

Account of a series of experiments, shewing the effects of compression in modifying the action of heat. Read in the Royal Society of Edinburgh, June 3, 1805 / by Sir James Hall.

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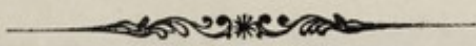


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S^r Clarke
from the author
3.

ACCOUNT
OF A
SERIES OF EXPERIMENTS,
SHEWING THE EFFECTS OF
COMPRESSION
IN
MODIFYING THE ACTION OF HEAT.

BY
SIR JAMES HALL, BART. F.R.S. EDIN.


READ IN THE ROYAL SOCIETY OF EDINBURGH, JUNE 3. 1805.

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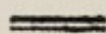
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I.

Ancient Revolutions of the Mineral Kingdom.—Vain attempts to explain them.—Dependence of Geology on Chemistry.—Importance of the Carbonate of Lime.—Dr BLACK's discovery of Carbonic Acid, subverted the former theories depending on Fire, but gave birth to that of Dr HUTTON.—Progress of the Author's Ideas with regard to that Theory.—Experiments with Heat and Compression, suggested to Dr HUTTON in 1790.—Undertaken by the Author in 1798.—Speculations on which his hopes of success were founded.

WHOEVER has attended to the structure of Rocks and Mountains, must be convinced, that our Globe has not always existed in its present state; but that every part of its mass, so far at least as our observations reach, has been agitated and subverted by the most violent revolutions.

FACTS leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for while discoveries
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of the utmost importance and accuracy were made in Astronomy and Natural Philosophy, the systems produced by the Geologists were so fanciful and puerile, as scarcely to deserve a serious refutation.

ONE principal cause of this failure, seems to have lain in the very imperfect state of Chemistry, which has only of late years begun to deserve the name of a science. While Chemistry was in its infancy, it was impossible that Geology should make any progress; since several of the most important circumstances to be accounted for by this latter science, are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes, are circumstances of a chemical nature, which all those who have attempted to frame theories of the earth have endeavoured by chemical reasonings to reconcile to their hypotheses.

FIRE and WATER, the only agents in nature by which stony substances are produced, under our observation, were employed by contending sects of geologists, to explain all the phenomena of the mineral kingdom.

BUT the known properties of Water, are quite repugnant to the belief of its universal influence, since a very great proportion of the substances under consideration are insoluble, or nearly so, in that fluid; and since, if they were all extremely soluble, the quantity of water which is known to exist, or that could possibly exist in our planet, would be far too small to accomplish the office assigned to it in the Neptunian theory*. On the other hand, the known properties of Fire are no less inadequate to the purpose; for, various substances which frequently occur in the mineral kingdom, seem, by their presence, to preclude

* *Illustrations of the Huttonian Theory*, by Mr Professor PLAYFAIR, § 430.

clude its supposed agency ; since experiment shews, that, in our fires, they are totally changed or destroyed.

UNDER such circumstances, the advocates of either element were enabled, very successfully, to refute the opinions of their adversaries, though they could but feebly defend their own : and, owing perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both systems maintained a certain degree of credit ; and writers on geology indulged themselves, with a sort of impunity, in a style of unphilosophical reasoning, which would not have been tolerated in other sciences.

OF all mineral substances, the *Carbonate of Lime* is unquestionably the most important in a general view. As limestone or marble, it constitutes a very considerable part of the solid mass of many countries ; and, in the form of veins and nodules of spar, pervades every species of stone. Its history is thus interwoven in such a manner with that of the mineral kingdom at large, that the fate of any geological theory must very much depend upon its successful application to the various conditions of this substance. But, till Dr BLACK, by his discovery of Carbonic Acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

THIS discovery was, in the first instance, hostile to the supposed action of fire ; for the decomposition of limestone by fire in every common kiln being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it.

THE contemplation of this difficulty led Dr HUTTON to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most inter-

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resting

resting problems that has ever engaged the attention of men of science.

HE supposed,

I. THAT Heat has acted, at some remote period, on all rocks.

II. THAT during the action of heat, all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

III. THAT in consequence of the combined action of Heat and Pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

THE essential and characteristic principle of his theory is thus comprised in the word *Compression*; and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

THIS system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own, that, on reading Dr HUTTON's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and perspicuity of whose conversation, formed a striking contrast to the obscurity

scurity of his writings. I was induced by that charm, and by the numerous original facts which his system had led him to observe, to listen to his arguments, in favour of opinions which I then looked upon as visionary. I thus derived from his conversation, the same advantage which the world has lately done from the publication of Mr PLAYFAIR's *Illustrations*; and, experienced the same influence which is now exerted by that work, on the minds of our most eminent men of science.

AFTER three years of almost daily warfare with Dr HUTTON, on the subject of his theory, I began to view his fundamental principles with less and less repugnance. There is a period, I believe, in all scientific investigations, when the conjectures of genius cease to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, against its improbability as an hypothesis: The partial view which we then obtain of truth, is perhaps the most attractive of any, and most powerfully stimulates the exertions of an active mind. The mist which obscured some objects dissipates by degrees, and allows them to appear in their true colours; at the same time, a distant prospect opens to our view, of scenes unsuspected before.

ENTERING now seriously into the train of reasoning followed by Dr HUTTON, I conceived that the chemical effects ascribed by him to compression, ought, in the first place, to be investigated; for, unless some good reason were given us for believing that heat would be modified by pressure, in the manner alleged, it would avail us little to know that they had acted together. He rested his belief of this influence on analogy; and on the satisfactory solution of all the phenomena, furnished by this supposition. It occurred to me, however, that this principle was susceptible of being established in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this proposal, on account of

the immensity of the natural agents, whose operations he supposed to lie far beyond the reach of our imitation; and he seemed to imagine, that any such attempt must undoubtedly fail, and thus throw discredit on opinions already sufficiently established, as he conceived, on other principles. I was far, however, from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian Theory. On the other hand, I considered myself as bound, in practice, to pay deference to his opinion, in a field which he had already so nobly occupied, and abstained, during the remainder of his life, from the prosecution of some experiments with compression, which I had begun in 1790.

IN 1798, I resumed the subject with eagerness, being still of opinion, that the chemical law which forms the basis of the Huttonian Theory, ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result.

