here are two objects to be held in view, in making those observations; the original formation or stratification of the schisti, and the posterior operations by which the present state of things has been procured. We had remarkable examples for the illustration of both those subjects.

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With regard to the first, we have every where among the rocks many surfaces of the erected strata laid bare, in being separated. Here we found the most distinct marks of strata of sand modified by moving water. It is no other than that which we every day observe upon the sands of our own shore, when the sea has ebbed and left them in a waved figure, which cannot be mistaken. Such figures as these are extremely common in our sand-stone strata; but this is an object which I never had distinctly observed in the alpine schisti; although, considering that the original of those schisti was strata of sand, and formed in water, there was no reason to doubt of such a thing being found. But here the examples are so many and so distinct, that it could not fail to give us great satisfaction.

We were no less gratified in our views with respect to the other object, the mineral operations by which soft strata, regularly formed in horizontal planes at the bottom of the sea, had been hardened and displaced. Fig. 4. represents one of those examples; it was drawn by Sir James Hall from a perfect section in the perpendicular cliff at Lumesden burn. Here is not only a fine example of the bendings of the strata, but also of a horizontal shift or hitch of those erected strata.

St Abb's Head is a promontory which, at a distance, one would naturally conclude to be composed of the schisti, as is all the shore to that place; but, as we approached it, there was some difference to be perceived in the external appearance, it having a more rounded and irregular aspect. Accordingly, upon our arrival, we found this head-land composed of a different substance. It is a great mass of red whin-stone, of a very irregular structure and composition. Some of it is full of small pebbles of calcareous spar, surrounded with a coat of a coloured substance, different both from the whin-stone ground and the inclosed pebble. Here ended our expedition by water.

Having thus found the junction of the sand-stone with the schistus or alpine strata to run in a line directed from Fast Castle to Oldhamstocks, or the heads of Dunglass burn, we set out to trace this burn, not only with a view to observe the junction, if it should there appear, but particularly to discover the source of many blocks of whin-stone, of all sizes, with which the bed of this burn abounds.

The sand-stone and coal strata, which are nearly horizontal at the mouth of this burn, or on the coast, become inclined as we go up the course of the rivulet; and of this we have fine sections in the bank. The Dean of Dunglass is formed of precipitous and perpendicular rocks, through which the running water has worn its way more than a hundred feet deep; above this Dean the banks are steep and very high, but covered with soil, which here is a deep gravel. The burn runs all the way up to Oldhamstocks upon the sand-stone strata; but there, these are traversed by a high whin-stone dyke, which crosses the burn obliquely, as we found it on both banks though not in the bed of the burn; it is in the south bank below the village, and on the north above it. Here is the source of the whin-stone which we were looking for; it is the common blue basaltes, of the same nature with the Giant's Causeway, but with no regular columner appearance.

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Above Oldhamstocks we again found the sand-stone in the bank, but it soon disappeared under a deep cover of gravel, and the burn then divided into several rivulets which come from the hills. We traced the one which led most directly up to the mountains, in expectation of meeting with the schistus, at least, if not the junction of it with the sandstone. But in this we were disappointed. We did not however lose our labour; for, though the junction which we pursued be not here visible, we met with what made it sufficiently evident, and was at the same time an object far more interesting in our eyes.

I have already quoted Mr Voigt's description of the *sol mort rouge*; he says, that in places it forms entire mountains; here we have a perfect example of the same thing; and the moment we saw it, we said, here is the *sol mort rouge*. We ascended to the top of the mountain through a gully of solid pudding-stone going into decay, and furnishing the country below with that great covering of gravel, soil, and water worn stones. We were now well acquainted with the pudding-stone, which is interposed between the horizontal and alpine strata; but from what we had seen to the eastward, we never should have dreamed of meeting with what we now perceived. What we had hitherto seen of this pudding-stone was but a few fragments of the schistus in the lower beds of sand-stone; here a mountain of water-worn schisti, imbedded in a red earth and consolidated, presented itself to our view. It was evident that the schisti mountains, from whence those fragments had come, had been prior to this secondary mass; but here is a secondary mountain equal in height to the primary, or schisti mountains, at the basis of which we had seen the strata superinduced on the shore. Still, however, every thing here is formed upon the same principle, and nothing here is altered except the scale on which the operation had been performed.

Upon the coast, we have but a specimen of the pudding-stone; most of the fragments had their angles entire; and few of them are rounded by attrition. Here, on the contrary, the mountain is one pudding-stone; and most of the fragments are stones much rounded by attrition. But the difference is only in degree, and not in kind; the stones are the same, and the nature of the composition similar. Had we seen the mass of which this mountain is only a relict, (having been degraded by the hands of time), we should have found this pudding-stone at the bottom of our sand-stone strata; could we have penetrated below this mass of pudding-stone, we should have found our schistus which we left on the shore at St. Helens and in the Tour burn. In Tiviotdale the vertical schisti are covered with a bed of pudding-stone, the gravel of which had been much worn by attrition, but the thickness of that bed is small; here again the wearing operation has been great, and the quantity of those materials even more than in proportion to those operations. We returned perfectly satisfied; and Sir James Hall is to pursue this subject farther when he shall be in those mountains shooting muir game.

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We had now only one object more to pursue; this was to examine the south side of those mountains of Lammermuir upon the sea shore, in order to see the junction of the primary schistus with the coal strata of Berwickshire. Mr Hall was to meet us at the Press, and we were afterwards to go with him to Whitehall. We met accordingly; but the weather was rainy; and we went directly to Whitehall. I had often seen the pudding-stone in great masse; in the banks of the Whiteader, as it comes out of the mountains, but then I had not seen its connection neither, on the one hand, with the schisti, nor, on the other, with the sand-stone strata. We knew that at Lammerton upon the sea coast there was coal, and consequently the sand-stone strata; and reasoning upon those data we were sure that our proper course of investigation was to trace the river Ey to the shore, and then go south the coast in search of the junction of the schistus with the horizontal strata. This we executed as well as the weather would permit; but had it to regret, that the rainy season was not so favourable for our views, as it was agreeable to the country which had been suffering with the drought.

It is needless now to enlarge upon this subject. I shall only mention that we found the red marly strata above the pudding-stone in the bed of the Ey and its branches; we then traced the schistus down the Ey, and found a mass of the most consolidated pudding-stone upon the coast to the north of the harbour of Eymouth. But this mass did not rest on the schistus; it is immediately upon a mass of whin-stone; and the schistus is in the harbour, so that this whin-stone mass seems to be here interposed between the pudding-stone and schistus. We then pursued the coast southwards until we found the junction of the schistus and sand-stone strata about two miles from Eymouth; but here the junction was not attended with any pudding-stone that we could perceive.

Having found the same or similar appearances from the one end to the other, and on both sides of that range of mountains which run from sea to sea in the south of Scotland, we may now extend our view of this mineral operation in comprehending every thing of the same kind which we meet with in our island or any other distant country.

Thus perhaps the pudding-stone of the south of England will be considered in the same light as having been formed of the *debri* and *detritus* of the flinty bodies.

In the island of Arran, there is also a pudding-stone, even in some of the summits of the island, exactly upon the border of the schistus district, as will be described in the natural history of that island. This pudding-stone is composed of gravel formed of the hardest parts of the schistus and granite or porphyry mountains. That compound parasitical stone has been also again cemented by heat and fusion; I have a specimen in which there is a clear demonstration of that fact. One of the water-worn stones which

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had been rounded by attrition, has in this pudding-stone been broken and shifted, the one half slipping over the other, three quarters of an inch, besides other smaller slips in the same stone. But the two pieces are again cemented; or they had been shifted when the stone was in that soft state, by which the two pieces are made perfectly to cohere. Those shifts and veins, in this species of stone, are extremely instructive, illustrating the mineral operations of the globe.

In like manner to the north of the Grampians, along the south side of Loch Ness, there are mountains formed of the debris of schistus and granite mountains, first manufactured into sand and gravel, and then consolidated into a pudding-stone, which is always formed upon the same principle. The same is also found upon the south side of those mountains in the shire of Angus.

I may also give for example the African *Brechia*, which is a pudding-stone of the same nature. This stone is composed of granites or porphyries, serpentines and schisti, extremely indurated and perfectly consolidated. It is also demonstrable from the appearance in this stone that it has been in a softened state, from the shape and application of its constituent parts; and in a specimen of it which I have in my cabinet, there is also a demonstration of calcareous spar flowing among the gravel of the consolidated rock.

This fact therefore of pudding-stone mountains, is a general fact, so far as it is founded upon observations that are made in Africa, Germany, and Britain. We may now reason upon this general fact, in order to see how far it countenances the idea of primitive mountains, on the one hand, or on the other supports the present theory, which admits of nothing primitive in the visible or examinable parts of the earth.

To a person who examines accurately the composition of our mountains, which occupy the south of Scotland, no argument needs be used to persuade him that the bodies in question are not primitive; the thing is evident from inspection, as much as would be the ruins of an ancient city, although there were no record of its history. The visible materials, which compose for the most part the strata of our south alpine schisti, are so distinctly the *debris* and *detritus* of a former earth, and so similar in their nature with those which for the most part compose the strata on all hands acknowledged as secondary, that there can remain no question upon that head. The consolidation, again, of those strata, and the erection of them from their original position, and from the place in which they had been formed, is another question.

But the acknowledging strata, which had been formed in the sea of loose materials, to be consolidated and raised into the place of land, is plainly giving up the idea of primitive mountains. The only question, therefore, which remains to be solved, must

respect the order of things, in comparing the alpine schisti with the secondary strata; and this indeed forms a curious subject of investigation.

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It is plain that the schisti had been indurated, elevated, broken, and worn by attrition in water, before the secondary strata, which form the most fertile parts of our earth, had existed. It is also certain that the tops of our schistus mountains had been in the bottom of the sea at the time when our secondary strata had begun to be formed; for the pudding-stone on the top of our Lammermuir mountains, as well as the secondary strata upon the vertical schisti of the Alps and German mountains, affords the most irrefragable evidence of that fact.

It is further to be affirmed, that this whole mass of water-formed materials, as well as the basis on which it rested, had been subjected to the mineral operations of the globe, operations by which the loose and incoherent materials are consolidated, and that which was the bottom of the sea made to occupy the station of land, and serve the purpose for which it is destined in the world. This also will appear evident, when it is considered that it has been from the appearances in this very land, independent of those of the alpine schisti, that the present theory has been established.

By thus admitting a primary and secondary in the formation of our land, the present theory will be confirmed in all its parts. For, nothing but those vicissitudes, in which the old is worn and destroyed, and new land formed to supply its place, can explain that order which is to be perceived in all the works of nature; or give us any satisfactory idea with regard to that apparent disorder and confusion, which would disgrace an agent possessed of wisdom and working with design.

CHAP. VII.

Opinions examined with regard to Petrifaction, or Mineral Concretion.

The ideas of naturalists with regard to petrifaction are so vague and indistinct, that no proper answer can be given to them. They in general suppose water to be the solvent of bodies, and the vehicle of petrifying substances; but they neither say whether water be an universal menstruum, nor do they show in what manner a solid body has been formed in the bowels of the earth, from that solution. It may now be proper to examine this subject, not with a view to explain all those petrifactions of bodies which is performed in the mineral regions of the earth, those regions that are inaccessible to man, but to show that what has been wrote by naturalists, upon this subject, has only a tendency to corrupt science, by admitting the grossest supposition in place of just principle or truth, and to darken natural history by introducing an ill conceived theory in place of matter of fact.

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M. le Comte de Buffon has attempted to explain the crystallization of bodies, or production of mineral forms, by the accretion or juxtaposition of elementary bodies, which have only form in two dimensions, length and breadth; that is to say, that mineral concretions are composed of surfaces alone, and not of bodies. This however is only an attempt to explain, what we do not understand, by a proposition which is either evidently contradictory, or plainly inconceivable. It is true that this eloquent and ingenious author endeavours to correct the palpable absurdity of the proposition, by representing the constituent parts of the mineral bodies as "*de lames infiniment minces*;" but who is it does not see, that these infinitely thin plates are no other than bodies of three dimensions, contrary to the supposition; for, infinitely thin, means a certain thickness; but the smallest possible or assignable thickness differs as much from a perfect superficies as the greatest.

M. de Luc has given us his ideas of petrifaction with sufficient precision of term and clearness of expression; his opinion, therefore, deserves to be examined; and, as his theory of petrifaction is equally applicable to every species of substance, it is necessary again to examine this subject, notwithstanding of what has been already said, in the first part of this work, concerning consolidation and mineral concretion from the fluid state of fusion.

This author has perhaps properly exposed Woodward's Theory of Petrification in saying[33], "Son erreur a cet egard vient de ce qu'il n'a point reflechi sur la maniere dont se fait la *petrifaction*. Il ramollit d'abord les *pierres* pour y faire entrer les coquilles, sans bien connoitre l'agent qu'il y employe; et il les duroit ensuite, sans reflechir au comment." To avoid this error or defect, M. de Luc, in his Theory of Petrifaction, sets out with the acknowledged principle of cohesion; and, in order to consolidate strata of a porous texture, he supposes water carrying minute bodies of all shapes and sizes, and depositing them in such close contact as to produce solidity and concretion. Now, if Dr Woodward softened stones without a proper cause, M. de Luc, in employing the specious principle of cohesion, has consolidated them upon no better grounds; for, the application of this principle is as foreign to his purpose, as is that of magnetism. Bodies, it is true, cohere when their surfaces are closely applied to each other; But how apply this principle to consolidation?—only by supposing all the separate bodies, of which the solid is to be composed, to be in perfect contact in all their surfaces. But this, in other words, is supposing the body to be solid; and, to suppose the agent, water, capable of thus making hard bodies solid, is no other than having recourse to the fortuitous concourse of atoms to make a world; a thought which this author would surely hold in great contempt.

[Note 33: Lettres Physiques et Morales.]

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He then illustrates this operation of nature by those of art, in building walls which certainly become hard, and which, as our author seems to think, become solid. But this is only an imperfect or erroneous representation of this subject; for, mortar does not become hard upon the principle of petrification adopted by our author. Mortar, made of clay, instead of lime, will not acquire a stony hardness, nor ever, by means of water, will it be more indurated than by simply drying; neither will the most subtile powder of chalk, with water and sand, form any solid body, or a proper mortar. The induration of mortar arises from the solution of a stony substance, and the subsequent concretion of that dissolved matter, operations purely chemical. Now, if this philosopher, in his Theory of Petrifaction, means only to explain a chemical operation upon mechanical principles, why have recourse, for an example in this subject, to mineral bodies, the origin of which is questioned? Why does he not rather explain, upon this principle, the known concretion of some body, from a fluid state, or, conversely, the known solution of some concreted body? If again he means to explain petrifaction in the usual way, by a chemical operation, in that case, the application of his polished surfaces, so as to cohere, cannot take place until the dissolved body be separated from the fluid, by means of which it is transported from place to place in the mineral regions. But it is in this preliminary step that lies all the difficulty; for, could we see how every different substance might be dissolved, and every dissolved substance separated from its solvent at our pleasure, we should find no difficulty in admitting the cohesion of hard bodies, whether by means of this doctrine of polished surfaces, or by the principle of general attraction, a principle which surely comprehends this particular, termed a cohesive power.

It must not be alleged, that seeing we know not how water dissolves saline bodies, therefore, this fluid, for any thing that we know, may also dissolve crystal; and, if water thus dissolves a mineral substance in a manner unknown to us, it may in like manner deposit it, although we may not be able to imagine how. This kind of reasoning is only calculated to keep us in ignorance; at the same time, the reasoning of philosophers, concerning petrifaction, does not in general appear to be founded on any principle that is more sound. That water dissolves salt is a fact. That water dissolves crystal is not a fact; therefore, those two propositions, with regard to the power of water, are infinitely removed, and cannot be assimilated in sound physical reasoning. It is no more a truth that water is able to dissolve salt, than that we never have been able to detect the smallest disposition in water to dissolve crystal, flint, quartz, or metals. Therefore, to allege the possibility of water being capable of dissolving those bodies in the mineral regions, and of thus changing the substance of one body into another, as naturalists have supposed, contrary to their knowledge, or in order to explain appearances, is so far from tending to increase our science, that it is abandoning the human intellect to be bewildered in an error; it is the vain attempt of lulling to sleep the scientific conscience, and making the soul of man insensible to the natural distress of conscious ignorance.

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But besides that negative argument concerning the insolubility of crystal, by which the erroneous suppositions of naturalists are to be rejected, crystal in general is found regularly concreted in the cavities of the most solid rock, in the heart of the closest agate, and in the midst of granite mountains. But these masses of granite were formed by fusion; I hope that I shall give the most satisfactory proof of that truth: Consequently, here at least there is no occasion for the action of water in dissolving siliceous substances in one place, in order to concrete and crystallise it in another.

In these cavities of the solid granite rock, where crystal is found regularly shooting from a basis which is the internal surface of the cavity, we find the other constituent substances of the granite also crystallised. I have those small cavities, in this rock, from the island of Arran, containing crystal, felt-spar, and mica, all crystallised in the same cavity[34]. But this is nothing to the *druzen* or crystalline concretions, which are found in a similar manner among metallic and mineral substances in the veins and mines; there, every species of mineral and metallic substance, with every variety of mixture and composition, are found both concreted and crystallised together in every imaginable shape and situation.

