

XXII. *On the Consolidation of the Strata of the Earth.* By
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THE public attention, animated by scientific controversy, has of late years been much directed to Geological subjects; and the certainty of many important facts, has in consèquence been ascertained beyond dispute, which were formerly unknown, or at least involved in such obscurity, that no person could have ventured to assert them, without being charged with extravagance. But though, no doubt, many branches of this science still remain to be investigated, such inquiries may now be said to have acquired a considerable degree of consistency and interest, from the substantial basis upon which they have been found to rest.

Thus, in the present day, it is universally admitted, that a great part, I believe, in point of bulk, by far the greatest part, of the solid rock which constitutes the external mass of our globe, is stratified: that these strata, or at least a considerable portion of them, have at one period consisted of a loose assemblage of sand and gravel, broken from rocks of still higher antiquity: that these fragments are infinitely various in quality, in bulk, and in form; some retaining their original sharpness, others rounded and polished by agitation in the water: that these beds alternate with others of limestone, composed, in a great measure, of the shells of sea-fish, which shells are also occasionally scattered through the other strata. So that on the whole, it seems to be ascertained to the satisfaction of all par-

ties in geology, that the strata,—those, at least, of later formation, have once constituted collections of incoherent parts. And it is further admitted, that these beds have undergone various remarkable changes, some chemical, some mechanical.

The chemical changes consist in the consolidation of these loose assemblages into their present state of rock, passing, in that transition, through boundless varieties, in point of flexibility and toughness, and occasional brittleness. The mechanical revolutions are no less remarkable, principally in the change of the strata to their present contorted shape, and elevated position, often many thousand feet above the surface of the sea ; though there is full reason to believe that they all once lay in a horizontal position at its bottom.

I have said that the greatest part of the crust of our habitable globe seems unquestionably to be stratified, and produced from detritus or fragmented materials. The other portion, though probably the least in bulk, is, generally, the most conspicuous, owing to its durability, elevation, and picturesque beauty. This kind of rock is contrasted with the former class, particularly in its negative qualities ; in being, according to some geologists, altogether devoid of stratification in the general mass, and entirely free from component fragments ; the whole being made up of crystalline forms, moulded upon each other, in obedience to certain chemical laws.

This crystalline rock, as the Society are well aware, abounds in the neighbourhood of Edinburgh, in Arthur's Seat, Salisbury Craigs, and in Corstorphine Hill. It is decidedly posterior to the stratified class, of which it penetrates the crevices at all angles, in the form of dykes or veins, like stucco cast in a mould ; frequently also lodging between the strata in vast shapeless masses.

As the rock in question never fails to preserve this quality of universal and perfect crystallisation, I heartily concur with Dr HOPE in bestowing upon it the general name of *Crystallite*, un-

der which are comprehended all substances of this kind, including not only Whinstone and Basalt, but also Porphyry, Granite, and Sienite of every description.

The solid mass of our globe, then, in so far as it is naturally exposed to our view, or has been penetrated by the labours of the miner, would appear, (with the exception of some streams which have flowed from Vesuvius, Lipari, and other volcanoes, in which the rock possesses a glassy structure), to be comprehended under these two classes, Aggregates and Crystallites.

The whole of these rocks, of both classes, furnish, at every turn, proofs of their having undergone revolutions of the utmost magnitude ; and much ingenuity has been exerted, in endeavouring to trace these changes to some consistent and rational system. But of all the active powers of nature, one only has occurred to me as capable of affording a solution, in any degree satisfactory of the phænomena,—I mean the power of internal heat, which, in all ages, and in various countries, has made its appearance at the surface of the earth, not unfrequently from under the ocean, and which still, in our own days, gives occasional proofs of its unabated activity.

To ascertain the reality and sufficiency of this agent, and to trace the volcanic fire to its source, with tolerable probability, is, doubtless, an object of great interest and curiosity ; but it has always appeared to me, that the progress of geology was retarded by a premature anxiety to enter into such investigations.