IN the arrangement of the following paper, I shall first confine myself to the investigation of the chemical effects of Heat and Compression, reserving to the concluding part, the application of my results to Geology. I shall, then, appeal to the volcanoes, and shall endeavour to vindicate the laws of action assumed in the Huttonian Theory, by shewing, that lavas, previous to their eruptions, are subject to similar laws; and that the volcanoes, by their subterranean and submarine exertions,

tions, must produce, in our times, results similar to those ascribed, in that Theory, to the former action of fire.

IN comparing the Huttonian operations with those of the volcanoes, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr HUTTON's hypothesis: For, according to him, the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phenomena of nature.

My attention was almost exclusively confined to the Carbonate of Lime, about which I reasoned as follows: The carbonic acid, when uncombined with any other substance, exists naturally in a gaseous form, at the common temperature of our atmosphere; but when in union with lime, its volatility is repressed, in that same temperature, by the chemical force of the earthy substance, which retains it in a solid form. When the temperature is raised to a full red-heat, the acid acquires a volatility by which that force is overcome, it escapes from the lime, and assumes its gaseous form. It is evident, that were the attractive force of the lime increased, or the volatility of the acid diminished by any means, the compound would be enabled to bear a higher heat without decomposition, than it can in the present state of things. Now, pressure must produce an effect of this kind; for when a mechanical force opposes the expansion of the acid, its volatility must, to a certain degree, be diminished. Under pressure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us, what compressing force is requisite to enable it to resist any given elevation of temperature; and what is to be the result of such an operation. Some of the compounds of lime with acids
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are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

ONE circumstance, derived from the Huttonian Theory, induced me to hope, that the carbonate was easily fusible, and indicated a precise point, under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian Theory, that the whin and the spar had been liquid together; the two fluids keeping separate, like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove, that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shews, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization; as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, felspar, &c.; all of which having congealed in the liquid whin, have assumed their peculiar forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of WEDGWOOD, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew, that this conjecture was not without foundation.

I SHALL now enter upon the description of those experiments, the result of which I had the honour to lay before this Society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various

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ous particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only, as seem to lead to conclusions of importance.

THE result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations: in the eyes of such gentlemen, I trust, that none of the details into which I must enter, will appear superfluous.

II.

Principle of execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the muzzle.—Method with the Fusible Metal.—Remarkable effects of its expansion.—Necessity of introducing Air.—Results obtained.

WHEN I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device depends upon the

the following general view: If we take a hollow tube or barrel (AD, fig. 1.) closed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if necessary, exposed to extreme cold. If, then, the substance which we mean to subject to the combined action of heat and pressure, be introduced into the breech or closed end of the barrel (CD), and if the middle part be filled with some refractory substance, leaving a small empty space at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the subject of experiment, is kept cool, and thus close the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of sealing-wax. Things being then reversed, and the breech put into the furnace, a heat of any required intensity may be applied to the subject of experiment, now in a state of constraint.

My first application of this scheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very strongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previously rammed into a cartridge of paper or pasteboard, in order to protect it from the iron, by which, in some former trials, the subject of experiment had been contaminated throughout during the action of heat. I then rammed the rest of the barrel full of pounded clay, previously baked in a strong heat, and I had the muzzle closed like the breech, by a plug of iron welded upon it in a common forge; the rest of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then introduced horizontally into a common muffle, heated to about 25° of WEDGWOOD. To the muzzle a rope was fixed, in such a manner, that the barrel could be withdrawn without

out danger from an explosion*. I likewise, about this time, closed the muzzle of the barrel, by means of a plug, fixed by folder only; which method had this peculiar advantage, that I could shut and open the barrel, without having recourse to a workman. In these trials, though many barrels yielded to the expansive force, others resisted it, and afforded some results that were in the highest degree encouraging, and even satisfactory, could they have been obtained with certainty on repetition of the process. In many of them, chalk, or common limestone previously pulverised, was agglutinated into a stony mass, which required a smart blow of a hammer to break it, and felt under the knife like a common limestone; at the same time, the substance, when thrown into nitric acid, dissolved entirely with violent effervescence.

IN one of these experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been used to fill the barrel, was stained black throughout, to the distance of two-thirds of the length of the barrel from its breech. This circumstance is of importance, by shewing, that though all is tight at the muzzle, a protrusion may take place along the barrel, greatly to the detriment of complete

* ON one occasion, the importance of this precaution was strongly felt. Having inadvertently introduced a considerable quantity of moisture into a welded barrel, an explosion took place, before the heat had risen to redness, by which, part of the barrel was spread out to a flat plate, and the furnace was blown to pieces. Dr KENNEDY, who happened to be present on this occasion, observed, that notwithstanding this accident, the time might come when we should employ water in these experiments to assist the force of compression. I have since made great use of this valuable suggestion: but he scarcely lived, alas! to see its application; for my first success in this way, took place during his last illness.—I have been exposed to no risk in any other experiment with iron barrels; matters being so arranged, that the strain against them has only commenced in a red heat, in which the metal has been so far softened, as to yield by laceration like a piece of leather.

plete compression: and, at the same time, it illustrates what has happened occasionally in nature, where the bituminous matter seems to have been driven by superior local heat, from one part of a coaly bed, though retained in others, under the same compression. The bitumen so driven off being found, in other cases, to pervade and tinge beds of slate and of sandstone.

I WAS employed in this pursuit in spring 1800, when an event of importance interrupted my experiments for about a year. But I resumed them in March 1801, with many new plans of execution, and with considerable addition to my apparatus.

IN the course of my first trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemists, that a certain composition of different metals *, produces a substance so fusible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and dispatch, might be gained in these experiments, by substituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fusible metal, in a liquid state, might be poured in, so as to fill the barrel to its brim: That when the metal had cooled and become solid, the breech might, as before, be introduced into a muffle, and exposed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fusible metal being melted, but what lay at the breech, the rest, continuing in a solid state, would effectually confine the carbonic acid: That after the action of strong heat had ceased, and after all had been allowed to cool completely, the fusible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occasion any decomposition of the

* Eight parts of bismuth, five of lead, and three of tin.

the carbonate by calcination, though acting upon it in freedom ; and then, that the subject of experiment might, as before, be taken out of the barrel.