[Note 34: The Chevalier Dolomieu makes the following observation. Journal de Physique, Juillet 1791.

“J’ai ete etonne de trouver au centre d’un enorme massif de granit, que l’on avoit ouvert avec la poudre pour pratiquer un chemin, des morceaux, gros comme le poing et au dessous, de spath calcaire blanc, tres-effervescent, en grandes ecailles, ou lames entrecroisees. Il n’occupoit point des cavites particulieres, il n’y paroissoit le produit d’une infiltration qui auroit rempli des cavites, mais il etoit incorpore avec les feld-spath, le mica, et le quartz, faissoit masse avec eux, et ne pouvoit se rompre sans les entrainer avec lui.”

This great naturalist is convinced that the spar had not been here introduced by infiltration, although that is the very method which he employs to form concretions, not only of spar but of crystal, zeolite, and pyrites, in the closest cavities of the most solid rocks of basaltes. These four substances in this stone were so mixed together that nothing but the fusion of the whole mass could explain the state in which they appeared; but, thinking that such a supposition could not be allowed, this naturalist, like a man of science when his data fail, leaves the matter without any interpretation of his own. This however is what he has not done in the case of basaltes, or that which he mistakes for proper lavas, as I shall have occasion to show.]

Here is an infinite operation, but an operation which is easily performed by the natural arrangement of substances acting freely in a fluid state, and concreting together, each substance, whether more simple or more compound, directing itself by its internal principle of attraction, and affecting mechanically those that are concreting around it.

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We see the very same thing happen under our eye, and precisely in the same manner. When a fluid mass of any mineral or metallic substance is made to congeal by sudden cooling on the outside, while the mass within is fluid, a cavity is thus sometimes formed by the contraction of the contained fluid; and in this cavity are found artificial *druzen*, as they may be called, being crystallizations similar to those which the mineral cavities exhibit in such beauty and perfection.

Petrification and consolidation, in some degree, may doubtless be performed, in certain circumstances, by means of the solution of calcareous earth; but the examples given by M. de Luc, of those bodies of lime-stone and agate petrified in the middle of strata of loose or sandy materials, are certainly inexplicable upon any other principle except the fusion of those substances with which the bodies are petrified[35].

[Note 35: Vid. Lettre 28 et Lettre 103. Lettres Physiques et Morales.]

This subject deserves the strictest attention; I propose it as a touchstone for every theory of petrification or perfect consolidation. First, There are found, among argillaceous strata, insulated bodies of iron-stone, perfectly consolidated; secondly, There are found, in strata of chalk and lime-stone, masses of insulated flints; thirdly, There are found, in strata of sea sand, masses of that sand cemented by a siliceous substance; fourthly, In the midst of blocks of sand-stone, there are found masses of loose or pure sand inclosed in crystallised cavities; and in this sand are found insulated masses of crystallised spar, including within them the sand, but without having the sparry or calcareous crystallization disturbed by it. There are also other globular masses of the same kind, where the sparry crystallization is either not to be observed, or appears only partially[36]: And now, lastly, In strata of shell-sand, there are found masses of consolidated lime-stone or marble. In all those cases, the consolidated bodies are perfectly insulated in the middle of strata, in which they must of necessity have been petrified or consolidated; the stratum around the bodies has not been affected by the petrifying substance, as there is not any vestige of it there; and here are examples of different substances, all conspiring to prove one uniform truth. Therefore, a general theory of petrification or consolidation of mineral bodies must explain this distinct fact, and not suffer it any longer to remain a *lusus naturae*.

[Note 36: Mem. de l'Academie Royale des Sciences, an. 1775.]

Let us now consider what it is that we have to explain, upon the supposition of those concretions being formed from a solution. We have, first, To understand what sort of a solution had been employed for the introducing of those various substances; secondly, How those concretions had been formed from such solutions within those bodies of strata; and, lastly, How such concretions could have been formed, without any vestige appearing of the same substance, or of the same operation, in the surrounding part of the stratum. Whatever may be the difficulty of explaining those particular appearances by means of fusion and mechanical force, it is plainly impossible to conceive those

bodies formed in those places by infiltration, or any manner of concretion from a state of solution.

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Naturalists, in explaining the formation of stones, often use a chemical language which either has no proper meaning, or which will not apply to the subject of mineral operations. We know the chemical process by which one or two stony concretions may be formed among bodies passing from one state to another. When, therefore, a change from a former state of things in mineral bodies is judged by naturalists to have happened, the present state is commonly explained, or the change is supposed to have been made by means of a similar process, without inquiring if this had truly been the case or not. Thus their knowledge of chemistry has led naturalists to reason erroneously, in explaining things upon false principles. It would be needless to give an example of any one particular author in this respect; for, so far as I have seen, it appears to be almost general, every one copying the language of another, and no one understanding that language which has been employed.

These naturalists suppose every thing done by means of solution in the mineral kingdom, and yet they are ignorant of those solvents. They conceive or they imagine concretions and crystallizations to be formed of every different substance, and in every place within the solid body of the earth, without considering how far the thing is possible which they suppose. They are constantly talking of operations which could only take place in the cavities of the earth above the level of the sea, and where the influence of the atmosphere were felt; and yet this is the very place which we have it in our power to examine, and where, besides the stalactite, and one or two more of the same kind, or formed on the same principle, they have never been able to discover one of the many which, according to their theory, ought always to be in action or effect. So far from knowing that general consolidating operation, which they suppose to be exerted in filling up the veins and cavities of the earth by means of the infiltrating water of the surface, they do not seem fully to understand the only operation of this kind which they see. The concretion of calcareous matter upon the surface of the earth is perhaps the only example upon which their theory is founded; and yet nothing can be more against it than the general history of this transaction.

Calcareous matter, the great *vinculum* of many mineral bodies, is in a perpetual state of dissolution and decay, in every place where the influences of air and water may pervade. The general tendency of this is to dissolve calcareous matter out of the earth, and deliver that solution into the sea. Were it possible to deny that truth, the very formation of stalactite, that operation which has bewildered naturalists, would prove it; for it is upon the general solubility of calcareous matter exposed to water that those cavities are formed, in which may be found such collections of stalactical concretion; and the general tendency of those operations is to waste the calcareous

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bodies through which water percolates. But how is the general petrifaction or consolidation of strata, below the surface of the sea, to be explained by the general dissolution of that consolidating substance in the earth above that level? Instead of finding a general petrifying or consolidating operation in the part of the earth which we are able to examine, we find the contrary operation, so far at least as relates to calcareous spar, and many other mineral bodies which are decomposed and dissolved upon the surface of the earth.

Thus in the surface of the earth, above the level of the sea, no petrifying operation of a durable nature is found; and, were such an operation there found, it could not be general, as affecting every kind of substance. But, even suppose that such a general operation were found to take place in the earth above the level of the sea, where there might be a circulation of air and percolation of water, How could the strata of the earth below the level of the sea be petrified? This is a question that does not seem to have entered into the heads of our naturalists who attempt to explain petrifaction or mineral concretion from aqueous solutions. But the consolidation of loose and incoherent things, gathered together at the bottom of the sea, and afterwards raised into rocks of various sorts, forms by far the greatest example of petrification or mineral operation of this globe. It is this that must be explained in a mineral theory; and it is this great process of petrifaction to which the doctrine of infiltration, whether for the mechanical purpose of applying cohesive surfaces, or the chemical one of forming crystallizations and concretions, will not by any means apply.

Nothing shows more how little true science has been employed for the explanation of phenomena, than the language of modern naturalists, who attribute, to stalactical and stalagmical operations, every superficial or distant resemblance to those calcareous bodies, the origin of which we know so well. It is not a mere resemblance that should homologate different things; there should be a specific character in every thing that is to be generalised. It will be our business to show that, in the false stalactites, there is not the distinctive character of those water formed bodies to be found.

In the formation of stalactical concretions, besides the incrustation as well as crystallization of the stony substance from the aqueous vehicle by which it had been carried in the dissolved state, we have the other necessary accompanyments of the operation, or collateral circumstances of the case. Such, for example, is that tubular construction of the stalactite, first formed by the concretion of the calcareous substance upon the outside of the pendant gut of water exposed to the evaporation of the atmosphere; we then see the gradual filling up of that pervious tube through which the petrifying water had passed for a certain time; and, lastly, we see the continual accretion

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which this conducting body had received from the water running successively over every part of it. But among the infinite number of siliceous concretions and crystallizations, as well as those of an almost indefinite variety of other substances, all of which are attributed to solution, there is not the least vestige of any collateral operation, by which the nature of that concretion might be ascertained in the same manner. In all those cases, we see nothing but the concreted substances or their crystallizations; but, no mark of any solvent or incrusting process is to be perceived. On the contrary, almost all, or the greatest part of them, are so situated, and attended with such circumstances, as demonstrate the physical impossibility of that being the manner in which they had been concreted; for, they are situated within close cavities, through which nothing can pervade but heat, electricity, magnetism, *etc*.; and they fill those cavities more or less, from the thinnest incrustation of crystals to the full content of those cavities with various substances, all regularly concreted or crystallised according to an order which cannot apply to the concretion of any manner of solution.

That there is, in the mineral system, an operation of water which may with great propriety be termed *infiltration*, I make no doubt. But this operation of water, that may be employed in consolidating the strata in the mineral regions, is essentially different from that which is inconsiderately employed or supposed by mineralists when they talk of infiltration; these two operations have nothing in common except employing the water of the surface of the earth to percolate a porous body. Now, the percolation of water may increase the porousness of that body which it pervades, but never can thus change it from a porous to a perfect solid body. But even the percolation of water through the strata deposited at the bottom of the sea, necessarily required, according to the supposition of naturalists, must be refused; for, the interstices of those strata are, from the supposition of the case, already filled with water; consequently, without first removing that stagnant water, it is in vain to propose the infiltration of any fluid from the surface.

This is a difficulty which does not occur in our theory, where the strata, deposited at the bottom of the sea, are to be afterwards heated by the internal fires of the earth. The natural consequence of those heating operations may be considered as the converting of the water contained in the strata into steam, and the expulsion of steam or vapour, by raising it up against the power of gravity, to be delivered upon the surface of the earth and again condensed to the state of water.

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Let us now conceive the strata, which had been deposited at the bottom of the sea, as exhausted of their water, and as communicating with the surface of the earth impregnated with water. Here again we have the power of gravity to operate in carrying down water to that place which had been before exhausted by the power of heat; and in this manner, by alternately employing those two great physical agents, we cannot doubt that nature may convey soluble substances from above, and deposit them below for the purpose of consolidating porous bodies, or of filling with saline and earthy matter those interstices which had been originally filled with water, when the strata were deposited at the bottom of the sea. How far any marks of this operation may be perceived, by carefully examining our mines and minerals, I know not; I can only say that, on the contrary, whenever those examined objects were clear and distinct, with the concomitant circumstances, so as to be understood, I have always found the most certain marks of the solid bodies having concreted from the fluid state of fusion. This, however, does not exclude the case of infiltration having been previously employed; and I would intreat mineralists, who have the opportunity of examining the solid parts of the earth, to attend particularly to this distinction. But do not let them suppose that infiltration can be made to fill either the pores or veins of strata without the operation of mineral heat, or some such process by which the aqueous vehicle may be discharged.

Not only are mineral philosophers so inconsiderate, in forming geological theories upon a mere supposition or false analogy, they have even proceeded, upon that erroneous theory, to form a geological supposition for explaining the appearances of strata and other stony masses in employing a particular physical operation, which is, that of *crystallization*[37]. Now crystallization may be considered as a species of elective concretion, by which every particular substance, in passing from a fluid to a solid state, may assume a certain peculiar external shape and internal arrangement of its parts, by which it is often distinguished. But, to suppose the solid mineral structure of the earth explained, like an enigma, by the word *crystallization*, is to misunderstand the science by which we would explain the subject of research; and, to form a general mineral theory thus upon that term, is an attempt to generalise without a reason. For, when it were even admitted that every solid body is crystallised, we thus know no more of the geology of this earth, or understand as little of the general theory of mineral concretion, as we did before;—we cannot, from that, say whether it be by the operation of solution or of fusion which had produced the perceived effect.

[Note 37: Journal de Physique; Avril 1753.]

M. de Carosi has wrote a treatise upon certain petrifactions[38]. In the doctrine of this treatise there is something new or extraordinary. It will therefore be proper to make some observations on it.

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[Note 38: Sur la Generation du Silex et du Quartz en partie. Observations faites en Pologne 1783, a Cracovie.]

The object of this treatise is to describe the generation of silex and quartz, with their modifications or compositions, formed within mineral bodies of a different substance. The natural history contained in this little treatise is well described and sufficiently interesting. But It is chiefly in order to examine the means which, according to the theory of this treatise, are employed in petrifying bodies, that I consider it in this place.

The first section of this treatise has for title, *Generation du Caillou et du Quartz de la terre calcaire pure*. It may be worth while to compare the natural history of this part of the earth with the flint and chert found in our chalk and lime-stone countries. I shall therefore transcribe what is worth observing upon that subject (p. 5.).

“Nous rencontrons chez nous dans les parties le plus montagneuses, et les moins couvertes de terreau, ou tout-au plus de sable, entre de purs rochers calcaires une quantite incroyable de cailloux (silex) tant en boules, que veines, couches, et debris. Au premier coup d’oeil l’on s’imagine que ce font des debris de montagnes eloignees, qui y furent amenes par les eaux, mais, en examinant la chose de plus pres, on est convaincu, que ce sont tout au contraire, des parties detachees des montagnes de la contree. Car il y a sur presque toute l’etendue de nos montagnes calcaires une couche, ou pour mieux dire, un banc compose de plusieurs couches de base calcaire, mais qui ou sont parsemees irregulierement de boules, de rognons, de veines, et de petits filons de silex, ou qui contiennent cette pierre en filon, veines, et couches paralleles, et regulierement disposees. Les boules et rognons de silex y font depuis moins de la grandeur d’une petite noisette, jusqu’au diametre de plus de six pouces de notre mesure. La plupart de ces boules tant qu’elles sont dans l’interieur cache de la roche vive, et qu’elles n’ont rien souffert de l’impression de l’air, ont, pour l’ordinaire, une croute de spath calcaire, au moyen de la quelle elles sont accrues a la roche mere; ou pour mieux dire la croute spatheuse fait l’intermede entre le silex, et la roche calcaire, par ou se fait le passage de l’une a l’autre. Mais ceci ne vaut que de boules de silex entierement formees. C’est dont on peut meme se convaincre a la vue, par beaucoup de pierres dont le pave de la ville de Cracovie est compose. Mais la, ou le silex n’est pas encore entierement acheve, la croute spatheuse manque, en revanche on y voit evidemment le passage par degres successifs de la roche calcaire au silex qui y est contenu, et les nuances de ce passage sont souvent si peu marquees que meme les acides mineraux ne suffisent pas a les determiner, ce n’est que le briquet, qui nous aide a les decouvrir. On voit bien ou la pierre calcaire s’enfonce en couleur, l’on s’appercoit, ou sa durete, ses cassures changent, mais, comme elle y souffre encore quelque impression des acides, l’on ne sauroit determiner au juste le point, ou elle a deja plus de la nature du silex, que de celle de la chaux, qu’en la frappant du briquet.

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“Tels sont les cailloux en boules et rognons avant leur etat de perfection, il y aura meme au milieu une partie de pierre calcaire non changee.

“Ceux au contraire, ou la nature a acheve son ouvrage, ont une croute de chaux endurcie, et sont purement du silex fini, mais de toutes couleurs, d’un grain et d’une texture plus ou moins fine, qui passe assez souvent par degres dans les differentes varietes du noble silex. Ils ont, pour l’ordinaire, dans leur interieur une cavite, mais pas toujours au centre, et qui vient apparemment de la consommation de cette partie calcaire qui y resta la derniere, et n’en fut changee ou dissolute et separee, que lorsque le reste du silex etoit deja entierement fini. Ces cavites sont toujours, ou enduites de calcedoine en couche concentriques recouverte de petits cristaux fort brillans et durs de quartz, ou bien seulement de ces derniers-ci. Par-fois il y a aussi du spath calcaire crystallise, mais cela est extremement rare. Quelque-fois enfin ces cavites sont remplies d’une noix de calcedoine. Je n’ai reussi qu’une seule fois en cassant un pareil silex en boule d’y trouver encore le reste de l’eau de crystallisation.”

The only remark that I would here make is this, that, if the crystallization of those close cavities in the *silex* had at any time required water of solution, it must always have required it. But, if there had been water of solution contained in those close cavities, for the crystallization of the various things which are often found within them, How comes it that this water is almost never found? I have good reason to believe that water contained within a solid flint will not make its escape, as does that contained in the *anhydrites* of Mount *Berico*, which are composed of a porous calcedony. But the siliceous crystallizations within close cavities is a curious subject, which we shall have occasion to examine more particularly in treating of agates. We now proceed to the next section, which is the generation of silex and quartz in marl, (p. 19.)