Taking it for granted, however, as, indeed, no one can dispute, that there frequently do arise violent exertions of heat from under the bed of our ocean, Dr HUTTON held that this might furnish a rational and sufficient theory of the earth, without entering into any inquiry as to the origin of that heat ; and admitting that there are many geological facts which cannot be accounted for by such a fire as that of Vesuvius, now acting at the surface, in free communication with the air, he contended that

the case may be very different, where that same cause acts at the bottom of a deep sea, and under various modifying circumstances, by which its operation could not fail to be influenced.

This, indeed, constitutes the essence of the Huttonian Theory, which I learned principally in conversation with its illustrious author; and which, since his death, I have taken every means of submitting to a variety of chemical tests; being for ever on the watch for such natural scenes as might illustrate these principles, as well as for opportunities of making experiments, to determine whether such modifications on the action of heat were, or were not, sufficient to justify the expectations of Dr HUTTON.

It was in prosecution of these views that I formerly undertook a set of experiments, proving, I believe to the satisfaction of the scientific world, the identity of Whinstone and Lava, of which a full detail is given in your Transactions. In farther illustration of the same topic, my experiments on Carbonate of Lime were formerly undertaken, by which it was shewn, that calcareous matters, exposed to heat under pressure, might be fused; and, on cooling, would crystallise, so as in every respect to resemble marble. To these I beg leave likewise to refer the Society.

The immediate object of the paper I have now the honour of submitting to the Society—the consolidation of the strata—has been pursued in a similar spirit, and with similar views to those formerly announced. In making efforts to trace the modifications which the action of heat would undergo, when compelled to act under the influence of compression, or of other circumstances, all of which, in company, I have always been willing to distinguish by the name of *Plutonic*, (although the term was originally suggested, ironically, by one of our keenest antagonists, the late celebrated Dr KIRWAN), I was led to the particular topic of this paper, by an unexpected scene which presented itself in my own neighbourhood, in the country.

It had often been urged, and apparently with good reason, against this branch of the Huttonian Theory, that no amount of heat applied to loose sand, gravel, or shingle, would occasion the parts to consolidate into a compact stone. And as all my experience led to the same conclusion, I saw that, unless, along with heat, some flux were introduced amongst the materials, no agglutination of the particles would take place. The striking circumstance above alluded to, as occurring near Dunglass, and which will be particularly described presently, having suggested to me the idea that the salt of the ocean might possibly have been the agent in causing the requisite degree of fusion, I instituted a series of experiments, the details of which I am about to bring before the Society. By these, I conceive it will be shown, that this material, under various modifications, is fully adequate to explain the consolidation of the strata, and many other effects which we see on the surface of the Earth.

My success, from the first, was such as to promise the most satisfactory result, though it is only within the last year that I have been able to command the repetition of the experiments in a manner fit to be laid before this Society. This must be my apology to those who hear me, and to such of my friends as take an interest in these investigations, for having so long delayed the publication of a set of facts, some of which had presented themselves to my view many years ago.

Whoever, indeed, has had any experience in the prosecution of new subjects of experimental inquiry, knows that, owing to his ignorance of the requisite adjustment of the proportions of the ingredients, and of other similar arrangements, he must depend, in a great degree, upon chance for the success of his first results, and that he must often submit to spend much time and labour upon a subject, even after it has been made out to his own satisfaction, before he has acquired sufficient command over its details to answer for the result of any particular experi-

ment, so as to be able to produce it with confidence to the public.

It may be interesting, in the first place, to describe, in a general way, the geological structure of the country, in the neighbourhood of the singular scene which gave rise to these speculations.

On different occasions I have laid before this Society observations made on the rugged shore which occupies the southern entrance of our estuary the Firth of Forth, which, from being frequently washed by a very boisterous ocean, presents to view a distinct exhibition of its internal structure. The eastern part is occupied by the promontory of Fastcastle, composed entirely of the elder quality of strata, called by the Germans Grey Wacke. Further to the west it consists of cliffs formed of Sandstone, nearly in a horizontal position. These two meeting in the crag called the Siccar Point, afford the most distinct view we any where have of the peculiar relation and mutual history of these two rocks.