THIS scheme, with various modifications and additions, which practice has suggested, forms the basis of most of the following methods.

IN the first trial, a striking phenomenon occurred, which gave rise to the most important of these modifications. Having filled a gun-barrel with the fusible metal, without any carbonate ; and having placed the breech in a muffle, I was surprised to see, as the heat approached to redness, the liquid metal exuding through the iron in innumerable minute drops, dispersed all round the barrel. As the heat advanced, this exudation increased, till at last the metal flowed out in continued streams, and the barrel was quite destroyed. On several occasions of the same kind, the fusible metal, being forced through some very minute aperture in the barrel, spouted from it to the distance of several yards, depositing upon any substance opposed to the stream, a beautiful assemblage of fine wire, exactly in the form of wool. I immediately understood, that the phenomenon was produced by the superior expansion of the liquid over the solid metal, in consequence of which, the fusible metal was driven through the iron as water was driven through silver* by mechanical percussion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fusible metal a small quantity of air, which, by yielding a little to the expansion of the liquid, would save the barrel. This re-

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medy

* *Essays of Natural Experiments made in the Academie del Cimento*, translated by WALLER, London, 1684, page 117. The same in MUSSCHENBROEK's Latin translation, Lugd. Bat. 1731, p. 63.

medy was found to answer completely, and was applied, in all the experiments made at this time *.

I NOW proposed, in order to keep the carbonate clean, to inclose it in a small vessel; and to obviate the difficulty of removing the result at the conclusion of the experiment, I further proposed to connect that vessel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleasure.

A SMALL tube of glass †, or of Raumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2. A) was half filled with pounded carbonate of lime, rammed as hard as possible; the other half of the tube
being

* I found it a matter of much difficulty to ascertain the proper quantity of air which ought to be thus inclosed. When the quantity was too great, the result was injured by diminution of elasticity, as I shall have occasion fully to shew hereafter. When too small, or when, by any accident, the whole of this included air was allowed to escape, the barrel was destroyed.

I hoped to ascertain the bulk of air necessary to give liberty to the expansion of the liquid metal, by measuring the actual quantity expelled by known heats from an open barrel filled with it. But I was surprised to find, that the quantity thus discharged, exceeded in bulk that of the air which, in the same heats, I had confined along with the carbonate and fusible metal in many successful experiments. As the expansion of the liquid does not seem capable of sensible diminution by an opposing force, this fact can only be accounted for by a distention of the barrel. In these experiments, then, the expansive force of the carbonic acid, of the included air, and of the fusible metal, acted in combination against the barrel, and were yielded to in part by the distention of the barrel, and by the condensation of the included air. My object was to increase the force of this mutual action, by diminishing the quantity of air, and by other devices to be mentioned hereafter. Where so many forces were concerned, the laws of whose variations were unknown, much precision could not be expected, nor is it wonderful, that in attempting to carry the compressing force to the utmost, I should have destroyed barrels innumerable.

† I have since constantly used tubes of common porcelain, finding glass much too fusible for this purpose.

being filled with pounded filix, or with whatever occurred as most likely to prevent the intrusion of the fusible metal in its liquid and penetrating state. This tube so filled, was placed in a frame or cradle of iron (*d f k b*, figs. 3, 4, 5, and 6,) fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from six to three inches in length, and as much in diameter as a gun-barrel would admit with ease. It was composed of two circular plates of iron, (*d e f g* and *b i k l*, seen edge-wise in the figures,) placed at right-angles to the ram-rod, one of these plates (*d e f b*) being fixed to it by the centre (*m*). These plates were connected together by four ribs or flattened wires of iron (*d b*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrusting the adjacent ribs asunder. Along with the tube just mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewise, in the cradle, a pyrometer * piece (C) was placed in contact with (A) the tube containing the carbonate. These articles generally occupied the

* THE pyrometer-pieces used in these experiments were made under my own eye. Necessity compelled me to undertake this laborious and difficult work, in which I have already so far succeeded as to obtain a set of pieces, which, though far from complete, answer my purpose tolerably well. I had lately an opportunity of comparing my set with that of Mr WEDGWOOD, at various temperatures, in furnaces of great size and steadiness. The result has proved, that my pieces agree as well with each other as his, though with my set each temperature is indicated by a different degree of the scale. I have thus been enabled to construct a table, by which my observations have been corrected, so that the temperatures mentioned in this paper are such as would have been indicated by Mr WEDGWOOD's pieces. By Mr WEDGWOOD's pieces, I mean those of the only set which has been sold to the public, and by which the melting heat of pure silver is indicated at the 22d degree. I am well aware, that the late Mr WEDGWOOD, in his Table of Fusibilities, has stated that fusion was taking place at the 28th degree; but I am convinced that his observations must have been made with some set different from that which was afterwards sold,

the whole cradle ; when any space remained, it was filled up by a piece of chalk dressed for the purpose. (Fig. 4. represents the cradle filled, as just described).

THINGS being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fusible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, so that the carbonate was placed very near the breech, (as represented in fig. 5, the fusible metal standing at *o*). The air-tube (B) being placed so as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though some of it might be driven off by the heat, so as to escape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held fast. The barrel being then filled to the brim with fusible metal, the apparatus was ready for the application of heat to the breech, (as shewn in fig. 6.)

IN the experiments made at this time, I used a square brick furnace (figs. 7 and 8), having a muffle (*r s*) traversing it horizontally and open at both ends. This muffle being supported in the middle by a very slender prop, was exposed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hottest part, and the end next the muzzle projecting beyond the furnace, and surrounded with cloths which were drenched with water from time to time. (This arrangement is shewn in fig. 7). In this situation, the fusible metal surrounding the cradle being melted, the air contained in the air-tube would of course seek the highest position, and its first place in the air-tube would be occupied by fusible metal. (In fig. 6., the new position of the air is shewn at *p q*).

At the conclusion of the experiment, the metal was generally removed by placing the barrel in the transverse muffle, with its muzzle pointing a little downwards, and so that the heat was applied first to the muzzle, and then to the rest of the barrel in succession. (This operation is shewn in fig. 8). In some of the first of these experiments, I loosened the cradle, by plunging the barrel into heated brine, or a strong solution of muriate of lime; which last bears a temperature of 250° of FAHRENHEIT before it boils. For this purpose, I used a pan three inches in diameter, and three feet deep, having a flat basin at top to receive the liquid when it boiled over. The method answered, but was troublesome, and I laid it aside. I have had occasion, lately, however, to resume it in some experiments in which it was of consequence to open the barrel with the least possible heat*.