“Il y a des contrees, chez nous, qui out des etendus assez considerables en long et en large, de montagnes de pierre de marne calcaire, dans lesquelles on rencontre le meme phenomene que dans celles de chaux pure; c. a. d. nous y trouvons du silex de differentes varietes, et dans tous les degres successifs de leur formation, et de leur perfection. Outre cela, nous y voyons encore quelque chose, qui semble nous conduire a la decouverte des moyens, dont se sort la nature pour effecteur cette operation, et qui nous etoit cache dans les montagnes de chaux pure: ces bancs de pierre marnesilicieuse, contiennent une partie considerable de pyrites sulfureuses, qui non seulement y forment une grande quantite de petits sillons, mais toute la masse de la montagne est rempli de parcelles souvent presqu’imperceptibles de ce mineral. Ces pyrites sont evidemment des productions du phlogistique et de l’acide contenu dans la montagne.

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“L’eau, qui s’y trouve ordinairement en assez grande abondance, en detacha, extraha d’un et l’autre, et les combina apres tous les deux ensemble. Cette meme eau les dissout derechef, et en fait de nouvelles combinaisons. C’est ce qu’on voit evidemment la, ou la nature, ayant commence ses operations, il n’y est reste de la pyrite, qu’une portion de la partie inflammable liee a une base terrestre. Dans ces endroits la marne n’est que fort peu sensible aux acides, et de blanche qu’elle etoit, sa couleur est devenue presque noire. C’est la qu’on observe les differens degres du changement de la marne en silex, contenant, meme encore, par fois, de parties pyriteiques non detruites dans son interieur. Et comme la nature forme ici, de meme, que dans la chaux pure les silex, la plupart en boules ou rognons; comme les different degres de metamorphoses de la marne en silex, sont ici beaucoup plus nombreuses que la, de sorte qu’il y a des bandes entieres, qui meriteroient plutot d’etre appelles bandes silicieuses, que marneuses; comme il y a, enfin, une grande quantite de pyrites, qu’ailleurs, il est tres probable qu’elle se serve la du meme moyen qu’ici pour operer la metamorphose en question.

“Ne nous precipitons, cependant, pas a en tirer plus de consequences; poursuivons plutot le fil de notre recit.

“Le silex, qui se trouve ici, est non seulement de differents degres de perfection, il est de plus d’une espece. Il y a de la pierre a feu, 2 de la calcedoine, 3 des agathes, et 4 differentes nuances et passages des especes ordinaires aux fines du silex.

“La pierre a feu, est, ordinairement dans son etat de perfection d’un grain assez fin, d’une couleur grise plus ou moins foncee, et meme donnant, dans le noiratre, plus ou moins diaphane; ses cassures sont concentriques ou coquillees, et sa masse est assez compacte. Outre sa conformation ordinaire en boules et rognons, elle fait presque toujours la noix de ursins marins, qui y font en grand nombre, et dont la coquille est le plus souvent, et presque toujours de spath calcaire, meme au milieu d’une boule de silex parfait.

“Les calcedoines et agathes de ces couches sont toujours (au moins, je ne les ai pas encore vues autrement) de coraux et autres corps marins petrifies. Donc, il faut que les couches de pierres roulees, d’ou j’ai tire ma collection citee plus haut, soyent des debris de montagne” detruites de cette espece. Il y en a qui sont tres parfaites comme celles qui composent ma collection, d’autres meritent plutot d’etre rangees parmi les passages du silex ordinaire, et ses especes plus fines; d’autres encore sont, en effet, de vraies agathes, mais qui renferment dans leur interieur plus ou moins de parties non parfaites presque calcaires, qui s’annoncent d’abord par leur couleur blanche, par leur gros grains relativement au reste, par leur opacite, par leur mollesse respective, et souvent meme par leur sensibilite pour les acides mineraux. Mais celles, qui sont finies, quoiqu’elles ayent, pour la plupart, une couleur presque noire, ne laissent, cependant, pas d’avoir aussi des teintes plus claires comme brunatres, verdatres, rougeatres, jaunatres, bleuatres, tachetees, veinees, *etc*. Leur clarte n’est pas moins variable, que

leur couleur, il y en a de presqu’opaques, comme aussi de presque transparentes, sur tout la, ou la calcedoine predomine.

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“Le quartz s’y trouve comme dans les pierres de la premiere section, c, a, d, crystallise, en groupes dans de petites cavites; quelquefois aussi en veines. La calcedoine y est de meme, ou bien en mamelons, ou bien en stalactites, lorsqu’elle a de la place pour s’y deposer.

“Un phenomene encore plus curieux que cela est cette belle pyrite sulphureuse jaune, comme de l’or, qui est quelquefois parsemee par tout la substance de petrifications agathisees, et qui apparemment y fut deposee apres la dite metamorphose a la faveur des petits pores, qui y etoient restes ouverts.”

I would beg that mineralists, who use such language as this, would consider if it contains a distinct idea of the operation which they would thereby describe, or if it does not contain either a contradiction or an inconceivable proposition. It supposes a calcareous body to be metamorphosed, somehow by means of the mountain acid, into a siliceous body. But, finding many bodies of pyrites contained within that solid flint, it is said, that, when the calcareous body was flintified, there were left in it cavities which were afterwards filled with pyrites. Let us reflect a moment upon this doctrine. These cavities were first open to the outside of the flinty body; but now the pyrites with which they had been filled is insulated in the solid flint. Here three things are required; first, The calcareous body is to be flintified, at the same time leaving the body full of small cavities open to the outside; secondly, These cavities are to be filled with pyrites; lastly, These mineral bodies are to be so inclosed within the flint, as to leave no vestige of the former processes. This marly mountain itself, which had been formed of loose materials gathered together at the bottom of the sea, was first to be filled with pyrites, in various shapes, by means of the phlogistic and the acid of the mountain. Here is proposed to us an operation which is totally unknown, or of which we have no kind of idea. But, let us suppose pyrites formed in this mountain, (of whatever chemical substances), by means of water; Why should water again undo that pyrites, in order to form other concretions? And, Why should the flint be formed first with cavities, and then made solid, after pyrites had been introduced into those cavities of the agate, and, as our author expresses it, *parsemee pour toute la substance?* Here are suppositions which are not only perfectly gratuitous, but are also inconsistent with any thing that we understand. This is not explaining nature; it is only feigning causes[39].

[Note 39: The description of those insulated siliceous bodies, containing in their closed cavities all the usual concretions of calcedony and crystals, as well as full of small pyrites floating in the solid flint, are extremely interesting to a mineral system, or such a geological theory as should explain the present state of things in those strata that had been formed by deposits of known materials at the bottom

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of the sea; they are indeed such appearances as may be found, more or less, in all consolidated strata. But it is this author's explanation of that petrifaction which is our present object to consider; and, as he is so particular in giving us his theory upon the subject, it is easy to detect the error of his reasoning. Were those naturalists who explain things only in general, by saying that water is the agent, and infiltration the means employed by nature;—were these naturalists, I say, to give us as particular a description of their process, it would appear as inconsistent with the nature of things as that which we have from this author, who examines nature very minutely, and who sees distinctly that the infiltrating theory is inapplicable for the explanation of those petrifactions.]

The third section has for title, "*Generation du Silex et Quartz de la Pierre Puante*." Here we find an example worthy of being recorded, as contributing to throw great light upon those mineral operations; however, the opinion of our author and mine, upon this subject, differ widely. He proceeds thus:

"Cette pierre n'est, comme chacun le scait, qu'une pierre calcaire contenant du bitume.

"Nos montagnes n'en contiennent seulement pas de simples couches, mais il y en a meme de grandes bancs fort epais.

"Le caillou, ou silex qui s'y genere, forme, tantot de gros blocs informes, qui occupent des cavites dans l'interieure des montagnes, tantot, enfin, en forme de filons.

"J'ai remarque cette metamorphose sur trois endroits differens, dans chacun des quels la nature a autrement opere.

"Sur l'un, la pierre puante fait un banc horizontal dans une montagne de pierre calcaire crystalline, ou d'une espece de marbre, qui contient des couches et filons de metal. Ce banc de pierre puante y fait le toit d'une couche de galene de plomb et de pierre calaminaire, et dans ses cavites et fentes il y a non seulement des blocs de grandeur differente, mais aussi des veines et petites bandes courtes de silex, tant ordinaire, que noble c, a, d, de la pierre a feu, de calcedoine, d'agathes, et meme d'une espece de cornaline jaune et rouge pale. Je ne m'arreterai pas a en detailler les varietes, parce qu'elles sont trop accidentelles. Je ne les connois pas meme toutes, il s'en faut de beaucoup, parce qu'elles se trouvent dans des anciennes mines negligees, peut etre depuis plus d'un siecle, et par consequent peu accessibles. Je ne doute, cependant pas, que, si l'on pouvoit mieux sonder le terrain, on y trouveroit bien plus encore du peu que j'ai cite. Parmi ce silex, il y a aussi de petites groupes et de petites veines de quartz solide et crystallise.

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“Au second endroit la pierre puante fait un filon, ou si l’on veut, une couche ou bande verticale, qui partage la montagne en deux parties presqu’egales de l’epaisseur de trois aunes a peu pres. La montagne, ou cela se voit est aussi une ancienne mine de cuivre et de plomb, consistant en plusieurs varietes de marbre, different en couleur et en grain, deposees par couches les unes sur les autres. Le filon de silex est forme de feuilles alternatives de pierre puante et de silex, tous les deux de couleur brun de bois a peu pres; mais le silex est plus fonce que sa compagne. Ces feuilles alternatives, consistent d’autres bien plus minces encore, qui souvent n’ont pas l’epaisseur d’une ligne, mais ce qu’il y a de plus curieux, c’est que la meme feuille est d’un but de pierre porque, qui, vers le milieu, passe successivement en silex, qui, a son tour, vers l’autre but, qui etoit expose a l’air repasse par les memes gradations en une espece de tuffe calcaire. Ce qui nous fait voir evidemment la generation et la destruction du silex, meme avec une partie des moyens par lesquels elle s’opere. Comme l’endroit de cette decouverte n’est accessible qu’a la superficie, je ne saurois dire s’il y a d’autres varietes de silex outre la dite. Il l’est a supposer autant par analogie, que par quelques morceaux qui ont de petites veines transversales d’une espece de calcedoine, et qui sont, meme, sur leur fentes, garnis de petits cristaux de roche. Mais ce qu’il y a de sur c’est que ce filon, parvenu a une certaine profondeur, s’ennoblit et contient du metal, c. a. d. de la galene de plomb, et de la pyrite cuivreuse, j’y en ai trouves de morceaux, qui en font de preuves incontestables. Le caillou d’ici est un grain fin d’une texture forte, peu transparent, donne beaucoup d’etincelles au briquet, mais ses cassures sont ecailleuses.

“La montagne calcaire du troisieme lieu a une couche de pierre puante epaisse de plusieurs aunes, qui, derechef contient de petites couches irregulieres et des bandes transversales de silex, qui ont jusques a six pouces passes d’epaisseur. La pierre puante est d’une couleur gris-brune, d’un grain assez fin, et d’un tissu assez dur; ses cassures sont irregulieres, mais plus la pierre s’approche du silex, plus elles donnent dans le coquille. Le silex ordinaire est d’un brun de bois, d’un grain assez fin, et d’un tissu resistant, et ses cassures sont egales a la pierre porque. Ce n’est pas la la seule variete, il y a, aussi, de la calcedoine et des agathes de couleurs differentes. Meme la pierre a feu est assez souvent traversee de veines de calcedoine, de quartz crystallise, et de spath calcaire blanc en feuilles et en crystaux. Il arrive que la meme veine est composee de ces trois especes de pierres a la fois, de sorte que l’une semble passer dans l’autre, parce que les limites reciproques sont, souvent, assez indistinctes. Il est evident, que le silex est forme de la pierre puante, parce qu’on remarque ici les memes phenomenes dont j’ai parle plus haut, c. a. d. les passages successifs de l’une dans l’autre pierre, tant en montant qu’en descendant.”

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There is nothing particular in the siliceous mixture in this species of lime-stone, except the vein of that substance. It is evident that this vein, traversing the mountain, had been introduced in the fluid state of fusion. I do not mean to say, that, in this particular case now described, the evidence of that truth peculiarly appears; but that, from the general nature of mineral veins breaking and traversing the solid strata of the globe, no other conclusion can be formed; and that in the particulars of this example there is nothing that could lead us to suppose any other origin to the petrifactions contained in this vein of stinking lime-stone. It is plain, that our author has imagined to himself an unknown manner of executing his mineral metamorphoses. He sees plainly that the common notion of infiltration will not at all explain the evident confusion of those calcareous and siliceous bodies which appear to him to be metamorphosing into each other. Nothing, indeed, can explain those phenomena but a general cause of fluidity; and there is no such general cause besides that of heat or fusion.

But to show how mineralists of great merit, gentlemen who have examined systematically and with some accuracy, may impose upon themselves in reasoning for the explanation of mineral appearances from limited notions of things, and from the supposition of these having been formed where they now are found, that is, upon the surface of the earth, I would beg leave to transcribe what this author has said upon this species of petrifaction. It is not that he is ignorant of what mineralists have already said upon the subject; it is because he sees the incompetency of their explanations in those particular cases; and that he would employ some other more effectual means. (p. 50.)

“Toute terre calcaire a changer dans une autre doit, avant toute chose, etre rendue refractaire ce qui ne peut se faire qu’en la saturant avec un acide. Mais une terre simplement, saturee d’un acide, est d’une reduction fort aisee, vu que l’acide n’y tient pas trop fort, d’ailleurs ce n’est qu’un sel neutre terreux fort facile a dissoudre dans une quantite suffisante d’eau. Or pour rendre cette union plus constante, il faut que la terre alcaline s’assimile intimement a l’acide, ce qui ne se sera jamais sans un intermedeliant, qui homogene les parties de ce nouveau corps, et pour que cela ce fasse il est indispensable, qu’il s’opere une dissolution fonciere des parties terrestres de la chaux, qui facilite l’ingress a l’acide, et a l’intermede pour qu’ils s’y lie bien fortement. Supposons qu’il se forme une liqueur savonneuse de l’acide et du phlogistique, que l’air fixe, mis en liberte, ouvre les interstices des parties qui constituent la terre alcaline, qu’apres cela cette liqueur savonneuse ayant l’entree libre s’assimile a la terre en proportion requise, que l’eau, qui servoit de vehicule dans cette operation, s’evapore successivement, et emporte le superflu des ingrediens,

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pour qu'il se puisse operer le rapprochement le plus exacte des parcelles ou molecules homogenees de nouveau corps qu'enfin les molecules les plus pures et les mieux affinees soyent reunies en forme liquide dans des cavites, et que par l'evaporation et separation de l'eau, ou elles nageoient, il s'en forme des crystaux n'aurons-nous pas une boule de silex, avec de crystaux de quartz dans ses creux interieurs."

The supposed case is this; a calcareous body is to be metamorphosed into a siliceous nodule, having a cavity within it lined with quartz, crystals, *etc*. M. de Carosi means to inform us how this may be done. Now, as this process requires no other conditions than those that may be found upon the surface of this earth, the proper way to prove this hypothetical theory, would be to exhibit such a mineral body produced by those means. But, even supposing that such a process were to be exhibited, still it would remain to be explained, how this process, which requires conditions certainly not be found at the bottom of the sea, could be accomplished in that place, where the strata of the earth had been deposited, accumulated, consolidated, and metamorphosed.

This mineral process, which has been now described, will no doubt revolt the opinions of many of our chemists as well as naturalists; and I should not have thought of transcribing it, but as an example of that inconclusive reasoning which prevails in mineralogical writings upon this subject.

But this is not all. We have, upon this occasion, a most remarkable example of the fallaceous views that may be taken of things; and of the danger to science when men of sense and observation form suppositions for the explanation of appearances without that strict conformity with the principles of natural philosophy which is requited on all occasions. Both M. de Carosi, and also M. Macquart[40], to whom our author communicated his ideas and proper specimens, assert, that from their accurate experience, they find calcedony growing daily, not only in the solid body of gypsum, *etc*. while in the mine, but also in the solid stone when taktn out of the mine, and preserved in their cabinet.

[Note 40: Vid. Essais de Mineralogie par M. Macquart.]

What answer can be made to this positive testimony of these gentlemen, by a person who has not seen any such a thing, and who has not the opportunity of examining the cases in which those naturalists may have perhaps been led into some delusion? Were I however to conjecture upon a subject in which I have not any positive information, I should suppose that some part of the calcedony, like the *oculus mundi* when dipped in water, may be so transparent, while containing some portion of humidity, that it is not easily distinguishable from the gypsum in which it is concreted; but that in having the humidity evaporated, by being taken out of the mine and exposed to the dry air, those

portions of calcedony, which did not before appear, may be perceived by becoming more opaque[41].

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[Note 41: From the description given in this treatise, and from the drawings both of M. de Carosi and M. Macquart, I find a very valuable inference to be made, so much the more interesting, as I have not found any example of the like before. This arises from the intimate connection which is here to be perceived between agate and gypsum. Now, upon this principle, that the agate-calcedony had been formed by fusion, a truth which, from the general testimony of minerals, I must presume, it is plain, that those nodules of gypsum had been in the fluid state of fusion among those marly strata, and that the gypseous bodies had been penetrated variously with the siliceous substance of the calcedony.