More inland, on the borders of Lammermuir, a set of horizontal beds occur, consisting of a loose assemblage of rounded stones, intermixed with sand and gravel, which bear every appearance of having been deposited by water, and which, as to their general history, seem to have undergone no change since the overwhelming, though transient, agitations of water, of which I have frequently had occasion to speak in this Society.

In the summer of 1812, as I was returning from visiting the granitic range which occurs in the water of Fasnet, in the hills of Lammermuir, and riding down the little valley of Aikengaw, which deeply indents this loose collection of gravel and shingle, about two miles above the village of Oldhamstocks, and at the distance of eight or ten miles from the sea, I was struck with astonishment on seeing one of these gravel banks, formed, as

above described, of perfectly loose materials, traversed vertically by a dyke, which, in its middle, consisted of whinstone, and was flanked by solid conglomerate; but this solidity abated gradually till the conglutination of the rounded masses diminishing by degrees, the state of loose shingle and gravel was entirely restored on both sides. The agglutinated mass adjacent to the dyke bore no resemblance to the result of calcareous petrification; scarcely ever gave effervescence with acid; and, by its gradual termination, differed from any whinstone-dyke I have seen to penetrate the strata; for, in the ordinary case, the termination of the crystallite against the adjoining aggregate through which it passes, is almost always quite abrupt.

About a hundred yards higher up the valley of Aikengaw, there occurs an agglutination similar to the last, though without any whin-dyke, and sufficiently strong to resist the elements, by which the surrounding matters had been washed away, leaving the pudding-stone, or agglutinated shingle, to stand up by itself, in a manner remarkable enough to have attracted the notice of the peasantry as something supernatural, since they have bestowed upon it the name of the Fairy's Castle.

Farther up the stream, other agglutinations occur frequently, as we could see in little narrow glens cutting through the mass; and higher still, they are so numerous as to meet and convert the whole into one unbroken mass of pudding-stone, occupying all that is exposed to view.

These very remarkable, and, to me at least, novel appearances, were the first which suggested the idea, that the consolidation not only of this class of conglomerates, but of sandstone in general, had been occasioned by the influence of some substance in a gaseous or aëriform state, driven by heat into the interstices between the loose particles of sand and gravel, where it had acted as a flux on the contiguous parts. On considering what this penetrating substance might be, and from whence it could

have come, the following circumstance presented itself to my recollection at the moment, and promised to afford some assistance to these conjectures.

A few miles lower down the valley in which the above facts were observed, at the distance of more than a mile from the sea, and between two and three hundred feet perpendicularly above it, there occurs a crag of sandstone, in which a numerous succession of strata are distinctly visible. Several of these beds have yielded much to the action of the air, and, in dry weather, exhibit a considerable white efflorescence, which has completely the taste of common salt; and so remarkable is this circumstance, that the rock has acquired, in the country, the name of Salt-Heugh.

Here, then, it immediately occurred to me, was probably the source of an abundant supply of the elastic substance or fumigator, whose action as a flux had been pointed out by the agglutinations in Aikengaw above described.

I conceived, that, if there were at the bottom of the sea a bed of sand and gravel, drenched with brine of full saturation, and that heat were applied to it from beneath according to Dr HUTTON's hypothesis, the first effect would be, to drive the water from the lowest portion of the sand, and to convert the salt which remained amongst it, together with the sand, into a dry cake. During this operation, or until the cake became quite dry, the absorption of latent heat would prevent the temperature from surpassing the boiling point of brine. But no sooner was this dryness accomplished, than, I imagined, the temperature of the mass would begin to rise above that pitch; the portion of it next the fire would gradually acquire a red-heat; that then the salt, being made by the heat in part to assume an elastic form, would be sent in fumes through the dry cake just described, and thus, by partially melting the contiguous particles, produce an agglutination.