By these methods I made a great number of experiments, with results that were highly interesting in that stage of the business, though their importance is so much diminished by the subsequent progress of the investigation, that I think it proper to mention but very few of them.

On the 31st of March 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glass, and placed it in the cradle as above described. A pyrometer in the muffle along with the barrel indicated 33° . The barrel was exposed to heat during seventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one solid mass, which had visibly shrunk in bulk, the space thus left within the tube being accurately

* In many of the following experiments, lead was used in place of the fusible metal, and often with success; but I lost many good results in this way: for the heat required to liquefy the lead, approaches so near to redness, that it is difficult to disengage the cradle without applying a temperature by which the carbonate is injured. I have found it answer well, to surround the cradle and a few inches of the rod, with fusible metal, and to fill the rest of the barrel with lead.

accurately filled with metal, which plated the carbonate all over without penetrating it in the least, so that the metal was easily removed. The weight was reduced from forty to thirty-six grains. The substance was very hard, and resisted the knife better than any result of the kind previously obtained; its fracture was crystalline, bearing a resemblance to white saline marble; and its thin edges had a decided semitransparency, a circumstance first observed in this result.

ON the 3d of March of the same year, I made a similar experiment, in which a pyrometer-piece was placed within the barrel, and another in the muffle; they agreed in indicating 23° . The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have lost $3\frac{1}{2}$ grains. A thin rim, less than the 20th of an inch in thickness, of whitish matter, appeared on the outside of the mass. In other respects, the carbonate was in a very perfect state; it was of a yellowish colour, and had a decided semitransparency and saline fracture. But what renders this result of the greatest value, is, that on breaking the mass, a space of more than the tenth of an inch square, was found to be completely crystallized, having acquired the rhomboidal fracture of calcareous spar. It was white and opaque, and presented to the view three sets of parallel plates which are seen under three different angles. This substance, owing to partial calcination and subsequent absorption of moisture, had lost all appearance of its remarkable properties in some weeks after its production; but this appearance has since been restored, by a fresh fracture, and the specimen is now well preserved by being hermetically inclosed.

III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods used to confine the Carbonic Acid, and to close the Pores of the Porcelain in a Horizontal Apparatus.—Tubes made with a view to these Experiments.—The Vertical Apparatus adopted.—View of Results obtained, both in Iron and Porcelain.—The Formation of Limestone and Marble.—Inquiry into the Cause of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking.—Experiments with Porcelain Tubes proved to be limited.

WHILE I was carrying on the above-mentioned experiments, I was occasionally occupied with another set, in tubes of porcelain. So much, indeed, was I prepossessed in favour of this last mode, that I laid gun-barrels aside, and adhered to it during more than a year. The methods followed with this substance, differ widely from those already described, though founded on the same general principles.

I PROCURED from Mr WEDGWOOD's manufactory at Etruria, in Staffordshire, a set of tubes for this purpose, formed of the same substance with the white mortars, in common use, made there. These tubes were fourteen inches long, with a bore of half an inch diameter, and thickness of 0.2; being closed at one end (figs. 9, 10, 11, 12, 13.)

I PROPOSED to ram the carbonate of lime into the breech (Fig. 9. A); then filling the tube to within a small distance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the shops (borat of soda) previously reduced to glass, and then pounded; to apply heat to the muzzle alone, so as to convert that borax into solid glass; then, reversing the operation, to keep the muzzle cold, and apply the requisite heat to the carbonate lodged in the breech.

I THUS expected to confine the carbonic acid; but the attempt was attended with considerable difficulty, and has led to the employment of various devices, which I shall now shortly enumerate, as they occurred in the course of practice. The simple application of the principle was found insufficient, from two causes: First, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded filex, escaped from the compressing force, by lodging itself in cavities which were comparatively cold: Secondly, The glass of borax, on cooling, was always found to crack very much, so that its tightness could not be depended on.

To obviate both these inconveniences at once, it occurred to me, in addition to the first arrangement, to place some borax (fig. 10. C) so near the breech of the tube, as to undergo heat along with the carbonate (A); but interposing between this borax and the carbonate, a stratum of filex (B), in order to prevent contamination. I trusted that the borax in a liquid or viscid state, being thrust outwards by the expansion of the carbonic acid, would press against the filex beyond it (D), and totally prevent the elastic substances from escaping out of the tube, or even from wandering into its cold parts.

IN some respects, this plan answered to expectation. The glass of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viscid mass in the lowest red-heat; and as the stress in these experiments, begins only with redness, the borax being heated at the same time with the carbonate, becomes united and impervious, as soon as its action is necessary. Many good results were accordingly obtained in this way. But I found, in practice, that as the heat rose, the borax began to enter into too thin fusion, and was often lost among the pores of the filex, the space in which it had lain being found empty on breaking the tube. It was therefore

therefore found necessary to oppose something more substantial and compact, to the thin and penetrating quality of pure borax.

IN searching for some such substance, a curious property of bottle-glass occurred accidentally. Some of this glass, in powder, having been introduced into a muffle at the temperature of about 20° of WEDGWOOD; the powder, in the space of about a minute, entered into a state of viscid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged,) consolidated into a firm and compact mass of *Reaumur's porcelain* *. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this last might be effectually restrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and, along with it, the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass, and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as Reaumur's porcelain, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shewn in fig. 11.); and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice †. A still further refinement upon this me-

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* IN the same temperature, a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

thod was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, (as shewn in fig. 12.). During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube (*ab*); by this means, a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

I soon found, that notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil, in an air-pump. In this view, I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the inside of the large tube, and effectually closed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement just described being represented in fig. 13.)