The description of those siliceous penetrations of gypsum is followed by this conclusion: “En voila assez, je crois pour faire voir que le silex ci-decrit est effectivement une emanation du gypse, et non pas une matiere heterogene amenee d’autre part et deposee, ou nous la voyons.” In this instance our author had convinced himself that the calcedony concretions had not been formed, as he and other mineralists had before supposed, by means of infiltration; he has not, however, substituted any thing more intelligible in its stead. I do not pretend that we understand mineral fusion; but only that such mineral fusion is a thing demonstrable upon a thousand occasions; and that thus is to be explained the petrification and consolidation of the porous and naturally incoherent strata of the earth.]

There is, however, a subject in which I can more freely accuse this author of being deceived. This naturalist says, that calcareous stones become silex by a certain chemical operation; and that those flinty bodies, in being exposed upon the surface of the earth, out of their natural bed, are again, by a contrary chemical operation, changed from flint to a calcareous substance. I will give it in his own words, (p. 56.)

“Cela dit, venons au fait. Tout silex progenere de chaux, detache de son lieu natal, et expose aux changemens de saisons, s’amollit, recoit de crevasses, perd sa transparence, devient, enfin, tout-a-fait opaque, le phlogistique s’en evapore, l’acide en est detache, lave, et de terre vitrescible, qu’il etoit, il redevient chaux, comme il etoit auparavant.”

Here is no question with regard to mere opinion, but to matter of fact; and, in this case, nothing is more evident, than that upon the surface of this earth, that is, in the examinable parts above the level of the sea, there is no transition either of calcareous bodies into flint, nor of flinty bodies into calcareous substance. Calcareous matter is constantly dissolved by water, when it is exposed to the washing of that fluid; and it is even dissolved out of the most perfect union or combination with siliceous substance, and the most solid composition of an insoluble body, as may be perceived in the decaying of feld-spar. A superficial view of flints, which have come out of a body of chalk, may have created such an opinion, which will not either bear the light of chemical or mineral investigation. The subject of these chalk flints will be minutely examined in its proper place.

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Our author has carefully examined the subject of flintification; and the country where he makes his observations would seem to be well disposed for such a research. He has had great opportunity and inclination to examine the subject which he writes upon; and he has given a distinct account of what be has seen. His description of the flintification of sand-stone is extremely interesting. I will therefore transcribe it, both as a valuable portion of natural history, and also in order to contrast this author's opinion, with regard to the means employed by nature in petrifying bodies, and that which I maintain to be the general consolidating operation of the globe. It is Section V. *Generation du Caillou du Silex du Gres, ou Pierre Sablonneuse*.

"Tout gres est susceptible de cette metamorphose quant au grain et quant a la couleur; depuis la breccia quartzeuse jusqu'a la pierre a rasoir; et depuis le gres blanc jusqu'au brun et presque noiratre, tient ou non tient, dur, ou presque friable, c'est indifferent, toutes ces varietes donnent du silex, et surtout de la calcedoine, de la cornaline, et des agathes. Quant au ciment je l'y ai toujours remarque calcaire et faisant effervescence avec les acides dans les endroits de la pierre qui n'etoient point encore changes; et jamais je n'ai vu ce changement dans du gres dont le ciment fut ou quartzeux ou argileux et refractaire. Ainsi le ciment entre pour quelque chose dans ce changement.

"Le commencement de cette metamorphose paroit (autant que j'ai pu l'observer dans mes debris roules) se faire par le ciment, qui dissout la, ou les agens eurent l'acces libre, rend les grains en quartz mobiles, les emporte, les mele avec sa masse dense-liquide, les dissout, meme en partie, et forme, dans cet etat, des veines et de masses calcedonieuse, carneoliques, ou d'une autre espece de silex, au milieu du gres peu, ou pas du tout, change. Car autant que je puis voir, ce n'est pas par couches ou veines qu'elle s'opere, mais par boules et masses rond-oblongues. Au commencement ces veines et taches sont fort minces, et le reste du gres n'est point du tout, ou a peine sensiblement change hormis qu'il gagne, plus de consistence, a proportion du changement souffert. Mais a mesure que le silex y augmente et se perfectionne, on y appercoit les degres par lesquels a passe cette operation. Les nuance du passage d'une pierre a l'autre deviennent plus visibles, les veines et masses de silex grandissent au point, meme, qu'il y a jusqu'aux trois quart du gres change en silex clair comme de l'eau n'ayant que fort peu de grains de sable nageants dans sa masse. Des morceaux de cette espece sont rares a la verite, mais j'en ai, cependant, trouve quelques uns. Ordinairement, dans les beaux morceaux, le silex fait la base, et le sable y est, comme nageant tantot en grains separes tantot en parties et flocons. Dans les pieces moins belles, le sable fait la base, et le silex sert a la fois de ciment, et forme aussi

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plus ou moins de veines, qui traversent la masse en maintes et maintes directions. Mais si c'est un gres a gros grains, ou de la breccia, alors le reste prend la nature silicieuse mele de sable fin, et les gros grains de quartz restent tels, qu'ils etoient, sans changer. J'ai deja remarque que cette metamorphose semble s'operer, comme celle des cailloux d'origine calcaire en forme approchans la spherique, il faut encore y a jouter, que j'ai lieu de croire, qu'elle se fasse aussi du dedans en dehors, tout, comme la decomposition se fait du dehors au dedans.

"Il arrive dans cette pierre, comme dans toute autre, qu'il se forme des crystallisations dans les cavites. Lorsqu'elles sont de silex, leur figure est toujours mamelonnee, mais leur eau ou purete, leur grandeur et leur couleur n'est pas par tout egale. Il y en a qui sont grands, et de la plus pure calcedoine, d'autres sont petits et chaque goutte ou mamelon contient un grain de sable, de facon que cela a l'air d'un gres crystallise en mamelons ou stalagmitique. D'autres encore sont, de calcedoine, mais recouverts d'une croute, tantot blanche qui fait effervescence avec l'acide mineral, et qui est, par consequent, de nature calcaire; tantot cette croute est bleue foncee nuancee de bleu-celeste; tantot, enfin, elle est noire, mais toutes les deux refractaires. Outre ces crystallisations silicieuses, il y en a, quoique rarement, de quartzeuses, qui ou forment de petites veines de crystal, ou bien des groupes de crystaux quartzeux, ou qui enfin, enduisent les mamelons de silex."

Our author then makes a specification of the different varieties; after which he continues, p. 69.

"Apres tout ceci, l'on conviendra j'espere, que notre grais est une pierre bien singuliere, et surpassant, a bien des egards, le grais, faussement dit crystallise, de Fontainebleau. La raison de la figure du grais Francois est fort evidente, c'est le spath calcaire, qui lui sert de ciment, qui la lui fit prendre; mais qu'est-ce qui opere les metamorphoses racontees dans notre grais siliceux? Seroit-ce son ciment calcaire ou marneux par les memes raisons, qui font changer la marne en silex? La chose est tres-probable, et je n'en saurois pas meme, deviner d'autre. En ce cas la nature auroit un moyen d'operer par la voie humide, ce que nous faisons dans nos laboratoires en quelque facon, par la voie seche, c, a, d, de fondre et liquefier la terre vitrescible, au moyen des alcalis; secret que nous lui avons deja arrache en partie, en faisant la liqueur silicieuse."

"Je n'ose, cependant, decider pas meme hypothetiquement, sur cette matiere, pour n'avoir pu observer la nature dans ses ateliers, et parce que je ne possede que des pieces, qui detachees de leur lieu natal, depuis un tres long-tems, furent exposees aux intemperies des saisons, ou elles peuvent avoir souffert bien de changemens."

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There cannot be a more fair exposition of facts; and it is only our author's opinion of this mineral transmutation that I would controvert. I do not pretend to understand the manner of operating that our author here supposes nature to take. I only maintain, that here, as every where in general, the loose and incoherent strata of the globe have been petrified, that is, consolidated, by means of the fusion of their substances; and this I think is confirmed from the accurate description here given of the flintification of sand-stone. Here is described very distinctly an appearance which is very common or general on those occasions; this is the parts or particles of stone floating in the fluid siliceous substance, and there dissolving more or less.

M. de Carosi describes very systematically the generation of silex, calcedony, onyx, and quartz, in calcareous earth, marl, gypsum, sand-stone, and also what he terms *terre glaise, ou de l'Argile*. It is in this last that we find a perfect analogy with what is so frequent in this country of Scotland. These are the agates, calcedonies, calcareous and zeolite nodules, which are found produced in our whin-stone or subterraneous lavas, that is, the amygdaloides of Crondstedt. Naturalists explain the formation of those nodular bodies differently. The Chevalier de Dolomieu supposes these rocks to have been erupted lavas, originally containing cavities; and that these cavities in the solid rock had been afterwards filled and crystallised, by means of infiltration, with the different substances which are found variously concreted and crystallised within the solid rocks. Our author, on the contrary, supposes these formed by a species of chemical transmutation of calcareous and argillaceous earths, which, if not altogether incomprehensible, is at least not in any degree, so far as I know, a thing to be understood.

This is not the place where that subject of these particular rocks, which is extremely interesting, is to be examined. We shall afterwards have occasion to treat of that matter at large. It is sufficient here to observe, that our author finds occasion to generalise the formation of those petrifactions with the flintifications in calcareous and gypseous bodies. When, therefore, the formation of any of them shall be demonstrated, as having taken its origin in the fusion of those substances, this mode of operation, which is generalised in the consolidation of strata, will be properly inferred in all the rest.

Petrifaction is a subject in which mineralogists have perhaps wandered more widely from the truth than in any other part of natural history; and the reason is plain. The mineral operations of nature lie in a part of the globe which is necessarily inaccessible to man, and where the powers of nature act under very different conditions from those which we find take place in the only situation where we can live. Naturalists, therefore, finding in stalactical incrustation

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a cause for the formation of stone, in many respects analogous to what is found in the strata of the earth, and which had come from the mineral region in a consolidated state, have, without due consideration, attributed to this cause all the appearances of petrifaction or mineral concretion. It has been one of the objects of this work to show that this operation of incrustation, or petrifaction by means of solution, is altogether ineffectual for producing mineral concretions; and that, even were it capable of forming those mineral bodies, yet that, in the solid parts of this earth, formed by a deposit of travelled materials at the bottom of the sea, the conditions necessary to this incrustating process do not take place.

Those enlightened naturalists who have of late been employed in carefully examining the evidences of mineral operations, are often staggered in finding appearances inconsistent with the received doctrine of infiltration; they then have recourse to ingenious suppositions, in order to explain that enigma. In giving examples of this kind. I have in view both to represent the natural history these mineralists furnish us with, which is extremely interesting, and also to show the various shapes in which error will proceed, when ingenious men are obliged to reason without some necessary principle in their science. We have just now had an example in Europe; I will next present the reader with one from Asia.

M. Patrin, in his *Notice Mineralogique de la Daourie*, (Journal de Physique, Mars 1791) gives us a very distinct account of what he met with in that region. Describing the country of Doutchersk upon the river Argun, in Siberia, he proceeds thus:

“Ces colines sont formees d’un hornstein gris qui paroit se convertir en pierre calcaire par l’action des meteores; car tout celui qu’on prend hors du contact de l’air donne les plus vives etincelles, et ne fait pas la moindre effervescence avec les acides, meme apres avoir ete calcine; et l’on observe celui qui est a decouvert, passer, par nuances insensibles, jusqu’a l’etat de pierre calcaire parfaite de couleur blanchatre.”

Here M. Patrin has persuaded himself, probably from an imperfect examination of the subject, that there takes place a mineral metamorphosis, which certainly is not found in any other part of the earth, and for which he does not find any particular cause. The natural effect of the meteors, in other parts of the earth, is to dissolve the calcareous substance out of bodies exposed to those agents; and the gradation from the one of those two things to the other, which seems to be the data on which he had proceeded in forming his conclusion, is not sufficient to prove the metamorphosis, even were there not so strong a physical objection to it; for, it is by no means unusual for mineral bodies to graduate thus from one substance to another. However that be, this is not the principal object of the example[42].

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[Note 42: Here we have well informed naturalists reasoning with all the light of our present mineralogy, and maintaining, on the one hand, that gypsum is transformed into calcedony, by the operation of the meteors, or some such cause; and, on the other, that a siliceous substance is by the same means converted into lime-stone. What should we now conclude from this?—That calcareous and siliceous substances were mutually convertible. But then this is only in certain districts of Poland and Siberia. Every where, indeed, we find strange mixtures of calcareous and siliceous bodies; but neither mineralists nor chemists have, from these examples, ventured to affirm a metamorphosis, which might have spared them much difficulty in explaining those appearances.

This is a subject that may be taken in very different lights. In one view, no doubt, there would appear to be absurdity in the doctrine of metamorphosis, as there is now a days acknowledged to be in that of *lusus naturae*; and those reasoning mineralists might thus, in the opinion of some philosophers, expose their theory to contempt and ridicule. This is not the light in which I view the subject. I give those gentlemen credit for diligently observing nature; and I applaud them for having the merit to reason for themselves, which would seem to be the case with few of the many naturalists who now speak and write upon the subject.

Let us now draw an inference, with regard to this, in judging of the different theories. Either the received system concerning mineral operations is just, in which case those gentlemen, who employ a secret metamorphosis, may be to blame in laying it aside; or it is erroneous and deficient; and, in that case, they have the merit of distinguishing the error or deficiency of the prevailing system. How far they have seen the system of nature, in those examples which they have described, is another question. In the mean time, I am to avail myself of the testimony of those gentlemen of observation, by which the insufficiency at least of the received mineral system is acknowledged.]

After speculating upon the effect of the ancient ocean upon the mountains of that country, he proceeds as follows:

“Je laisse ces conjectures pour remarquer un fait singulier: la colline, qui est au nord de l’eglise de la fonderie, a son arrete composee de ce hornstein qui se decompose en pierre calcaire; mais ici, les parties, qui sont ainsi decomposees, offrent une substance calcedonieuse disposees par zones concentriques, comme on l’observe dans les agates d’oberstein; mais ce ne sont point ici des corps parasites formes par infiltration dans des cavites pre-existantes comme les agates; on voit que ce sont les parties constituantes de la roche qui, *par un travail interne*, et par une sorte de crystallisation, out pris cette disposition reguliere (que ce mot de *crystallisation* ne revolte point, j’appelle ainsi toute tendance a prendre une forme constante, polyedre ou non polyedre.) Les couches les plus voisine du centre sont nettes et distinctes; peu-a-peu elles le sont moins, et enfin elles s’evanouissent et se confondent avec le fond de la

roche. Chaque assemblage de ces zones a une forme ronde ou ovale plus ou moins reguliere de sept a huit pouces de diametre.

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“Cela ressemble en grand a ce qu’on observe dans les pierres oeillees, et la cause est vraisemblablement la meme. Je le repete, je regarde cette disposition reguliere comme une veritable cristallisation, qui peut s’operer et qui s’opere en effet dans l’interieur des corp les plus solide, tant qu’ils sont fournis a l’action des agens de la nature.

“Tous ceux qui visitent l’interieur de la terre savent que les roches memes le plus compactes y sont intimement penetrees d’humidite, et ce fluide n’est certainement pas l’eau pure; c’est l’agent qui opere toutes les agregations, toutes les cristallisations, tous les travaux de la nature dans le regne mineral. On peut donc aisement concevoir qu’a la faveur de ce fluide, il regne, dans les parties les plus intimes des corps souterrains, une circulation qui fait continuellement changer de place aux elemens de la matiere, jusqu’a ce que reunis par la force des affinites, les corpuscules similaires prennent la forme que la nature leur a assignee.”

Those nodular bodies or figured parts which are here inclosed in the rock, are evidently what may be called calcedony agates. M. Patrin is persuaded, from the examination of them, that they had not been formed in the manner of German agates, which he supposes is by mean of infiltration; and he has endeavoured to conceive another manner of operating, still however by means of water, which I suppose, according to this hypothesis, is to dissolve substances in one part, and deposits them in another, There must certainly be some great *desideratum* in that mineral philosophy which is obliged to have recourse to such violent suppositions. First, water is not an universal solvent, as it would require to be, upon this supposition; secondly, were water allowed to be an universal menstruum, here is to be established a circulation that does not naturally arise from the mixture of water and earth; and, lastly, were this circulation to be allowed, it would not explain the variety which is found in the consolidation and concretion of mineral bodies.

So long, therefore, as we are to explain natural appearances by reasoning from known principles, and not by ascribing those effects to preternatural causes, we cannot allow of this regular operation which M. Patrin alleges to be acting in the interior parts of the most solid bodies. This is indeed evident, that there has been a cause operating in the internal parts of the most solid bodies, a cause by which the elements, or constituent parts of those solid bodies, have been moved and regularly disposed, as this author very well observes must have been the case in our agates or eyed stones; but to ascribe to water this effect, or to employ either an ineffectual or an unknown cause, is not to reason philosophically with regard to the history of nature; it is to reason phantastically, and to imagine fable.

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M. Monnet has imagined a petrifying power in water very different from any that has hitherto been conceived, I believe, by natural philosophers, and I also believe, altogether inconsistent with experience or matter of fact; but as it is not without good reason that this naturalist has been induced to look out for a petrifying cause different from any hitherto supposed, and as he has endeavoured very properly to refute the systems of petrification hitherto received, I would beg leave to transcribe his reasoning upon the subject in corroboration of the present theory of consolidation by the means of fusion.

It is upon occasion of describing one of the species of alpine stone or schistus which contains quartzy particles. *Nouveau voyage mineralogique, etc.* Journal de Physique Aoust 1784.