Such being my theoretical views, no time was lost in submit-

ting them to the test of experiment. Taking it for granted that a quantity of sea-salt must frequently be formed and deposited, along with sand and gravel, at the bottom of the ocean (in the manner I shall have occasion to describe at another stage of this paper), where the water has been collected by its superior specific gravity, in the form of brine, I proceeded to make the following experiments.

Dry salt was placed along with sand, sometimes in a separate layer, at the bottom of the crucible, and sometimes mixed throughout the experiment : the whole was then exposed to heat from below. I found that the salt was invariably sent in fumes through the loose mass, and by its action produced solid stone in a manner completely satisfactory, as illustrative of the facts in Aikengaw ; and so as to give a good explanation of the production of sandstone in general.

These artificial stones are of various degrees of durability and hardness ;—some of them do not stand exposure to the elements, and crumble when immersed in water ;—some resist exposure for years ;—others are so soft as not to preserve their form for any length of time ;—while some bear to be dressed by the chisel ; and, it may be remarked generally, that, as far as the results of my experiments have been compared with natural sandstone, the same boundless variety exists in both cases. A striking instance of this resemblance occurs in the case of the Salt-Heugh, the sandstone of which, when immersed in water, crumbles down, exactly in the same manner as those results of my experiments which taste much of salt.

The fumes of the salt, no doubt, act, in all these cases, as a flux on the siliceous matter, and thus cement the adjacent particles together. The Society are, doubtless, well aware of the power of salt fumes in glazing pottery ; and the analogy, I conceive, is complete. It is the application alone that is new.

So far the results were satisfactory. But it next occurred, that it might be plausibly objected, that the presence of the superincumbent cool ocean, would interfere with the process, on

the principles of latent heat. To put this to the test, I proceeded to expose a quantity of sand, covered to the depth of several inches with common salt-water, to the heat of a furnace, and, as the liquid boiled away, replenished it from time to time by additions from the sea. Of course it gradually approached to a state of brine. But this proved a very tedious operation, requiring a continued ebullition, during three weeks without ceasing, before it became sufficiently saturated with salt by the discharge of the fresh-water; and I thought it much easier, and no less satisfactory, to employ brine from the first, formed at once by loading the water with as much salt as it could dissolve, amounting to about one-third of its weight.

The vessels employed in these early experiments, were the large black-lead crucibles used by the brass-founders. I filled the vessel, which was 18 inches high and 10 broad, nearly to the brim with brine of full saturation, the lower portion being occupied, to the depth of about 15 inches, with loose sand from the sea-shore, and thoroughly drenched with the brine. In order to have a view of the progress of the experiment, I placed an earthen-ware tube, about the size and shape of a gun-barrel, closed at bottom, and open at the top, in a vertical position, having its lower extremity immersed in the sand, and reaching to within about an inch of the bottom of the pot, while the other end rose a foot above the surface of the brine, and could be looked into without inconvenience.

After a great number of experiments, furnishing an unbounded variety of results, I at length obtained a confirmation of the main object in view. I observed that the bottom of the porcelain barrel, and of course the sand in which it rested, became red-hot, whilst the brine, which, during the experiment, had been constantly replenished from a separate vessel, continued merely in a state of ebullition: the upper portion of the sand, drenched with the liquid, remained permanently quite loose,

but the lower portion of the sand had formed itself into a solid cake.

On allowing the whole to cool, after it had been exposed to a high heat for many hours, and breaking up the mass, I was delighted to find the result, occupying the lower part of the pot, possessed of all the qualities of a perfect sandstone, as may be seen in the specimens now presented to the Society. Whenever the heat was not maintained so long, the sandstone which resulted was less perfect in its structure, tasted strongly of salt, and sometimes crumbled to sand when placed in water.

Many of these early experiments were accomplished with tolerable success. But still the result was somewhat precarious, and could not be announced with the confidence that I felt in presenting my former experiments to this Society.

The cause of this uncertainty I traced to the chemical operation of the salt, acting as a flux upon the porcelain vessels employed. This very action, I was well aware, was the main agent and cause of our success, when kept within proper bounds; but, on being allowed to pass those limits, and to act on the containing vessel as well as on the experiment, it destroyed the vessel, and converted the whole into a confused mass of slag.