I was thus enabled to carry on experiments with this porcelain, to the utmost that its strength would bear. But I was not satisfied with the force so exerted; and, hoping to obtain tubes of a superior quality, I spent much time in experiments with various porcelain compositions. In this, I so far succeeded, as to produce tubes by which the carbonic acid was in a great measure retained without any internal glaze. The best material I found for this purpose, was the pure porcelain-clay of Cornwall, or a composition in the proportion of two of this clay to one of what the potters call *Cornish-stone*, which I believe to be a granite in a state of decomposition. These tubes were seven or eight inches long, with a bore tapering

tapering from 1 inch to 0.6. Their thickness was about 0.3 at the breech, and tapered towards the muzzle to the thinness of a wafer.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By observing the thin state of borax whilst in fusion, I was convinced, that it ought to be treated as a complete liquid, which being supported in the course of the experiment from below, would secure perfect tightness, and obviate the failure which often happened in the horizontal position, from the falling of the borax to the lower side.

IN this view, (fig. 16.), I filled the breech in the manner described above, and introduced into the muzzle some borax (C) supported at the middle of the tube by a quantity of filix mixed with bottle-glass (B). I placed the tube, so prepared, with its breech plunged into a crucible filled with sand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilst the other remained cold. In that view, I constructed a furnace (fig. 14. and 15.), having a muffle placed vertically (*c d*), surrounded on all sides with fire (*ee*), and open both above (at *c*), and below (at *d*). The crucible just mentioned, with its tube, being then placed on a support directly below the vertical muffle, (as represented in fig. 14. at F), it was raised, so that the half of the tube next the muzzle was introduced into the fire. In consequence of this, the borax was seen from above to melt, and run down in the tube, the air contained in the powder escaping in the form of bubbles, till at last the borax stood with a clear and steady surface like that of water. Some of this salt being thrown in from above, by means of a tube of glass, the liquid surface was raised nearly to the muzzle, and, after all had been allowed to become cold, the position of the tube was reversed; the muzzle being now plunged

ged into the sand, (as in fig. 17.), and the breech introduced into the muffle. In several experiments, I found it answer well, to occupy great part of the space next the muzzle, with a rod of sand and clay previously baked, (fig. 19. K K), which was either introduced at first, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid state. In many cases I assisted the compactness of the tube by means of an internal glaze of borax; the carbonate being placed in a small tube, (as shewn in fig. 18.)

THESE devices answered the end proposed. Three-fourths of the tube next the muzzle was found completely filled with a mass, having a concave termination at both ends, (*f* and *g* figs. 17. 18. 19.), shewing that it had stood as a liquid in the two opposite positions in which heat had been applied to it. So great a degree of tightness indeed was obtained in this way, that I found myself subjected to an unforeseen source of failure. A number of the tubes failed, not by explosion, but by the formation of a minute longitudinal fissure at the breech, through which the borax and carbonic acid escaped. I saw that this arose from the expansion of the borax when in a liquid state, as happened with the fusible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of some force acting very powerfully, and to a very small distance. Accordingly, this source of failure was remedied by the introduction of a very small air-tube. This, however, was used only in a few experiments.

IN the course of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above described, amounting, together with those made in gun-barrels, to one hundred and fifty-six. In an operation so new, and in which the apparatus was strained to the utmost of its power, constant success could not be expected, and in fact many experiments failed, wholly or partially. The results, however, upon the

the whole, were satisfactory, since they seemed to establish some of the essential points of this inquiry.

THESE experiments prove, that, by mechanical constraint, the carbonate of lime can be made to undergo strong heat, without calcination, and to retain almost the whole of its carbonic acid, which, in an open fire, at the same temperature, would have been entirely driven off: and that, in these circumstances, heat produces some of the identical effects ascribed to it in the Huttonian Theory.

By this joint action of heat and pressure, the carbonate of lime which had been introduced in the state of the finest powder, is agglutinated into a firm mass, possessing a degree of hardness, compactness, and specific gravity *, nearly approaching to these qualities in a sound limestone; and some of the results, by their saline fracture, by their semitransparency, and their susceptibility of polish, deserve the name of marble.

THE same trials have been made with all calcareous substances; with chalk, common limestone, marble, spar, and the shells of fish. All have shewn the same general property, with some varieties as to temperature. Thus, I found, that, in the same circumstances, chalk was more susceptible of agglutination than spar; the latter requiring a heat two degrees higher than the former, to bring it to the same pitch of agglutination.

THE chalk used in my first experiments, always assumed the character of a yellow marble, owing probably to some slight contamination of iron. When a solid piece of chalk, whose bulk had been previously measured in the gage of Wedgwood's pyrometer was submitted to heat under compression, its contraction was remarkable, proving the approach of the particles during their consolidation; on these occasions, it was found

to

* See Appendix.

to shrink three times more than the pyrometer-pieces in the same temperature. It lost, too, almost entirely, its power of imbibing water, and acquired a great additional specific gravity. On several occasions, I observed, that masses of chalk, which, before the experiment, had shewn one uniform character of whiteness, assumed a stratified appearance, indicated by a series of parallel layers of a brown colour. This circumstance may hereafter throw light on the geological history of this extraordinary substance.

I HAVE said, that, by mechanical constraint, almost the whole of the carbonic acid was retained. And, in truth, at this period, some loss of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumstance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

WHEN the loss exceeded 10 or 15 *per cent* *. of the weight of the carbonate, the result was always of a friable texture, and without any stony character; when less than 2 or 3 *per cent*. it was considered as good, and possessed the properties of a natural carbonate. In the intermediate cases, when the loss amounted, for instance, to 6 or 8 *per cent*., the result was sometimes excellent at first, the substance bearing every appearance of soundness, and often possessing a high character of crystallization; but it was unable to resist the action of the air; and, by attracting carbonic acid or moisture, or both, crumbled to dust more or less rapidly, according to circumstances. This seems to prove, that the carbonate of lime, though not fully saturated with carbonic acid, may possess the properties of limestone; and perhaps a difference of this

* I have found, that, in open fire, the entire loss sustained by the carbonate varies in different kinds from 42 to 45.5 *per cent*.

this kind may exist among natural carbonates, give rise to their different degrees of durability.

I HAVE observed, in many cases, that the calcination has reached only to a certain depth into the mass; the internal part remaining in a state of complete carbonate, and, in general, of a very fine quality. The partial calcination seems thus to take place in two different modes. By one, a small proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the rest is left entire. Perhaps one result is the effect of a feeble calcining cause, acting during a long time, and the other of a strong cause, acting for a short time.