“Il y a loin de cette pierre, que je regarde comme une variete de roches ardoisees, aux veritable ardoises. La composition de toutes ces pierres est due aux terres quartzeuses et argileuses, et a la terre talqueuse, que je demontrerai un jour etre une espece particuliere et distincte des autres, qui constitue les bonnes ardoises, et fait, ainsi que le quartz, qu’elles resistent aux injures de l’air, sans s’effleurir, comme je ferai voir que cette terre, qu’on designera sous la denomination de terre talqueuse, si l’on veut, resiste au grand feu sans se fondre. Les differences de toutes ces pierres, quoique composees des memes matieres, mais dans des proportions differentes, sont frappantes, et pourroient faire croire qu’elles n’appartiennent pas a ce genre. Mais qui ne voit ici que toutes ces differences, ou ces varietes, ne sont dues qu’aux modifications de la matiere premiere, qu’elle a eprouvees, soit en se melant avec des matieres heterogenes, prevenantes du debris des etres qui ont existe, comme l’argile, par exemple, qui, de l’aveu de presque tous les naturalistes, est le produit de l’organization des plantes, ou soit en se melant avec de la matiere deja solidifiee depuis long-temps? Or nous ne craignons pas de dire, ce que nous avons dit plusieurs fois quand l’occasion s’en est presentee, que cette matiere unique, que se modifie selon les occasions et les circonstances, et qui prend un caractere analogue au matieres qu’elle rencontre, est l’eau, que beaucoup de naturalistes cherchent vainement ailleurs. Ils ne peuvent comprendre, malgre les exemples frappans qui pourroient les porter a adopter cette opinion, que ce fluide general soit l’element des corps solides du regne mineral, comme il est de ceux du regne vegetal et du regne animal. L’on cherche serieusement, par des experiences chimiques, a decouvrir si l’eau est susceptible de se convertir en terre comme si la nature n’avoit pas d’autre moyen que nous de la faire passer de l’etat fluide a l’etat solide. Voyez le spath calcaire et le quartz transparens; est il a presumer qu’ils ne sont que le resultat du depot des matieres terreuses fait par les eaux? Mais, dans ce ca-la encore,

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il faut supposer que l’eau qui est restee entre ces partie s’est solidifiee; car, qu’est-elle donc devenue, et quel est donc le lien qui a uni ces parties et leur a fait prendre une forme reguliere? Il est vrai qu’on nous parle d’un suc lapidifique; mais c’est-la un etre de raison, dont il seroit bien plus difficile d’etablir l’existence, que de croire a la solidification de l’eau. On nous donne cependant comme un principe certain que l’eau charie d’un lieu a un autre les matieres qu’il a dissoutes, et qu’elle les depose a la maniere des sels. Mais c’est supposer une chose dementie par l’experience; savoir, que l’eau ait la propriete de dissoudre les matieres terreuses, telles que la quartzeuse. A la verite, M. Auchard de Berlin y joint de l’air fixe; mais cet air fixe ne sauroit tenir en dissolution un atome de quartz dans l’eau; et quelle qu’ait ete l’exactitude de ceux qui ont repete les experiences de M. Auchard, on n’a pu reussir a imiter la nature, c’est-a-dire, a former des cristaux quartzeux, comme il a annonce. Que l’eau ait la faculte de tenir en dissolution quelques petites parties de terre calcaire, au moyen de cet air fixe, il n’en faut pas conclure qu’elle puisse former de cette maniere tous les cristaux calcaires, sans que l’eau elle-meme y concoure pour sa part; car ce seroit conclure quelque fois que la partie seroit egale au tout. Voyez ces geodes calcaire et argileuses, qui renferment des cristaux nombreux de quartz ou de spath calcaire; ne sont ils que le resultat du depot de l’eau qui y a ete renfermee, ou que la cristallization pure et simple des molecules que vous supposez avoir ete tenues en dissolution par cette eau? Il naitroit de cette opinion une foule d’objections qu’il seroit impossible de resoudre. Cependant M. Guettard, dans la mineralogie du Dauphine, qui vient de paroitre, ouvrage tres-estimable a beaucoup d’egards, explique, selon cette maniere de penser, la formation de cristallizations quartzeuses qu’on trouve dans certaines geodes de cette province, et celle des mines de cristal des hautes montagnes. En supposant meme comme vraie l’explication qu’il en donne, on trouveroit en cela un des plus grands probleme, et des plus difficiles a resoudre qu’il y ait en mineralogie; car d’abord il faudroit expliquer comment un si petite quantite d’eau que celle qui a ete renfermee dans les geodes, et celle qui est parvenue dans les fentes des rochers, ont pu fournir un si grande quantite de matiere que celle qui constitue ces cristallisations, et ce qui n’est pas le moins difficile a concevoir, comment l’eau a pu charrier cette matiere a travers tant de matieres differentes, et la conserver precisement pour cette destination; comment, par exemple, l’eau est venue deposer de la terre quartzeuse dans les masses enormes de pierres calcaires, qui forment la cote qui domine le village de Champigny, a quatre lieues de Paris, au dela de Saint-maur; car s’il nous faut citer un exemple frappant de cette singularite, et a portee d’etre vue des naturalistes qui sont

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dans la capitale, je ne puis mieux faire que de citer cette cote, une des plus curieuses de la France, et que je me propose de fair connoitre en detail dans la troisieme partie de la mineralogie de la France. On verra, dis-je, dans cette bonne pierre a chaux, et une de plus pure des environs de Paris, de tres-abondantes cristallisations de quartz transparent, et quelque fois de belle eau, que les ouvriers sont forces de separer de la partie calcaire, a laquelle elles adherent fortement. Mais c'est trop nous arreter a combattre une opinion qui doit son origine aux premieres idees qu'ont eues les premiers observateurs en mineralogie, qui se detruira d'elle meme comme tant d'autres dont il nous reste a peine le souvenir."

We find here an accurate naturalist, and a diligent observer, who, in conformity with what my sentiments are upon the subject, thinks it impossible that the crystallizations in close cavities, and concretions of different solid substances within each other, which so frequently occur in the mineral regions, could have been produced, by means of solution and crystallization, from a fluid vehicle. But what has he now substituted in place of this solution, in order to explain appearances?—a mere supposition, *viz*. that nature may have the power of converting water, in those secret places, into some other thing; or rather that the substance of water is here converted into every other thing; for, though he has only mentioned quartz and calcareous spar, what mineral substance is there that may not be found in those close cavities? They are actually almost all, not even excepting gold; for, small grains of gold are inclosed within the cavities of a porous stone, in the Siberian mine. Now, for what purpose should nature, (to the power of which we are not to set a limit) have such an object in view as to convert water into every thing, unless it were to confound human understanding? For, so far as human experience has been as yet able to reach, there would appear to be certain elementary substances; and among these is water, or the principles of that fluid[43]. But because water is so generally found in bodies, and so necessarily in most of the operations of this world, why convert it into every other thing? Surely, for no better reason than that there has not occurred to this mineralist any other way of explaining certain natural appearances which aqueous solution could not produce. Here is no dispute about a matter of fact; it is on all hands allowed, that in certain cavities, inaccessible to any thing but heat and cold, we find mineral concretions, which contain no water, and which, according to the known operations of nature, water could not have produced; must we therefore have recourse to water acting according to no known principle, that is to say, are we to explain nature by a preternatural cause?

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[Note 43: Water is now considered by men of science, as a compound substance; this doctrine, which seems to follow so necessarily from the experiments of the French philosophers, must be tried by the growing light of chemical science. In the oxygenating operation of inflammable and combustible bodies when burning, those ingenious chemists overlooked the operation of *phlogistic matter*, which has no weight, and which escapes on that occasion, as I have had occasion to show in a dissertation upon phlogiston, and in the Philosophy of Light, Heat, and Fire. How far this view, which I have given of those interesting experiments, may lead to the explanation of other collateral phenomena, such as that of the water produced, I will not pretend to conjecture. One thing is evident, that if the weight of the water, procured in burning inflammable and vital air, be equal to that of those two gasses, we would then have reason to conclude, either that water were a compound substance, or that vital air, and inflammable vapour were compounds of water and the matter of light, or solar substance.]

I dare say that this is not the view that M. Monnet takes of the subject, when he thinks to explain to himself the concretion of those different substances by means of water; but, according to my apprehension of the matter, his theory, when sifted to the bottom, will bear no other construction; and, unless he shall consider water like the matter of heat, as capable of producing the fluidity of fusion, and of being also again abstracted from the fluid, by pervading the most solid body, which would then be a substance different from water, he must employ this aqueous substance as a menstruum or solvent for solid bodies, in the same manner as has been done by those naturalists whom he he justly censure, and conform to those erroneous ideas which first observations, or inaccurate knowledge of minerals, may have suggested to former naturalists.

It is the dissolution and concretion of siliceous substance, no doubt, that gives such difficulty to our naturalists in explaining petrifaction: they have, however, something apparently in their favour, which it may be proper now to mention.

In the *first* place, although siliceous substance is not soluble, so far as we know, by simple water, it is soluble by means of alkaline substance; consequently, it is possible that it may be dissolved in the earth.

Secondly, The water of Giezer in Iceland, actually petrifies bodies which are alternately imbibed with that hot water and exposed to the air. This water, therefore, not only contains siliceous substance in a dissolved state, but deposits this again, either by means of cooling, or being aerated, or of evaporating. Consequently, without knowing the principle upon which it proceeds, we here perceive a natural operation by which siliceous petrifaction may be performed.

Lastly, We have another principle for the dissolution of siliceous substance. This is the fluor acid which volatilises the siliceous substance. This, however, requires certain conditions, which cannot be found as a general cause in the mineral regions.

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Thus we would seem to have every thing necessary for explaining the concretion and crystallization of siliceous bodies, provided we could find the proper conditions requisite for that operation; for whether it shall be by means of acid or alkaline substances that siliceous matter is to be dissolved, volatilised, and transported from one place to another, it is necessary that those dissolving substances should be present upon those occasions. Nor is it sufficient only to dissolve the siliceous substance which is to be transported; the necessary conditions for the concretion again of the dissolved substances, whatever these may be, are also absolutely required for this operation. Now, though those requisite conditions may be, upon many occasions, allowed in the earth, it is not according to the theory of our modern naturalists, who explain petrifaction upon the principles of simple infiltration of water, that any advantage can be taken of those conditions; nor are natural appearances to be explained without employing more complicated chemical agents in the mineral regions.

To this subject of the petrifactions of Giezier, I may now add the information which we have received in consequence of a new voyage from this country to Iceland.

When Sir Joseph Banks returned from his expedition to Iceland, he landed at this place; and, having brought specimens of the petrifications of Giezer, Dr Black and I first discovered that these were of a siliceous substance. I have always conjectured that the water of Giezer must be impregnated with flinty matter by means of an alkaline substance, and so expressed my opinion in the Theory of the Earth published in the Transactions of the Edinburgh Royal Society. We have therefore been very desirous of procuring some of that water, in order to have it analysed.

An opportunity favourable to our views has occurred this summer. Mr Stanley set out from this place with the same purpose of examining Iceland. He was so good as to ask of Dr Black and I what inquiries we would incline that he should make. We have now, by the favour of this gentleman, obtained specimens of the petrifactions of Giezer; and, what is still more interesting, we have procured some of the water of those petrifying boiling springs.

It appears from these specimens, that the boiling water which is ejected from those aqueous volcanoes, if we may use the expression, is endued with the quality of forming two different species of petrifaction or incrustation; for, besides the siliceous bodies, of which we had before received specimens, the same stream of water incrustates its channel with a calcareous substance. All the specimens which I have seen consist of incrustation, some purely siliceous, some calcareous, and others mixed of those two, more or less.

Dr Black has been analysing the water; and he finds in it siliceous matter dissolved by an alkaline substance, in the manner of liquor silicum[44]. My conjecture has thus been verified.

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[Note 44: See Trans. of the Edin. Royal Society.]

It must not be alleged that nature may operate in the mineral regions, as she does here upon the surface in the case of Giezer. Such an argument as this, however sound it may be in general, will not apply to the subject of which we treat at present. There is no question about the limiting the powers of nature; we are only considering nature as operating in a certain determined manner, *viz*. by water acting simply upon the loose materials of the land deposited at the bottom of the sea, and accumulated in regular strata, one upon another, to the most enormous depth or thickness. This is the situation and condition of things in which nature is to operate; and we are to find the means of consolidating those strata, and concreting every species of substance in almost every possible composition, according to some known physical principle. Here is an operation which is limited; for, we must reason strictly, according to the laws of nature, in the case which we have under consideration; and we cannot suppose nature as ever transgressing those laws.

It is acknowledged, that, by means sometimes of an aeriform, sometimes of an alkaline, perhaps also of an acid substance, calcareous matter is dissolved in the earth, and certain metallic substances, such as lead and iron. This solution also, upon particular occasions, (where the proper conditions for separating the solvent from the dissolved substance exist), forms certain concretions; these are sometimes a mere incrustation, as in the case of the siliceous incrustation of Giezer, sometimes again in a crystallised or sparry form, as in the case of stalactical concretions. But here is no question of those cases where the proper conditions may be found; first, of dissolving the substance which is afterwards to be concreted; secondly, of separating the menstruum from the dissolved substance; and, lastly, of removing the fluid deprived of its solution, and of supplying a new solution in its room; the question is, how far those concretions are formed where those conditions do not take place. Now, this last case is that of almost all mineral concretions.

It must not be here alleged that certain concretions have been found in mines posterior to these having been worked by man; consequently, that those concretions have been formed by nothing but the infiltration of water. In those cases, where such concretions are truly found, I am persuaded that all the conditions proper to that operation will also be found; and it is only, I believe, in those cases where such proper conditions may be found, that this aqueous concretion ever appears. Now, if we shall except calcareous stalactite, and the bog ore of iron, How seldom is it that any appearance of those aqueous mineral concretion ever is found? Those very few cases in which they are found, afford the strongest proof against these being operations general to the globe, or proper mineral concretions; because it is only where all the necessary conditions conspire in each contributing its part, that the effect is accomplished; and this is a thing which cannot possibly take place in the aquiform strata below the surface of the sea. But, without attending to this clear distinction of things perfectly different, naturalists are apt to see false analogies, and thus in generalising to form the most erroneous theories.

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I shall now give an example of this fallaceous manner of reasoning; it is in the case of certain mineral appearances which are erroneously considered as stalactical concretions.

The only true stalactical bodies are of a calcareous substance; they are formed by water containing this substance in a dissolved state; and the principles upon which this particular concretion is formed are well known. It is therefore easy to compare other concretions, which may have some superficial resemblance to these stalactical bodies, in order to see if they have proceeded upon the same principle of concretion from a dissolved state, or by water depositing its dissolved substance in a similar manner.

There are two different mineral substances which give appearances of this sort. These are certain concretions of calcedony, and also of iron-ore, which are thought to have such resemblance to stalactical concretions as, by some superficial observers, to be reckoned of the same kind. It is now proposed to show that those conclusions are not well founded; and that, in this case of calcedony and iron-ore, it could not be upon the principle of stalactical concretion that the bodies now in question had their forms.

The principle upon which calcareous substance is dissolved in water, and made to concrete by the evaporation of the acid substance, or fixed air by which it had been dissolved, is too well known to require any explanation in this place; we are only to consider the sensible effects of those operations of which we know so well the proper conditions.

There are just two distinct views under which we may consider all stalactical concretions formed; these are the incrustation of the calcareous substance concreting upon a foreign body, and the incrustation of the same substance upon itself. By the first any manner of shape may be formed, provided there be a solid body, upon the surface of which the calcareous solution is made to pass. By the second, again, we have various forms; but we know the principles upon which they had been made. These are the shape and motions of the fluid which gives the calcareous concretion. Now, these principles are always to be perceived, more or less, in all the bizarre or fantastical, as well as regular shapes which are produced by stalactical concretions. At present, we shall confine our views to one particular shape, which is simple, regular, and perfectly understood wherever it is formed.

Drops of water falling from a roof, and forming stalactite, produce first tubular bodies, and then gradually consolidate and increase those pendulous bodies by incrustation. These appearances are thought to be observed in the calcedony and ferruginous concretions, which has led some mineralists to conclude, that those concretions had been formed in the same manner, by means of water. We are now to show that these mineral appearances are not analogous to stalactites in their formation, and that they have evidently been formed in a different manner.

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It must be evident, that, in the formation of those pendulous bodies, each distinct stalactite must be formed by a separate drop of water; consequently, that no more stalactites can be formed in a given space, than there could have subsisted separate drops of water. Now, a drop of water is a very determined thing; and thus we have a principle by which to judge of those mistaken appearances.

Let us suppose the gut of water to be but one eighth of an inch, although it is a great deal more, we should have no stalactites formed nearer to each other than that measure of space. But those mineral concretions, which are supposed to be stalactical, are contained in half that space, or are nearer to each other than the tenth or twentieth of an inch. I have them like needles, and in every degree of proximity or contiguity, at the same time that they are perfectly solid. Therefore, it is plainly impossible that they could have been formed upon this principle of calcareous stalactite. But, it is only by this false resemblance, that any argument can be formed for the concretion of those bodies from an aqueous solution; in every other respect they are true mineral concretions; and, that these have had a very different origin, has been already the subject of investigation, and will be more particularly examined in the course of this work.