After numberless unsuccessful attempts, and after returning again and again to the charge, with an interval sometimes of years, I at last met with a quality in some of the materials to me altogether unlooked for, by means of which may be obtained successful results, with scarcely any risk of failure.

I found that the action of the salt upon the substances of the crucibles of clay, did not exert itself in the same manner upon iron; but that a large vessel of cast-iron, 18 inches deep by 10 wide, and a common gun-barrel welded up at the breech, and open at the top, enabled me to work with the heat of melting gold, without injuring the vessels, and at any time to produce a perfect freestone; thus satisfying our theoretical expectations.

Similar results, in all respects, were produced by exposing pure pounded quartz to the action of the salt fumes,—and also when gravel, or any other mass of loose materials, was used instead of sand.

Having now shewn, in a satisfactory manner, that salt, whether in a dry state mixed along with loose materials, or driven in fumes through them, or applied in the state of brine, and exposed to heat, is a sufficient agent to produce a consolidation, such as we see in natural sandstones and other stratified rocks, it remains to be investigated, whether an adequate supply of this flux may be reckoned upon in nature.

It is well known that great diversity exists in the degree of saturation of the sea by salt, at different places; and BUFFON has been at much pains in collecting examples of this diversity in his geological volumes, introductory to his Natural History. It is known that, in many of the communications between sea and sea, a constant current sets one way, indicating that the evaporation from the sea, to which this stream flows, surpasses in quantity its supply of fresh-water from the rivers, rains, and springs. This is remarkably the case with the Mediterranean, into which a perpetual stream sets from the ocean, at the Gut of Gibraltar. We have reason, then, to conclude, both that the surface of the Mediterranean is lower than that of the ocean, and likewise that the quantity of salt in the former is perpetually on the increase; so that the specific gravity of the waters, and the intensity of their saturation, must be perpetually advancing to a state of brine. I am well aware, that an attempt has been made to render such a conclusion unnecessary, by the supposition of a counter-current flowing at the bottom, out of this great basin; but such suppositions are, in my opinion, altogether gratuitous.

What is here said of the Mediterranean, will apply no less to other seas, and even to the great oceans. And wherever a basin

occurs, in which a bottom of great depth is surrounded by a ridge comparatively shallow, we may expect to find the lower portion, at least, of the water in a state approaching to brine.

Without any such theoretical explanation of the manner in which a supply of salt is supposed to be formed, it may perhaps be considered sufficient for my purpose, to recal to the recollection of the Society, that there are in almost every part of the world vast districts of rock-salt, and in some countries extensive salt lakes and salt rivers; and in our own country we have many instances of brine springs, besides rock-salt in abundance.

Here then it seems to me, we are plentifully furnished with the means of accounting, in the manner experimentally shewn, for the agglutinations of such gravel as that of Aikengaw and for the strata of the Salt-Heugh, which, by an easy analogy, may be transferred to sandstone in general, and, perhaps, to stratified rocks of every description.

A member of this Society, however, well known by his scientific acuteness, alleged, first in his public lectures, and afterwards, upon my requesting an explanation of his objection, again repeated, that I was not justified in such theoretical conclusions, respecting the influence of heat at the bottom of the sea, since the neighbourhood of the cool water would necessarily counteract that influence.

In answer to this difficulty, I must beg leave to remark, that, in all my experiments above alluded to, the sand (viewed by means of the gun-barrel) was seen to become red-hot during the process of consolidation, while the superincumbent brine remained boiling above; and it was even found easy, by supplying cool brine in sufficient quantity, to maintain the temperature of the fluid permanently such, that the hand could be plunged into it at top, without injury, the sandstone below remaining all the while at a full red heat. But whenever I repeated this experiment,

with every circumstance the same, both as to duration and temperature, as in the example above detailed, but in which, instead of brine, *fresh* water was used, the result was very different. The lower part of the gun-barrel, immersed in the sand, and in which gold had melted in the brine experiment just mentioned, now remained permanently black and cold; and the whole of the sand in the pot, when removed from the furnace, fell out loose by its own weight; not the least trace of consolidation having taken place.