SOME of the results which seemed the most perfect when first produced, have been subject to decay, owing to partial calcination. It happened, in some degree, to the beautiful specimen produced on the 3d of March 1801, though a fresh fracture has restored it.

A SPECIMEN, too, of marble, formed from pounded spar, on 15th May 1801, was so complete as to deceive the workman employed to polish it, who declared, that, were the substance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to dust.

NUMBERLESS specimens, however, have been obtained, which resist the air, and retain their polish as well as any marble. Some of them continue in a perfect state, though they have been kept without any precaution during four or five years. That set, in particular, remain perfectly entire, which were shewn last year in this Society, though some of them were made in 1799, some in 1801 and 1802, and though the first eleven were long soaked in water, in the trials made of their specific gravity.

A CURIOUS circumstance occurred in one of these experiments, which may hereafter lead to important consequences. Some rust of iron had accidentally found its way into the tube: 10 grains of carbonate were used, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had escaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black flaggy matter, and partly by sphericles of various sizes, from that of a small pea downwards, of a white substance, which proved to be quicklime; the sphericles being interspersed through the flag, as spar and agates appear in whinstone. The flag had certainly been produced by a mixture of the iron with the substance of the tube; and the spherical form of the quicklime seems to shew, that the carbonate had been in fusion along with the flag, and that they had separated on the escape of the carbonic acid.

THE subject was carried thus far in 1803, when I should probably have published my experiments, had I not been induced to prosecute the inquiry by certain indications, and accidental results, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was possible to establish, by experiment, the truth of all that was hypothetically assumed in the Huttonian Theory.

THE principal object was now to accomplish the entire fusion of the carbonate, and to obtain spar as the result of that fusion, in imitation of what we conceive to have taken place in nature.

IT was likewise important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itself, and on account of its consequences; the result being visibly improved by every approach towards complete saturation. I therefore became anxious to investigate the cause of the partial calcinations which had always taken place, to

a greater or a less degree, in all these experiments. The question naturally suggests itself, What has become of the carbonic acid, separated in these partial calcinations from the earthy basis? Has it penetrated the vessel, and escaped entirely, or has it been retained within it in a gaseous, but highly compressed state? It occurred to me, that this question might be easily resolved, by weighing the vessel before and after the action of heat upon the carbonate.

WITH iron, a constant and inappreciable source of irregularity existed in the oxidation of the barrel. But with porcelain the thing was easy; and I put it in practice in all my experiments with this material, which were made after the question had occurred to me. The tube was weighed as soon as its muzzle was closed, and again, after the breech had been exposed to the fire; taking care, in both cases, to allow all to cool. In every case, I found some loss of weight, proving, that even in the best experiments, the tubes were penetrated to a certain degree. I next wished to try if any of the carbonic acid separated, remained within the tube in a gaseous form; and in that view, I wrapt the tube, which had just been weighed, in a sheet of paper, and placed it, so surrounded, on the scale of the balance. As soon as its weight was ascertained, I broke the tube by a smart blow, and then replaced upon the scale the paper containing all the fragments. In those experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already escaped during the action of heat. But in the good results, I always found that a loss of weight was the consequence of breaking the tube.

THESE facts prove, that both causes of calcination had operated in the porcelain tubes; that, in the cases of small loss, part of the carbonic acid had escaped through the vessel, and that part had been retained within it. I had in view methods

by which the last could be counteracted ; but I saw no remedy for the first. I began, therefore, to despair of ultimate success with tubes of porcelain *.

ANOTHER circumstance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to these tubes, when charged as above with carbonate, without destroying them, either by explosion, by the formation of a minute rent, or by the actual swelling of the tube. Sometimes this swelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate sustaining but a very small loss. This ductility of the porcelain in a low heat is a curious fact, and shews what a range of temperature is embraced by the gradual transition of some substances from a solid to a liquid state : For the same porcelain, which is thus susceptible of being stretched out without breaking in a heat of 27° , stands the heat of 152° , without injury, when exposed to no violence, the angles of its fracture remaining sharp and entire.

IV.

* I am nevertheless of opinion, that, in some situations, experiments with compression may be carried on with great ease and advantage in such tubes. I allude to the situation of the geologists of France and Germany, who may easily procure, from their own manufactories, tubes of a quality far superior to any thing made for sale in this country.

IV.

Experiments in Gun-Barrels resumed.—The Vertical Apparatus applied to them.—Barrels bored in solid Bars.—Old Sable Iron.—Fusion of the Carbonate of Lime.—Its action on Porcelain.—Additional apparatus required in consequence of that action.—Good results; in particular, four experiments, illustrating the theory of Internal Calcination, and shewing the efficacy of the Carbonic Acid as a Flux.

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expose the carbonate in them to strong heats; I at last determined to lay them aside, and return to barrels of iron, with which I had formerly obtained some good results, favoured, perhaps, by some accidental circumstances.

ON the 12th of February 1803, I began a series of experiments with gun-barrels, resuming my former method of working with the fusible metal, and with lead; but altering the position of the barrel from horizontal to vertical; the breech being placed upwards during the action of heat on the carbonate. This very simple improvement has been productive of advantages no less remarkable, than in the case of the tubes of porcelain. In this new position, the included air, quitting the air-tube on the fusion of the metal, and rising to the breech, is exposed to the greatest heat of the furnace, and must therefore react with its greatest force; whereas, in the horizontal position, that air might go as far back as the fusion of the metal reached, where its elasticity would be much feebler. The same disposition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a vessel filled with water; which contributed

contributed very much both to the convenience and safety of these experiments.

IN this view, making use of the brick-furnace with the vertical muffle, already described in page 23. I ordered a pit (*aaa*, fig. 20.) to be excavated under it, for the purpose of receiving a water-vessel. This vessel (represented separately, fig. 21.) was made of cast iron; it was three inches in diameter, and three feet deep; and had a pipe (*de*) striking off from it at right angles, four or five inches below its rim, communicating with a cup (*ef*) at the distance of about two feet. The main vessel being placed in the pit (*aa*) directly below the vertical muffle, and the cup standing clear of the furnace, water poured into the cup flowed into the vessel, and could thus conveniently be made to stand at any level. (The whole arrangement is represented in fig. 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its position was secured by an iron chain (*gf*). The heat communicated downwards generally kept the surface of the water (at *c*) in a state of ebullition; the waste thus occasioned being supplied by means of the cup, into which, if necessary, a constant stream could be made to flow.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel would admit, in order to exclude as much of the fusible metal as possible; for the expansion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the less risk there was of destroying the barrels.