The term *infiltration*, which has been much employed for explaining mineral appearances, is too vague, imperfect, or unexplicit, for science, whether as the means of knowing nature, or the subject of confutation. This is not the case with that of stalactite; here is a term that implies a certain natural operation, or a most distinct process for attaining a certain end; and we know the principles upon which it proceeds, as well as the several steps that may be traced in the general result. It is an operation which has not only been analysed to its principles; it is also a process which is performed by man, proceeding on his acquired knowledge. Now, were this operation common to the mineral regions, as it is proper to the surface of this earth; we could not remain in any degree of suspense with regard to the origin of those mineral bodies; for, having the true clue of knowledge, we should be able to unravel the most intricate and mysterious appearance. But, so far from this being the case, the more we come to inquire into nature, and employ this principle, the less we find it applicable, and the more involved in darkness is our science.

The places where these false appearances of stalactite are found, are precisely those in which, from the nature of things, all possibility for such an operation is excluded. For, How can this take place within a closs cavity in the mineral regions? The term *vegetation* may as well be employed for the explanation of those appearances: But what would now be said of such an explication? It is high time that science were properly applied to the natural history of this earth, and mineralists not allowed to impose upon themselves with false reasoning, or to please themselves with the vain attempt of explaining visible effects by unknown causes.

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Such various inconsistent opinions, respecting petrifaction or mineral concretion, as I have now exposed, opinions that are not founded on any sound physical principle, authorise me to conclude that they are all erroneous. If this be admitted, it will follow that we have no proof of any proper mineral concretion except that which had proceeded by congelation from the fluid state of fusion. This has been the doctrine which I have held out in my Theory of the Earth; and this will be more and more confirmed as we come to examine particular mineral appearances.

CHAP. VIII.

The Nature of Mineral Coal, and the Formation of Bituminous Strata, investigated.

SECT. I.—Purpose of this Inquiry.

In the first chapter, I have given a perfect mark by which to judge, of every consolidated stratum, how far that had been the operation or effect of water alone, or if it had been that of heat and fusion. This is the particular veins or divisions of the consolidated stratum, arising from the contraction of the mass, distended by heat, and contracted in cooling. It is not an argument of greater or lesser probability; it is a physical demonstration; but, so far as I see, it would appear to be for most mineralists an unintelligible proposition. Time, however, will open the eyes of men; science will some day find admittance into the cabinet of the curious. I will therefore now give another proof,—not of the consolidation of mineral bodies by means of fusion, for there is no mineral body in which that proof is not found,—but of the inconsistency of aqueous infiltration with the appearances of bodies, where not only fusion had been employed for the consolidation, but where the application of heat is necessary, and along with it the circumstances proper for *distillation*.

Short-sighted naturalists see springs of water issuing from the earth, one forming calcareous incrustations, the other depositing bituminous substances. Here is enough for them to make the theory of a world; on the one hand, solid marble is explained, on the other, solid coal. Ignorance suspects not error; their first step is to reason upon a false principle;—no matter, were they only to reason far enough, they would soon find their error by the absurdity into which it lands them. The misfortune is, they reason no farther; they have explained mineralogy by infiltration; and they content themselves with viewing the beautiful specimens in their cabinet, the supposed product of solution and crystalization. How shall we inform such observators; How reason with those who attend not to an argument!

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As naturalists have explained all mineral concretions from aqueous or other solution, and attributed to infiltration the formation of those stony bodies in which there are marks of their original composition, so have they explained to themselves, I suppose, the origin of those bituminous bodies which are found among the strata of the earth. In the case of stony substances, I have shown how unfounded all their theories are for the production of those concretions, crystallizations, and consolidated bodies. I am here to examine the subject of inflammable and combustible bodies, which I believe have been little considered by those theorists who suppose mineral bodies consolidated by infiltration. It is here that we shall find an infinite difference between the aqueous and igneous theories; for, we shall find it impossible to explain by the one certain operations which must have necessarily required the great agent generally employed in the other.

The subject of this chapter is a touch-stone for every theory of the earth. In every quarter of this globe, perhaps in every extensive country, bituminous strata are to be found; they are alternated with those which are called aquiform, or which had been evidently formed by subsidence of certain moved materials at the bottom of the sea; so far, therefore, all those strata have had the same origin. In this point I think I may assert, that all the different theories at present are agreed; and it is only concerning certain transformations of those strata, since their original collection, that have been ascribed to different causes.

Of these transformations, which the strata must have undergone, there are two kinds; one in relation to change of place and position; the other in relation to solidity or consistence. It is only the last of those two changes which is here to be the subject of consideration; because, with regard to the first, there is nothing peculiar in these bituminous strata to throw any light, in that respect, upon the others. This is not the case with regard to the transformation in their chemical character and consistence; bituminous bodies may not be affected by chemical agents, such as fire and water, in the same manner as the argillaceous, siliceous, micaceous, and such other strata that are alternated with the bituminous; and thus we may find the means for investigating the nature of that agent by which those strata in general have been transformed in their substance; or we may find means for the detecting of false theories which may have been formed with regard to those operations in which the original deposits of water had been changed.

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We have had but two theories, with regard to the transformation of those bodies which have had a known origin, or to the change of their substance and consistence; the one of these which I have given is that of heat or fusion; the other, which I wish to be compared with mine, is that of water and infiltration. It is by this last that all authors hitherto, in one shape or another, have endeavoured to explain the changes that those strata must have undergone since the time of their first formation at the bottom of the sea. They indiscriminately apply the doctrine of infiltration to those strata of mineral coal as to any other; they say that bituminous matter is infiltrated with the water, impregnates certain strata of earth with bituminous matter, and thus converts them into mineral coal, and bituminous strata. This is not reasoning physically, or by the inductive method of proceeding upon matter of fact; it is reasoning fantastically, or by making gratuitous supposition founded merely on imagination. It was thus that natural philosophers reasoned before the age of science; the wonder now is, how men of science, in the present enlightened age, should suffer such language of ignorance and credulity to pass uncensured.

The subject which I am now to treat of consists of peculiar strata of the earth, bodies which we may investigate through all the stages of their change, which is extreme; for, from vegetable bodies produced upon the habitable earth, they are now become a mineral body, and the most perfect coal,—a thing extremely different from what it had been, and a thing which cannot be supposed to have been accomplished by the operation of water alone, or any other agent in nature with which we are acquainted, except the action of fire or heat. It is therefore impossible for a philosopher, reasoning upon actual physical principles, not to acknowledge in this a complete proof of the theory which has been given, and a complete refutation of that aqueous operation which has been so inconsiderately supposed as consolidating the strata of the earth, and forming the various mineral concretions which are found in that great body.

To see this, it will be sufficient to trace the progress of vegetable and animal substances, (bodies which had certainly lived by means of a former earth), to this changed state in which they have become perfect mineral bodies, and constitute a part of the present earth. For, as these changes are perfectly explained by the one theory, and absolutely inconsistent with the other, there arises from this a conviction that must be irresistible to a person who can give proper attention to a chain of reasoning from effect to cause.

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But if we thus succeed to illustrate the theory of the earth by the natural history of those particular strata, we have but one step farther to make in order to bring all the other parts of the earth, whether stratified or not, into the most perfect consistence with the theory; now this step, it will be most easy to make; and I shall now mention it, that so the reader may keep it in his view: Pyrites is a sulphureo-metallic substance, which cannot be produced by means of water, a substance which the influences of the atmosphere decomposes or separates into its elements, and which even our imperfect art may be considered as able to produce, by means of fusion in our fires. Therefore, the finding of this creature of fire intimately connected with those consolidated strata of mineral coal, adds the greatest confirmation, were it necessary, to the doctrine of those mineral bodies having been consolidated by fusion. This confirmation, however, is not necessary, and it is not the only thing which I am at present to illustrate in that doctrine. What I have now in view is, to homologate the origin of those coal strata, with the production of every other mineral substance, by heat or fusion; and this is what the intimate connection of pyrites with those strata will certainly accomplish. This will be done in the following manner:

Pyrites is not only found in great masses along with the coal strata; it is contained in the veins which traverse those strata, and in the minute ramifications of those veins, which are occasioned by the contraction of the mass, and generally divide it into small cubical pieces; but besides that extrinsic connection, (as it may be called,) with the stratum of coal, pyrites is found intimately connected with that solid body, in being mixed with its substance. If, therefore, it were proved, that either the one or other of those two substances had been consolidated by fusion, the other must be acknowledged as having had the same origin; but now I am to prove, from the natural history of mineral coal, that pyrites had been there formed by fusion; and then, by means of the known origin of that sulphureo-metallic substance, we shall extend our knowledge to the origin of every other mineral body.

The process of this argument is as follows: Every mineral body, I believe, without exception, will be found so intimately connected with pyrites, that these two things must be concluded as having been together in a fluid state, and that, whatever may have been the cause of fluidity in the one, this must have also caused the fluidity in the other; consequently, whatever shall be proved with regard to the mineral operations of pyrites, must be considered as proved of every other mineral substance. But, from the connection of pyrites with mineral coal, it is to be proved that the origin of this metallic body had been fusion; and then it will appear, that all other mineral bodies must have been more or less in fusion, or that they must have been consolidated by means of heat, and not by any manner of solution or aqueous infiltration. I therefore now proceed to take a view of the natural history of coal strata,—a subject which mineralogists seem not inclined to engage with, although the most ample data are to be found for that investigation.

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SECT. II.—Natural History of Coal Strata, and Theory of this Geological Operation.

Fossil coal is the species of stratum best understood with regard to its accidents, as being much sought after; at least, this is the case in many parts of Britain, where it supplies the place of wood for burning. This fossil body has the most distinguished character; for, being inflammable or combustible in its nature, there is no other species of stratum that may be confounded with it.

But, though coal be thus the most distinguishable mineral, and that which is best understood in the science of mining, it is perhaps the most difficult to be treated of in the science of mineralogy; for, not having properly any distinguishable parts, we have nothing in the natural constitution of this body, as we have in most other strata, to lead us to the knowledge of its original state or first formation.

The varieties of coal are distinguished by their different manner of burning; but, from appearances of this kind, no perfect judgement can be formed with regard to the specific manner in which those strata had been made; although, from chemical principles, some conclusion may be drawn concerning certain changes which they have undergone since they had been formed.

Thus we have one species of coal which is extremely fusible, abounds with oil, and consequently is inflammable; we have another species again which is perfectly fixed and infusible in the fire; therefore, we may conclude upon principle, that, however, both those coals must have undergone the operation of heat and fusion, in bringing them to their present state, it is only the last that has become so much evaporated as to become perfectly fixed, or so perfectly distilled, as to have been reduced to a caput mortuum.

The argument here employed is founded upon this fact; that, from the fusible species of coal, a caput mortuum may be formed by distillation, and that this chemical production has every essential quality, or every peculiar property, of the fixed and infusible species; although, from the circumstances of our operation, this caput mortuum may not have precisely the exterior appearance of the natural coal. But, we have reason to believe, it is not in the nature of things to change the infusible species, so as to make it fusible or oily. Now, that this body was not formed originally in its present state, must appear from this, that the stratum here considered is perfectly solid; but, without fusion, this could not have been attained; and the coal is now supposed to be infusible. Consequently, this fixed substance, which is now, properly speaking, a perfect coal, had been originally an oily bituminous or fusible substance. It is now a fixed substance, and an infusible coal; therefore, it must have been by means of heat and distillation that it had been changed, from the original state in which this stratum had been formed.

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We have thus, in the examination of coal strata upon chemical principles, received a certain lesson in geology, although this does not form a proper distinction by which to specify those strata in general, or explain the variety of that mineral. For, in this manner, we could only distinguish properly two species of those strata; the one bituminous or inflammable; the other proper coal, burning without smoke or flame. Thus it will appear that, as this quality of being perfectly charred is not originally in the constitution of the stratum, but an accident to which some strata of every species may have been subjected, we could not class them by this property without confounding together strata which had differences in their composition or formation. Therefore, we are led to inquire after some other distinction, which may be general to strata of fossil coal, independent of those changes which this substance may have undergone after it had been formed in a stratum.

Perfect mineral coal being a body of undistinguishable parts, it is only in its resolution that we may analyse it, and this is done by burning. Thus, in analysing coal by burning, we have, in the ashes alone, that by which one species of coal may be distinguished from another; and, if we should consider pure coal as having no ashes of itself, we should then, in the weight of its ashes, have a measure of the purity of the coal, this being inversely as the quantity of the ashes. Now, though this be not accurately true, as the purest coal must have some ashes proper to itself, yet, as this is a small matter compared with the quantity of earthy matter that may be left in burning some species of coal, this method of analysis may be considered as not far removed from the truth.

But, in distinguishing fossil coal by this species of chemical analysis, not only is there to be found a perfect or indefinite gradation from a body which is perfectly combustible to one that is hardly combustible in any sensible degree, we should also fall into an inconveniency similar to that already mentioned, of confounding two things extremely different in their nature, a bituminous body, and a perfect charcoal. Thus, if we shall found our distinction upon the fusibility and different degree of having been charred, we shall confound fossil coals of very different degrees of value in burning, or of very different compositions as strata; if, again, we found it upon the purity of composition, in judging from the ashes, we shall confound fossil bodies of very different qualities, the one burning with much smoke and flame, the other without any; the one fusible almost like wax, the other fixed and infusible as charcoal.

It will now appear, that what cannot be done in either the one or other of those two methods, may in a great degree, or with considerable propriety, be performed in employing both.

Thus, whether for the economical purposes of life, or the natural history of fossil coal, those strata should be considered both with regard to the purity of their composition as inflammable matter deposited at the bottom of the sea, and to the changes which they have afterwards undergone by the operation of subterranean heat and distillation.

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We have now considered the original matter of which coal strata are composed to be of two kinds; the one pure bitumen or coal, as being perfectly inflammable or combustible; the other an earthy matter, with which proper coal may be variously mixed in its composition, or intimately connected, in subsiding from that suspended state by which it had been carried in the ocean. It is a matter of great importance, in the physiology of this globe, to know that the proper substance of coal may be thus mixed with heterogeneous bodies; for, supposing that this earthy matter, which has subsided in the water along with coal, be no farther connected with the combustible substance of those strata, than that it had floated in the waters of the ocean, and subsided *pari passu* with the proper materials of the coal, we hence learn a great deal with regard to the state in which the inflammable matter must have been at the time of its formation into strata. This will appear by considering, that we find schistus mixed with coal in the most equal or uniform manner, and in almost every conceivable degree, from the purest coal to the most perfect schistus. Hence we have reason to conclude, that, at the formation of those strata, the bituminous matter, highly subtilised, had been uniformly mixed with the earth subsiding in the water.

Not only is the bituminous matter of coal found mixed in every different proportion with the earthy or uninflammable materials of strata, but the coaly or bituminous composition is found with perhaps every different species of substance belonging to strata. This is certain, that we have the coaly matter intimately mixed with argillaceous and with calcareous strata.

Thus it will appear, that it is no proper explanation of the formation of coal strata, to say that vegetable matter is the basis of those strata; for though, in vegetation, a substance proper for the formation of bituminous matter is produced, it remains to know by what means, from a vegetable body, this bituminous matter is produced, and how it comes to be diffused in that subtile state by which it may be uniformly mixed with the most impalpable earth in water. Could we once resolve this question, every other appearance might be easily explained. Let us therefore now endeavour to discover a principle for the resolving of this problem.

There are two ways in which vegetable bodies may be, in part at least, resolved into that subtilised state of bituminous matter after which we inquire; the one of these is by means of fire, the other by water. We shall now consider these severally as the means of forming bituminous strata, although they may be both employed by nature in this work.

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When vegetable bodies are made to burn, there is always more or less of a fuliginous substance formed; but this fuliginous substance is no other than a bituminous body in that subtilised state in which it is indefinitely divided, and may be mixed uniformly with any mass of matter equally subtilised with itself. But this is precisely what we want, in order to compose the strata of coal in question. If, therefore, there were to be found in the ocean such a fund of this fuliginous substance as might suffice for the formation of bituminous strata, no difficulty would be left in explaining the original of fossil coal. But tho' sufficient quantity of this fuliginous matter might not be found for the explanation of natural appearances, yet there cannot be a doubt that more or less of this matter must be produced in the mineral operations of the globe, and be found precisely in that place where it is required for the forming of those strata of coal.

In order to conceive this, we are to consider, that there are actually great quantities of coal strata in a charred state, which indicates that all their more volatile oleaginous or fuliginous matter had been separated by force of subterranean heat; and, we are to suppose that this had been transacted at the bottom of the ocean: Consequently, a subtile oleaginous, bituminous, or fuliginous substance, must have been diffused in that ocean; and this bituminous matter would be employed in forming other strata, which were then deposited at the bottom of the waters.

But besides this quantity of bituminous matter which is necessarily formed in the mineral operations of the earth, and with regard to the quantity of which we can never form a proper estimate, there must enter into this same calculation all the fuliginous matter that is formed in burning bodies upon the surface of this earth. This bituminous matter of smoke is first delivered into the atmosphere, but ultimately it must be settled at the bottom of the sea. Hence though, compared with the quantity that we think required, each revolution of the globe produces but a little in our estimation, yet the progress of time, in reforming worlds, may produce all that is necessary in the formation of our strata.