We may thus, I trust, presume to have added one more new and important modifying circumstance of heat, to those already advanced in support of the Huttonian doctrines; for, since it has been experimentally shewn, that heat, under the modifications produced by the presence of salt, as above described, is fully adequate to the consolidation of loose materials, exposed to its action, it may fairly be presumed, that salt has performed a part, and a very important part, in the consolidation of the strata of the globe.

I should be doing injustice to the subject, were I not to state, that, besides the views developed in the foregoing paper, and supported by actual experiment, many others have occurred to me, respecting the agency of salt under various modifications, and all bearing more or less directly upon the Huttonian Theory of the Earth. Some of these views have been submitted to the test of experiment, and the results, as far as they have yet been carried, give me great hopes of ultimate success. Others are still in the shape of mere conjecture; and none of them are yet in a state to lay before the Society in detail. A simple allusion to one or two of the most important of these views may probably be received with indulgence; and I shall be very happy if gentlemen possessed of adequate leisure, shall be induced to

follow up, by actual experiment, what I have thrown out as mere matter of speculation.

I conceive that salt, in the state of fumes, and urged by a powerful heat, possibly also modified by pressure, or perhaps combined with other substances, may have penetrated a great variety of rocks, acting as a flux on some, as in basalt, granite, &c. ; agglutinating others, as in the case of sandstone, pudding-stone, &c. ; softening others, as in the case of contorted strata of greywacke. In many cases, too, I conceive that these fumes may have had the power of carrying along with them various other materials, such as metals in a sublimed state, which would in this way be introduced into rents, veins, and cavities, or may even have entered into the solid mass of the rocks, which I imagine these fumes may have had power to penetrate. I have already tried some experiments in pursuit of these ideas. Salt, for instance, has been mixed with oxide of iron, reduced to fine powder, and then exposed to heat along with quartzose sand. The iron, I found, was borne up along with the salt fumes. The sandstone, formed in this way, was deeply stained with iron, and other most curious appearances presented themselves.

Every one who has seen a sandstone quarry, must have noticed evident traces of iron, the rock being stained in a great variety of ways ; sometimes in parallel layers,—sometimes in concentric circles, or rather in portions of concentric spheres, like the coats of an onion,—and, generally speaking, disposed in a way not accountable by deposition from water. All these appearances I would account for, by supposing the rock, either at the moment of its agglutination into sandstone, or at some subsequent period, to have been penetrated by the fumes of salt, charged with iron, also in a state of vapour.

I may mention one very curious result of my experiments with salt and iron, acting upon sand, namely, that, upon breaking up the specimen of artificial sandstone, an appearance often

presents itself of incipient crystallisation, if I may use this term ; a number of large, shining, parallel faces pervade the whole mass, and, by holding the specimen at the proper angle to the light, this appearance becomes very obvious. What the nature of these crystals is, I have not investigated ; but as they very much resemble what we see in different kinds of sandstone, I am of opinion that they hold out a fair expectation, of our being able to produce many of the crystalline appearances with which we are familiar in nature.

Common sea-salt, such as I have used, as is well known, is not pure muriate of soda ; and, in my experiments, I have mixed various other substances with it. In Nature, we must suppose that various contaminating substances would in like manner occur, to diversify the phenomena ; and, accordingly, we do find a boundless variety, in the aspect not only of sandstone, but of almost every kind of rock ; and I am by no means without expectation, that, in the course of time, we shall be able to imitate in our laboratory as many of these varieties as we choose to exhibit.

I have long been engaged also in a series of experiments on the formation of *Crystallites*, the name by which, as I have before stated, every crystallised rock might, perhaps, be usefully distinguished in contradistinction to *Aggregates*, or those formed of fragments. This great object in experimental geology, I hope to accomplish by means of an instrument which I have long had in use, for the regulation of high heats, a description of which may probably soon be laid before the Society, together with some further results in support of the Huttonian Theory of the Earth.