IN the course of practice, a simple mode occurred of removing the metal and withdrawing the cradle: it consisted in placing
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cing the barrel with its muzzle downwards, so as to keep the breech above the furnace and cold, while its muzzle was exposed to strong heat in the muffle. In this manner, the metal was discharged from the muzzle, and the position of the barrel being lowered by degrees, the whole metal was removed in succession, till at last the cradle and its contents became entirely loose. As the metal was delivered, it was received in a crucible, filled with water, standing on a plate of iron placed over the pit, which had been used, during the first stage of the experiment, to contain the water-vessel. It was found to be of service, especially where lead was used, to give much more heat to the muzzle than simply what was required to liquefy the metal it contained ; for when this was not done, the muzzle growing cold as the breech was heating, some of the metal delivered from the breech was congealed at the muzzle, so as to stop the passage.

ACCORDING to this method, many experiments were made in gun-barrels, by which some very material steps were gained in the investigation.

ON the 24th February, I made an experiment with spar and chalk ; the spar being placed nearest to the breech of the barrel, and exposed to the greatest heat, some baked clay intervening between the carbonates. On opening the barrel, a long-continued hissing noise was heard. The spar was in a state of entire calcination ; the chalk, though crumbling at the outside, was uncommonly hard and firm in the heart. The temperature had risen to 32° .

IN this experiment, we have the first clear example, in iron barrels, of what I call *Internal Calcination* ; that is to say, where the carbonic acid separated from the earthy basis, has been accumulated in cavities within the barrel. For, subsequently to the action of strong heat, the barrel had been completely cooled ; the air therefore introduced by means of the air-tube, must have

have resumed its original bulk, and by itself could have no tendency to rush out; the heat employed to open the barrel being barely sufficient to soften the metal. Since, then, the opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident, that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

THE chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the least heated of the two, would be least exposed to a change not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means, the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass, promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate,

nate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner, I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

ON the 28th of February, I introduced some carbonate, accurately weighed, into a small porcelain tube, placed within a larger one, the rest of the large tube being filled with pounded chalk; these carbonates, together with some pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air rushed out with a long-continued hissing noise. The contents of the little tube were lost by the intrusion of some borax which had been introduced over the filex, in order to exclude the fusible metal. But the rest of the carbonate, contained in the large tube, came out in a fine state, being porous and frothy throughout; sparkling every where with facettes, the angular form of which was distinguishable in some of the cavities by help of a lens: in some parts the substance exhibited the rounding of fusion; in many it was in a high degree transparent. It was yellow towards the lower end, and at the other almost colourless. At the upper end, the carbonate seemed to have united with the tube, and at the places of contact to have spread upon it; the union having the appearance of a mutual action. The general mass of carbonate effervesced in acid violently, but the thin stratum immediately contiguous to the tube, feebly, if at all.

ON the 3d of March, I introduced into a very clean tube of porcelain 36.8 of chalk. The tube was placed in the upper

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part of the cradle, the remaining space being filled with two pieces of chalk, cut for the purpose; the uppermost of these being excavated, so as to answer the purpose of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer used; but the heat was guessed to be about 30° . After the barrel had stood during a few minutes in its delivering position, the whole lead, with the rod and cradle, were thrown out with a smart report, and with considerable force. The lowermost piece of chalk had scarcely been acted upon by heat. The upper part of the other piece was in a state of marble, with some remarkable facettes. The carbonate, in the little tube, had shrunk very much during the first action of heat, and had begun to sink upon itself, by a further advancement towards liquefaction. The mass was divided into several cylinders, lying confusedly upon each other; this division arising from the manner in which the pounded chalk was rammed into the tube in successive portions. In several places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a soft substance; the globular form and shining surface of all these cavities, clearly indicating fusion. The substance was semitransparent; in some places yellow, and in some colourless. When broken, the solid parts shewed a saline fracture, composed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, so as to render it impossible to ascertain what loss had been sustained. In general, the line of contact was of a brown colour; yet there was no room for suspecting the presence of any foreign matter, except, perhaps, from the iron-rod which was used in ramming down the chalk. But, in subsequent experiments, I have observed the same brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purposely rammed with a piece of wood;

wood ; so that this colour, which has occurred in almost every similar case, remains to be accounted for. The carbonate effervesced violently with acid ; the substance in contact with the tube, doing so, however, more feebly than in the heart, leaving a copious deposit of white sandy matter, which is doubtless a part of the tube, taken up by the carbonate in fusion.

ON the 24th of March, I made a similar experiment, in a stout gun-barrel, and took some care, after the application of heat, to cool the barrel slowly, with a view to crystallization. The whole mass was found in a fine state, and untouched by the lead ; having a semitransparent and saline structure, with various facettes. In one part, I found the most decided crystallization I had obtained, though of a small size : owing to its transparency it was not easily visible, till the light was made to reflect from the crystalline surface, which then produced a dazzle, very observable by the naked eye : when examined by means of a lens, it was seen to be composed of several plates, broken irregularly in the fracture of the specimen, all of which are parallel to each other, and reflect under the same angle, so as to unite in producing the dazzle. This structure was observable equally well in both parts of the broken specimen. In a former experiment, as large a facette was obtained in a piece of solid chalk ; but this result was of more consequence, as having been produced from chalk previously pounded.

THE foregoing experiments proved the superior efficacy of iron vessels over those of porcelain, even where the thickness was not great ; and I persevered in making a great many experiments with gun-barrels, by which I occasionally obtained very fine results : but I was at last convinced, that their thickness was not sufficient to ensure regular and steady success : For this purpose, it appeared proper to employ vessels of such strength, as to bear a greater expansive force than was just necessary ; since, occasionally, (owing to our ignorance of the re-

lation between the various forces of expansion, affinity, tenacity, &c.), much more strain has been given to the vessels than was requisite. In such cases, barrels have been destroyed, which, as the results have proved, had acted with sufficient strength during the first stages of the experiments, though they had been unable to resist the subsequent overstrain. Thus, my success with gun-barrels, depended on the good fortune of having used a force no more than sufficient, to constrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourse to iron barrels of much greater strength, and tried various modes of construction.