There now remains to explain the other way in which bituminous matter may be obtained from vegetable bodies, that is, by means of water. For this purpose we must begin with a part of natural history that will throw some light upon the subject.

All the rivers in Scotland run into the sea tinged with a brown substance; this is most evident in some of them after a flood, and while yet the river is swelled; but, in travelling to the north of Scotland in the summer season, without any rain, I saw all the rivers, without exception, of a brown colour, compared with a river of more clear water. This colour proceeds from the moss water, as it is called, which runs into the rivers, or the infusion of that vegetable substance which forms combustible turf, called peat. Now, this moss water leaves, upon evaporation, a bituminous substance, which very much resembles fossil coal. Therefore, in order to employ this vegetable infusion, delivered into the ocean for the purpose of forming bituminous strata at its bottom, it is only required to make this bituminous matter separate and subside.

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If now we consider the immense quantity of inflammable vegetable substance, dissolved in water, that is carried into the sea by all the rivers of the earth, and the indefinite space of time during which those rivers have been pouring in that oily matter into the sea; and if we consider, that the continual action of the sun and atmosphere upon this oily substance tends, by inspissation, to make it more and more dense or bituminous, we cannot hesitate in supposing a continual separation of this bituminous matter or inspissated oil from the water, and a precipitation of it to the bottom of the sea. This argument is corroborated by considering, that, if it were otherwise, the water of the sea must have, during the immense time that rivers are proved to have run, be strongly impregnated with that oily or bituminous substance; but this does not appear; therefore we are to conclude, that there must be the means of separating that substance from the water in which it had been dissolved.

If there is thus, from the continual perishing of animal and vegetable bodies upon the surface of this earth and in the sea, a certain supply of oily or bituminous matter given to the ocean, then, however small a portion of this shall be supposed the whole oily or inflammable matter produced upon the surface of the earth, or however long time it may require for thus producing a stratum or considerable body of coal, we must still see in this a source of the materials proper for the production of that species of strata in the bottom of the sea.

We have now considered the proper materials of which pure fossil coal is chiefly formed; we have at present to consider what should be the appearances of such a substance as this collected at the bottom of the sea, and condensed or consolidated by compression and by heat. We should thus have a body of a most uniform structure, black, breaking with a polished surface, and more or less fusible in the fire, or burning with more or less smoke and flame, in proportion as it should be distilled or inspissated, less or more, by subterranean heat. But this is the description of our purest fossil coals, which burn in giving the greatest quantity of heat, and leave the smallest quantity of ashes.

In order to form another regular species of coal, let us suppose that, along with the bituminous substance now considered, there shall be floating in the water of the ocean a subtile earthy substance, and that these two different substances shall subside together in an uniform manner, to produce a stratum which shall be covered with immense weight, compressed, condensed, and consolidated as before, we should thus have produced a most homogeneous or uniform body to appearance, but not so in reality. The mixture of heterogeneous matter, in this case, is too minute to be discovered simply by inspection; it must require deep reflection upon the subject, with the help of chemical analysis, to understand the constitution of this body, and judge of all the circumstances or particulars in which it differs from the former. It is worth while to examine this subject with some attention, as it will give the most instructive view of the composition of bituminous strata, both those which are not considered as coal, and also the different species of that mineral body.

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In the first place then, if the mixture of those two different substances had been sufficiently perfect, and the precipitation uniform, the solid body of coal resulting from this mixture, would not only appear homogeneous, but might break equally or regularly in all directions; but the fracture of this coal must visibly differ from the former, so far as the fracture of this heterogeneous coal cannot have the polished surface of the pure bituminous body; for, the earthy matter that is interposed among the bituminous particles must affect the fracture in preventing its surface from being perfectly smooth. This imperfect plane of the fracture may be improved by polishing; in which case the body might be sufficiently smooth to have an agreeable polish; but it cannot have a perfect polish like a homogeneous body, or appear with that glassy surface which is naturally in the fracture of the pure bituminous coal.

But this is also a perfect description of that species of coal which is called in England Kennel coal, and in Scotland Parrot coal. It is so uniform in its substance that it is capable of being formed on the turning loom; and it receives a certain degree of polish, resembling bodies of jet.

Thus, we have a species of coal in which we shall find but a small degree of fusibility, although it may not be charred in any degree. Such an infusible coal may therefore contain a great deal of aqueous substance, and volatile oily matter; consequently may burn with smoke and flame. But this same species of coal may also occasionally be charred more or less by the operation of subterranean heat; and, in that case, we should have a variety of coal which could only be distinguished, from a similar state of pure bituminous coal, by the ashes which they leave in burning. At least, this must be the case, when both species are, by sufficient distillation, reduced to the state of what may be properly termed a chemical coal.

But in the natural state of its composition, we find those strata of kennel or parrot coal, possessing a peculiar property, which deserves to be considered, as still throwing more light upon the subject.

We have been representing these strata of coal as homogeneous to appearance, and as breaking indifferently in all directions; this last, perhaps, is not so accurate; for they would seem to break chiefly into two directions, that is, either parallel or perpendicular to the bed. Thus we have this coal commonly in rectangular pieces, in which it is extremely difficult to distinguish the direction of the bed, or stratification of the mass. By an expert eye, however, this may be in general, or at least sometimes, distinguished, and then, by knowing the habit of the coal in burning, a person perfectly ignorant of the philosophy of the matter may exhibit a wonderful sagacity, or even of power over future events, in applying this body to fire; for, at his pleasure, and unknown to those who are not in the secret; he may apparently, in equal circumstances, make this coal either kindle quietly, or with violent cracking and explosions, throwing its splinters at a distance.

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The explanation lies in this, that, though the rectangular mass of coal appears extremely uniform in its structure, it is truly a stratified mass; it is therefore affected, by the sudden approach of fire in a very different manner, according as the edge of the stratum, which is seen in four of the sides of this supposed cube, shall be applied to the fire, or the other two sides, which are in the line of the stratum, or parallel to the bed of coal. The reason of this phenomenon now remains to be considered.

When the edge of the coal is exposed to the fire, the stratification of the coal is opened gradually by the heat and expanding vapours, as a piece of wood, of a similar shape, would be by means of wedges placed in the end way of the timber. The coal then kindles quietly, and quickly flames, while the mass of this bituminous schistus is opening like the leaves of a book, and thus exhibits an appearance in burning extremely like wood. But let the fire be applied to the middle of the bed, instead of the edge of the leaves, and we shall see a very different appearance; for here the expanded aqueous vapours, confined between the *laminae*, form explosions, in throwing off splinters from the kindling mass; and this mass of coal takes fire with much noise and disturbance.

The ashes of this coal may be determined as to quality, being in general a subtile white earth; but, as to quantity, the measure of that earth produces an indefinite variety in this species of coal; for, from the kennel or parrot coal, which is valuable for its burning with much flame, to that black schistus which our masons use in drawing upon stone, and which, though combustible in some degree, is not thought to be a coal, there is a perfect gradation, in which coal may be found with every proportion of this earthy alloy.

Among the lowest species of this combustible schistus are those argillaceous strata in Yorkshire from whence they procure alum in burning great heaps of this stone, which also contains sulphur, to impregnate the aluminous earth with its acid. We have also, in this country, strata which differ from those aluminous schisti only in the nature of the earth, with which the bituminous sediment is mixed. In the strata now considered, the earth, precipitated with the bituminous matter, being calcareous, has produced a limestone, which, after burning especially, is perfectly fissile.

Therefore, with regard to the composition of mineral coal, the theory is this. That inflammable, vegetable, and animal substances, in a subtilised state, had subsided in the sea, being mixed more or less with argillaceous, calcareous, and other earthy substances in an impalpable state. Now, the chemical analysis of fossil coal justifies that theory; for, in the distillation of the inflammable or oily coal, we procure volatile alkali, as might be naturally expected.

Thus we have considered fossil coal as various, both in its state and composition; we have described coal which is of the purest composition, as well as that which is most impure or earthy; and we have shown that there is a gradation, from the most bituminous state in which those strata had been formed in being deposited at the

bottom of the sea, to the most perfect state of a chemical coal, to which they have been brought by the operation of subterranean fire or heat.

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We have been hitherto considering fossil coal as formed of the impalpable parts of inflammable bodies, united together by pressure, and made to approach in various degrees to the nature of a chemical coal, by means of subterranean heat; because, from the examination of those strata, many of them have evidently been formed in this manner. But vegetable bodies macerated in water, and then consolidated by compression, form a substance of the same kind, almost undistinguishable from some species of fossil coal. We have an example of this in our turf pits or peat mosses; when this vegetable substance has been compressed under a great load of earth, which sometimes happens, it is much consolidated, and hardens, by drying, into a black body, not afterwards dilutable or penetrated by water, and almost undistinguishable in burning from mineralised bodies of the same kind.

Also, when fossil wood has been condensed by compression and changed by the operation of heat, as it is frequently found in argillaceous strata, particularly in the aluminous rock upon the coast of Yorkshire, it becomes a jet almost undistinguishable from some species of fossil coal.

There cannot therefore be a doubt, that if this vegetable substance, which is formed by the collection of wood and plants in water upon the surface of the earth, were to be found in the place of fossil coal, and to undergo the mineral operations of the globe, it must at least augment the quantity of those strata, though it should not form distinct strata by itself.

It may perhaps be thought that vegetable bodies and their impalpable parts are things too far distant in the scale of magnitude to be supposed as subsiding together in the ocean; and this would certainly be a just observation with regard to any other species of bodies: But the nature of vegetable bodies is to be floatant in water; so that we may suppose them carried at any distance from the shore; consequently, the size of the body here makes no difference with regard to the place or order in which these are to be deposited.

The examination of fossil coal fully confirms those reasonable suppositions. For, *first*, The strata that attend coal, whether the sandstone or the argillaceous strata, commonly, almost universally, abound with the most distinct evidence of vegetable substances; this is the impressions of plants which are found in their composition. *Secondly*, There is much fossil coal, particularly that termed in England clod coal, and employed in the iron foundry, that shows abundance of vegetable bodies in its composition. The strata of this coal have many horizontal interstices, at which the more solid shining coal is easily separated; here the fibrous structure of the compressed vegetable bodies is extremely visible; and thus no manner of doubt remains, that at least a part of this coal had been composed of the vegetable bodies themselves, whatever may have been the origin of the more compact parts where nothing is to be distinguished.

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The state in which we often find fossil wood in strata gives reason to conclude that this body of vegetable production, in its condensed state, is in appearance undistinguishable from fossil coal, and may be also in great quantity; as, for example, the Bovey coal in Devonshire.

Thus the strata of fossil coal would appear to be formed by the subsidence of inflammable matter of every species at the bottom of the sea, in places distant from the shore, or where there had been much repose, and where the lightest and most floatant bodies have been deposited together. This is confirmed in examining those bodies of fossil coal; for, though there are often found beds of sand-stone immediately above and below the stratum of the coal, we do not find any sand mixed in the strata of the coal itself.

Having found the composition of coal to be various, but all included within certain rules which have been investigated, we may perceive in this an explanation of that diversity which is often observed among the various strata of one bed of coal. Even the most opposite species of composition may be found in the thickness of one bed, although of very little depth, that is to say, the purest bituminous coal may, in the same bed, be conjoined with that which is most earthy.

Fossil coal is commonly alternated with regular sand-stone and argillaceous strata; but these are very different bodies; therefore, it may perhaps be inquired how such different substances came to be deposited in the same place of the ocean. The answer to this is easy; we do not pretend to trace things from their original to the place in which they had been ultimately deposited at the bottom of the sea. It is enough that we find the substance of which we treat delivered into the sea, and regularly deposited at the bottom, after having been transported by the currents of the ocean. Now the currents of the ocean, however regular they may be for a certain period of time, and however long this period may be protracted, naturally change; and then the currents, which had given birth to one species of stratum in one place, will carry it to another; and the sediment which the moment before had formed a coal stratum, or a bed of that bituminous matter, may be succeeded either with the sediment of an argillaceous stratum, or covered over with a bed of sand, brought by the changed current of the sea.

We have now considered all the appearances of coal strata, so far as these depend upon the materials, and their original collection. But, as those bituminous strata have been changed in their substance by the operation of subterranean heat and inspissation, we are now to look for the necessary consequences of this change in the body of the stratum; and also for other mineral operations common to fossil coal with consolidated strata of whatever species.

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If coal, like other mineral strata, have been inspissated and consolidated by subterranean heat, we should find them traversed with veins and fissures; and, if the matter found in those veins and fissures corresponds to that found in similar places of other strata, every confirmation will be hence given to the theory that can be expected from the consideration of those bituminous strata. But this is the case; we find those fissures filled both with calcareous, gypseous, and pyritous substances. Therefore, we have reason to conclude, that the strata of fossil coal, like every other indurated or consolidated body in the earth, has been produced, *first*, by means of water preparing and collecting materials proper for the construction of land; and, *secondly*, by the operation of internal fire or subterranean heat melting and thus consolidating every known substance of the globe.

Not only are those sparry and pyritous substances, which are more natural to coal strata, found forming veins traversing those strata in various directions, but also every other mineral vein may occasionally be found pervading coal mines, or traversing bituminous strata. Gold, silver, copper, lead, calamine, have all, in this manner, been found in coal.

There remains now only to consider those bituminous strata of fossil coal in relation to that change of situation which has happened more or less to every stratum which we examine; but which is so much better known in those of coal, by having, from their great utility in the arts of life, become a subject for mining, and thus been traced in the earth at great expense, and for a long extent.

Coal strata, which had been originally in a horizontal position, are now found sometimes standing in an erect posture, even almost perpendicular to the plane in which they had been formed. Miners therefore distinguish coal strata according as they deem them to approach to the one or other of those two extremes, in terming them either flat or edge seams or veins. Thus, it will appear, that every possible change from the original position of those strata may have happened, and are daily found from our experience in those mines.

But besides the changed position of those strata, in departing from the horizontal line or flat position in which they had been formed, there is another remarkable change, termed by miners a *trouble* in the coal. The consideration of this change will further illustrate the operations of nature in placing that which had been at the bottom of the sea above its surface.

Strata, that are in one place regularly inclined, may be found bended, or irregularly inclined, in following their course. Here then is a source of irregularity which often materially effects the estimates of miners, judging from what they see, of those parts which are to be explored; and this is an accident which they frequently experience.

But, without any change in the general direction of the stratum, miners often find their coal broke off abruptly, those two parts being placed upon a higher and lower situation in respect to each other, if flat beds, or separated laterally if they are edge seams. This is by miners termed a *slip, hitch*, or *dyke*.

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These irregularities may either be attended with an injected body of subterraneous lava or basaltes, here termed whin-stone, or they may not be attended, at least apparently, *i.e.* immediately, with any such accident. But experienced miners know, that, in approaching to any of those injected masses of stone, which are so frequent in this country, their coal is more and more subject to be troubled.

As there is, in this country of Scotland, two different species of mountains or hills, one composed both in matter and manner exactly similar to the Alps of Switzerland, the other of whin-stone, basaltic rock, or subterraneous lava; and as the fossil coal, argillaceous and sand-stone strata, are found variously connected with those hills, nothing can tend more to give a proper understanding, with regard to the construction of the land in general, of the globe than a view of those different bodies, which are here found much mixed together in a little space of country, thus exhibiting, as it were in miniature, what may be found in other parts of the world, upon a larger scale, but not upon any other principle. I will therefore endeavour to give a short description of the mineral state of this country with regard to coal, so far as my experience and memory will serve.

This country might very properly be considered as consisting of primary and secondary mountains; not as supposing the primary mountains original and inexplicable in their formation, any more than those of the latest production, but as considering the one to be later in point of time, or posterior in the progress of things. The first are those which commonly form the alpine countries, consisting of various schisti, of quartzy stone, and granites. The second, again, are the whinstone or basaltic hills scattered up and down the low country, and evidently posterior to the strata of that country, which they break, elevate, and displace.

Thus there are in this country, as well as every where else, three things to be distinguished; first, the alpine or elevated country; secondly, the flat or low country; and, thirdly, that which has been of posterior formation to the strata which it traverses, in whatever shape or quality; whether as a mountain, or only as a vein; whether as a basaltes, a porphyry, or a granite, or only as a metal, a siliceous substance, or a spar.

Those three things which are here distinguished do not differ with regard to the chemical character of their substances; for, in each of these, every different substance is to be found, more or less; and it is not in being composed of materials peculiar to itself, that makes an alpine country be distinguished from a flat country; it is chiefly in the changes which the strata of the alpine country have been made to undergo, posterior to their original collection, that the rocks of the alpine country differ from those of the flat country.

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But the observation that is most to the purpose of the present subject of bituminous strata, is this; it is chiefly in the strata of the flat country that fossil coal are found; there are none that I know of in all the alpine countries of Scotland; and it is always among the strata peculiar to the flat country that fossil coal is found. Now, this appearance cannot be explained by saying that the materials of mineral coal had not existed in the world while those primary strata were formed in the sea. I have already shown, (chap. 4.) that there had been the same system of a world, producing plants, and thus maintaining animals, while the primary strata were formed in the sea; I have even adduced an example of coal strata among those primary schisti, although this be an extremely rare occurrence: Consequently, we are under the necessity of looking out for some other cause.

If the changes which have been evidently superinduced in the strata of alpine countries arise from the repeated operations of subterranean fire, or to the extreme degree in which those strata have been affected by this consolidating and elevating cause, it will be natural to suppose that the bituminous or combustible part among those stratifications, may have been mostly consumed upon some occasion during those various and long continued operations; whereas, in the flat beds of the low country, although there is the most perfect evidence for the exertion of heat in the consolidation of those strata, the general quantity of this has been a little thing, compared with the universal manifestation of this cause in the operations of the alpine countries, the strata of which have been so much displaced in their situations and positions.

To illustrate this, strata of sand-stone are found in both the alpine and flat countries of Scotland. About Leadhills, for example, there are abundance of those strata; but, in the flat country, the generality of the sand-stone is so little changed as to appear to every enlightened naturalist aquiform strata; whereas the most enlightened of those philosophers will not perhaps attribute the same original to a similar composition in the alpine country, which is so much changed from its original state. It is not because there had been wanting a sufficient degree of heat to consolidate the sand-stone in the coal country; for I can show specimens of sand-stone almost contiguous with coal, that have been extremely much consolidated in this manner. But this is only a particular stratum; and the general appearance of the sand-stone, as well as other strata in the coal countries, is that of having been little affected by those subterranean operations of heat by which those bodies in the alpine country have been changed in their structure, shape, and position.

If we shall thus allow the principle of consolidation, consequently also of induration, to have been much exerted upon the strata of the alpine country, and but moderately or little upon those of the low country of Scotland, we shall evidently see one reason, perhaps the only one, for the lesser elevation of the one country above the level of the sea, than the other. This is because the one resists the powers which have been employed in leveling what has been raised from the bottom of the sea, more than the

other; consequently, we find more of the one remaining above the level of the sea than of the other.

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Let us now take the map of Scotland, in order to observe the mixture of those two different species of countries, whereof the one is generally low and flat, the other high and mountainous; the one more or less provided with fossil coal, the other not.

From St Abb's Head, on the east of Scotland, to the Mull of Galloway, on the west, there runs a ridge of mountains of granite, quartz, and schistus strata, which contain not coal. On each side of this ridge we find coal countries; Northumberland, on the one side, and, on the other, the shires of Ayr, Lanark, and the Lothians; the one is a mountainous country, the others are comparatively low or flat countries. Let us now draw another alpine line from Buchan and Caithness, upon the east, to the island of Jura, on the west; this traverses a mountainous country destitute of coal, and, so far as I know, of any marks of marine bodies. But, on each side of this great alpine ridge, we find the hard country skirted with one which is lower, flatter, or of a softer nature, in which coal is found, upon the one side, in the shires of Fife, Clackmannan, and Stirling; and, on the other, in that hollow which runs from the Murray Frith south-west, in a straight line, directed upon the end of Mull, and composed, for the most part, of water very little above the level of the sea. Here, to be sure, the coal is scarce, or not so evident; but there is coal upon the sea coast in several places of this great Bay betwixt Buchan and Caithness; and the lowness of the country, across this part of the island, is almost sufficient testimony that it had been composed of softer materials.

Thus the coal country of Scotland may be considered as in one band across the island, and included in the counties of Ayr, Lanark, and all those which border upon the Frith of Forth. Now, in all this tract of coal and tender strata, we do not find ridges of alpine stone or primary mountains, but we find many hills of solid rock, little mountains, from 500 to 1000 feet high; such as that beautiful conical hill North Berwick Law, Torpender Law, Arthur's Seat, the Lowmands, and others of inferior note. That is to say, the whole of this included space, both sea and land, has been invaded from below with melted masses of whin-stone, breaking up through the natural strata of the country, and variously embossing the surface of the earth at present, when all the softer materials, with which those subterranean lavas had been covered, are washed away or removed from those summits of the country. Hence there is scarcely a considerable tubercle, with which this country also abounds, that may not be found containing a mass of whin-stone as a nucleus.

But besides those insulated masses of whinstone that form a gradation from a mountain to a single rock, such, for example, as that on which the Castle of Edinburgh is built, we find immense quantities of the same basaltic rock interjected among the natural strata, always breaking and disordering them, but often apparently following their directions for a considerable space with some regularity. We also find dykes of the same substance bisecting the strata like perpendicular veins of rock; and, in some places, we see the connection of these rocks of the same substance, which thus appear to be placed in such a different form in relation to the strata.

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It will thus appear, that the regular form, and horizontal direction of strata throughout this country of coal, now under contemplation, has been broken and disordered by the eruption and interjection of those masses of basaltic stone or subterraneous lava; and thus may be explained not only the disorders and irregularities of coal strata, but also the different qualities of this bituminous substance from its more natural state to that of a perfect coal or fixed infusible and combustible substance burning without smoke. This happens sometimes to a part of a coal stratum which approaches the whin-stone.

Having thus stated the case of combustible or bituminous strata, I would ask those naturalists, who adhere to the theory of infiltration and the operation of water alone, how they are to conceive those strata formed and consolidated. They must consider, that here are immense bodies of those combustible strata, under hundreds, perhaps thousands, of fathoms of sand-stone, iron-stone, argillaceous and calcareous strata. If they are to suppose bituminous bodies collected at the bottom of the sea, they must say from whence that bitumen had come; for, with regard to the strata below those bituminous bodies, above them, and between them, we see perfectly from whence had come the materials of which they are formed. They cannot say that it is from a collection of earthy matter which had been afterwards bituminized by infiltration; for, although we find many of those earthy strata variously impregnated with the bituminous and coaly matter, I have shown that the earthy and the bituminous matter had subsided together; besides, there are many of those coaly and bituminous strata in which there is no more than two or three *per cent.* of earthy matter or ashes after burning; therefore the strata must have been formed of bituminous matter, and not simply impregnated with it.

To avoid this difficulty, we shall allow them to form their strata, which certainly is the case in great part, by the collection of vegetable bodies; then, I desire them to say, in what manner they are to consolidate those bodies. If they shall allege that it is by simple pressure, How shall we conceive the numerous veins of spar and pyrites, which traverse those strata in all directions, to be formed in those bodies consolidated by the compression of the superincumbent masses?—Here is a manifest inconsistency, which proves that it could not be. But, even were we to suppose all those difficulties to be over come, there is still an impossibility in the way of that inconsiderate theory, and this will appear more fully in the following chapter.

SECT. III.—The Mineralogical Operations of the Earth illustrated from the Theory of Fossil Coal.

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There is not perhaps a greater difference among the various qualities of bodies than that which may be observed to subsist between the burning of those two substances, that is, the inflammable bodies on the one hand, and those that are combustible on the other. I have treated of that distinction in Dissertations upon subjects of Natural Philosophy, part 3d. where I have considered the different effects of those two kinds of bodies upon the incident light; and, in a Dissertation upon the Philosophy of Fire, *etc*. I have distinguished those two kinds of substances in relation to their emitting, in burning, the fixed light which had constituted a part of those inflammable and combustible bodies.

All animals and vegetable bodies contain both those different chemical substances united; and this phlogistic composition is an essential part in every animal and vegetable substance. There are to be found in those bodies particular substances, which abound more or less with one of those species of phlogistic matter, but never is the one species of those burning substances to be found naturally, in animal and vegetable bodies, without being associated with the other; and it is all that the chemical art can do to separate them in a great degree upon occasion. Pure ardent spirit may perhaps be considered as containing the one, and the most perfect coal the other; the chemical principle of the one is proper carbonic matter; and of the other it is the hydrogeneous principle, or that of inflammable air.

Thus we so far understand the composition of animal and vegetable substances which burn or maintain our fires; we also understand the chemical analysis of those bodies, in separating the inflammable from the combustible substance, or the volatile from the fixed matter, the oil from what is the proper coal. It is by distillation or evaporation, the effect of heat, that this separatory operation is performed; and we know no other means by which this may be done. Therefore, wherever we find peculiar effects of that separatory operation, we have a right to infer the proper cause.

The subject, which we are to consider in this section, is not the composition of strata in those of mineral coal, but the transformation of those, which had been originally inflammable bodies, into bodies which are only combustible, an end which is to be attained by the separation of their volatile or inflammable substances. In the last section, I have shown of what materials the strata of mineral coal had been originally formed; these are substances containing abundance of inflammable oil or bitumen, as well as carbonic matter which is properly combustible; and this is confirmed by the generality of those strata, which, though perfectly consolidated by fusion, retain still their inflammable and fusible qualities. But now the object of investigation is that mineral operation by which some of those strata, or some parts of a fusible and inflammable stratum, have been so changed as to become infusible and only combustible.

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We have now examined those strata which may be considered as either proper mineral coal, or as only a bituminous schistus; we are now to class along with these another species of this kind of matter, which has had a similar origin, although it may assume a different character.

According to the common observations of mankind, the eminent quality by which coal is to be distinguished, is the burning of that substance, or its capacity for making a fire. Therefore, however similar in other respects, a substance which had not that eminent quality of coal could hardly be considered as being allied to it; far less could it be supposed, as being in every other respect the same. We are however to endeavour to show, that there are truly substances of this kind, substances which to common observation, having none of the properties of coal with respect to fire, consequently, no utility for the purpose of burning, might be considered as another species of mineral, while at the same time they are truly at bottom a composition very little different from those which we have considered as the most perfect coal.

It must be recollected that we have distinguished coal in general as of two different species, one perfect or proper coal, containing no perceptible quantity of either oil or phlegm; the other as burning with smoke and flame, consequently containing both aqueous and oleaginous substances which it emits in distillation. It is the first of these which we are now to consider more particularly, in order to see the varieties which may be found in this species of mineral substance.

When that bituminous fossil, which is the common coal of this country, is submitted to heat it is subject to melt more or less, and emits smoke which is composed of water and oil. If it be thus completely distilled, it becomes a perfect coal of a porous or spongy texture. Such a substance as this is extremely rare among minerals; I have however found it. It is in the harbour of Ayr, where a whinstone dyke traverses the coal strata, and includes some of that substance in the state of coals or cinder. I pointed this out many years ago to Dr Black; and lately I showed it to Professor Playfair.

But the culm of South Wales, the Kilkenny coal of Ireland, and the blind coal of Scotland, notwithstanding that these are a perfect coal, or charred to a coal, have nothing of the porous construction of the specimen which I have just now mentioned; they are perfectly solid, and break with a smooth shining surface like those which emit smoke and flame.

Here is therefore a mineral operation in the preparation of those coals which we cannot imitate; and here is the clearest evidence of the operation of mineral fire or heat, although we are ignorant of the reason why some coal strata are charred, while others are not, and why, in some particular cases, the charred coal may be porous or spongy like our coals, while in general those blind coals (as they are called) are perfectly solid in their structure.

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But to what I would call more particularly the attention of mineral philosophers is this, that it is inconceivable to have this effect produced by means of water; we might as well say that heat were to be the cause of ice. The production of coal from vegetable bodies, in which that phlogistic substance is originally produced, or from animal bodies which have it from that source, is made by heat, and by no other means, so far as we know. But, even heat alone is not sufficient to effect that end, or make a perfect coal; the phlogistic body, which is naturally compound, consisting of both inflammable and combustible substances, must be separated chemically, and this must be the operation of heat under the proper circumstances for distillation or evaporation.

Here is the impossibility which in the last chapter I have alleged the aqueous theory has to struggle against; and here is one of the absolute proofs of the igneous theory. Not only must the aqueous part of those natural phlogistic bodies be evaporated, in order to their becoming coal, but the oily parts must also, by a still increased degree of heat, be evaporated, or separated by distillation from the combustible part. Here, therefore, is evidently the operation of heat, not simply that of fusion in contradiction to the fluidity of aqueous solution, but in opposition to any effect of water, as requiring the absence or separation of that aqueous substance.

But those natural appearances go still farther to confirm our theory, which, upon all occasions, considers the compression upon the bodies that are submitted to the operation of heat, in the mineral regions, as having the greatest efficacy in modifying that operation. Coal strata, which are in the neighbourhood of each other, being of those two opposite species, the one fusible and inflammable, the other infusible and combustible, afford the clearest proof of the efficacy of compression; for, it is evident, that the coal, which was once bituminous or fusible, cannot be charred without the distillation of that substance; therefore, prevent the distillation by compression and the charring operation cannot proceed, whatever should be the intensity of the heat; and then, fusion alone must be the effect upon the bituminous body. But now, as we have both those species of coal in the vicinity of each other, and even the same strata of coal part charred, while the rest is not, this natural appearance, so far from being a stumbling block, as it must be to the opposite theory, is most clearly explained by the partial escape of vapours from the mineral regions, and thus confirms the theory with regard to the efficacy of compression.

It is owing to the solidity of those natural charred coals, and the want of oil, that they are so very difficult of kindling; but, when once kindled in sufficient quantity, they make a fire which is very durable. There are even some of them which, to common observation, seem to be altogether incombustible. I have of this kind a specimen from a stratum at Stair, which shall be afterwards mentioned.

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M. Struve, in the Journal de Physique for January 1790, describes a mineral which he calls *plombagine charbonneuse ou hexaedre*; and gives for reason, *parce qu'elle ressemble extremement au charbon de pierre schisteux, ou d'hexaedre*. He says farther, "Il est tres commun, dans une roche qui forme un passage entre les granits et les breches, qu'on n'a trouvee jusqu'a present qu'on masses roulees dans le pays de Vaud." He concludes his paper thus: "Ce fossile singulier ne paroit pas appartenir a la Suisse seule. J'ai dans ce moment devant les yeux une substance parfaitement semblable, si on excepte la couleur qui tient le milieu entre le gris de fer et le rouge modere; elle vient du pays de Gotha de la Friedrischs-grube, proche d'Umneau. On le regarde comme un eisenrahm uni a du charbon de pierre."

The specimen which I have from Stair upon the water of Ayr, so far as I can understand, perfectly resembles this *plombagine* of M. Struve. It consumes very slowly in the fire, and deflagrates like plumbago with nitre. Now this comes from a regular coal stratum; and what is more remarkable, in this stratum is contained a true plumbago, Farther up the country, the Earl of Dumfries has also a mine containing plumbago along with other coal strata; and though the plumbago of these two mines have not all the softness and beauty of the mineral of the same species from Cumberland, they are nevertheless perfect plumbago.

I have a specimen of steatetical whinstone or basaltes from some part of Cumberland, in which is contained many nodules of the most perfect and beautiful plumbago. It is dispersed through this stone in rounded masses of all sizes from a nut to a pin's head; and many of these are mixed with pyrites. There is therefore reason to believe that this plumbago had been in fusion.

Now, if we consider that every species of coal and every species of plumbago are equally, that is, perfectly combustible, and yield, in burning, the same volatile principles, differing only perhaps a little in the small quantity of fixed matter which remains, we shall be inclined to believe, that they have all the same origin in a vegetable substance; and that they are diversified by some very small composition of other matter. This being allowed, one thing is certain, that it is by the operation of mineral fire or heat that those combustible substances, however composed, have been brought to their present state of coal, although we are ignorant of the circumstances by which their differences and their peculiar chemical and mechanical qualities have been produced.

Let us resume in a few words. There is not perhaps one substance in the mineral kingdom by which the operation of subterraneous heat is, to common understanding, better exemplified than that of mineral coal. Those strata are evidently a deposit of inflammable substances which all come originally from vegetable bodies. In this state of their formation, those coal strata must all be oleagenous or bituminous. In many of them, however, these volatile parts are found wanting; and, the stratum is found in the state of the most perfect coal or caput mortuum. There, is, I presume, no other means to be found by which this eminent effect could be produced, except by distillation; and,

this distillation perhaps proceeded under the restraining force of an immense compression.

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To this theory it must not be objected, that all the strata of coal, which are found in the same place or neighbourhood, are not reduced to that caput mortuum or perfect coaly state. The change from a bituminous to a coaly substance can only take place in proportion as the distillation of the volatile parts is permitted. Now this distillation must be permitted, if any passage can be procured from the inflammable body submitted to the operation of subterraneous heat; and, one stratum of coal may find vent for the passage of those vapours, through some crevice which is not open to another. In this way, doubtless, some of those bodies have been inspissated or reduced to a fixed coal, while others, at a little distance, have retained most of their volatile parts.

We cannot doubt of this distilling operation in the mineral regions, when we consider that in most places of the earth we find the evident effects of such distillation of oily substances in the naphta and petroleum that are constantly emitted along with water in certain springs. These oily substances are no other than such as may be procured, in a similar manner, from the fusible or inflammable coal strata; we have therefore every proof of this mineral operation that the nature of things admit of. We have also sufficient evidence that those fusible and inflammable coals, which have not been distilled to a caput mortuum, had been subjected to the operation of subterraneous heat, because we find those fusible coals subject to be injected with pyrites, as well as the more perfect coal.

If we now consider those various appearances of mineral bodies which are thus explained by the theory of mineral fire, or exertion of subterraneous heat, appearances which it is impossible to reconcile by any supposition of aqueous solution, or that unintelligible language of mineral infiltration which has of late prevailed, we shall be fully satisfied, that there is a uniform system in nature of providing a power in the mineral regions, for consolidating the loose materials deposited at the bottom of the sea, and for erecting those masses of mineralized substances into the place of land; we shall thus be led to admire the wisdom of nature, providing for the continuation of this living world, and employing those very means by which, in a more partial view of things, this beautiful structure of an inhabited earth seems to be necessarily going into destruction.

END OF VOLUME FIRST.