I HAD some barrels executed by wrapping a thick plate of iron round a mandrel, as is practised in the formation of gun-barrels; and likewise by bringing the two flat sides together, so as to unite them by welding. These attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the solid, so as to form a barrel. In this manner I succeeded well. The first barrel I tried in this way was of small bore, only half an inch: Its performance was highly satisfactory, and such as to convince me, that the mode now adopted was the best of any that I had tried. Owing to the smallness of the bore, a pyrometer could not be used internally, but was placed upon the breech of the barrel, as it stood in the vertical muffle. In this position, it was evidently exposed to a much less heat than the fiducial part of the apparatus, which was always placed, as nearly as could be guessed, at the point of greatest heat.

ON the 4th of April, an experiment was made in this way with some spar; the pyrometer on the breech giving 33° . The spar came out clean, and free from any contamination, adhering to the inside of the porcelain tube: it was very much shrunk, still retaining a cylindrical form, though bent by partial adhesions. Its surface bore scarcely any remains of the impression taken by the
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the powder, on ramming it into the tube: it had, to the naked eye, the roughness and semitransparency of the pith of a rush stripped of its outer skin. By the lens, this same surface was seen to be glazed all over, though irregularly, shewing here and there some air-holes. In fracture, it was semitransparent, more vitreous than crystalline, though having a few facettes: the mass, was seemingly formed of a congeries of parts, in themselves quite transparent: and, at the thin edges, small pieces were visible of perfect transparency. These must have been produced in the fire; for the spar had been ground with water, and passed through sieves, the same with the finest of those used at Etruria, as described by Mr WEDGWOOD, in his paper on the construction of his Pyrometer.

WITH the same barrel I obtained many interesting results, giving as strong proofs of fusion as in any former experiments; with this remarkable difference, that, in these last, the substance was compact, with little or no trace of frothing. In the gun-barrels where fusion had taken place, there had always been a loss of 4 or 5 *per cent.*, connected, probably, with the frothing. In these experiments, for a reason soon to be stated, the circumstance of weight could not be observed; but appearances led me to suppose, that here the loss had been small, if any.

ON the 6th of April, I made another experiment with the square barrel, whose thickness was now much reduced by successive scales, produced by oxidation, and in which a small rent began to appear externally, which did not, however, penetrate to the bore. The heat rose high, a pyrometer on the breech of the barrel giving 37° . On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a state of saline marble;

marble ; another was hard and white, without any saline grains, and scarcely effervesced in acid. It was probably quicklime, formed by internal calcination, but in a state that has not occurred in any other experiment.

THE workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was seen in this piece, I was surprised to see the inside of the barrel lined with a set of transparent and well-defined crystals, of small size, yet visible by the naked eye. They lay together in some places, so as to cover the surface of the iron with a transparent coat ; in others they were detached, and scattered over the surface. Unfortunately, the quantity of this substance was too small to admit of much chemical examination ; but I immediately ascertained, that it did not in the least effervesce in acid, nor did it seem to dissolve in it. The crystals were in general transparent and colourless, though a few of them were tinged seemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a strong resemblance to the crystals of the Lamellated Stylbite of HAÜY. Though made above two years ago, they still retain their form and transparency unchanged. Whatever this substance may be, its appearance, in this experiment, is in the highest degree interesting, as it seems to afford an example of the mode in which Dr HUTTON supposes many internal cavities to have been lined, by the sublimation of substances in a state of vapour ; or, held in solution, by matters in a gaseous form. For, as the crystals adhered to a part of the barrel, which must have been occupied by air during the action of heat, it seems next to certain that they were produced by sublimation.

THE very powerful effects produced by this last barrel, the size of which (reduced, indeed, by repeated oxidation) was not
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above an inch square, made me very anxious to obtain barrels of the same substance, which being made of greater size, ought to afford results of extreme interest. I found upon inquiry, that this barrel was not made of Swedish iron, as I at first supposed, but of what is known by the name of *Old Sable*, from the figure of a Sable stamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made*.

A WORKMAN explained to me some of the properties of different kinds of irons, most interesting in my present pursuit; and he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus, as he shewed me, cast iron crushes in a dull-red heat, or perhaps about 15° of WEDGWOOD; steel, in a heat perhaps of 30° ; Swedish iron, in a bright white heat, perhaps of 50° or 60° ; old sable, itself, likewise yields, but in a much higher heat, perhaps of 100° . I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality, that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable
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* I WAS favoured with this account of it by the late Professor ROBISON.

number of bars of that iron, which fully answered my expectations.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance, a necessity arose of adding some new devices to those already described: for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any method previously used. I therefore determined in future to adopt the following arrangement.

A SMALL tube of porcelain (*ik*, fig. 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment, was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken place between the tube and the carbonate. The balance which I used, turned, in a constant and steady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that space laid a small piece of lump-chalk (*i*), dressed to a cylinder, with the ends cut flat and smooth, and I usually cut a letter on each end, the more effectually to observe the effects produced by heat upon the chalk; the weight of this piece of chalk being always estimated along with that of the powder contained in the tube. In some experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide against the case of ebullition:

ebullition: But as that did not often occur, I seldom took the trouble of this last precaution.

It was now of consequence to protect the tube, thus prepared, from being touched during the experiment, by any substance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The small tube (Fig. 23. *ik*), with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*pk*, Fig. 24.). Upon this a fragment of porcelain (*l*), of such a size as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dressed, so as nearly to fit and fill up the inside of the large tube, one end of it being rudely cut into the form of a cone. This mass being then introduced, with its cylindrical end downwards, was made to press upon the fragment of porcelain (*l*). I then dropped into the space (*n*), between the conical part of this mass and the tube, a set of fragments of chalk, of a size beyond what could possibly fall between the cylindrical part and the tube, and pressed them down with a blunt tool, by which the chalk being at the same time crushed and rammed into the angle, was forced into a mass of some solidity, which effectually prevented any thing from passing between the large mass of chalk and the tube. In practice, I have found this method always to answer, when done with care. I covered the chalk, thus rammed, with a stratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of flint and chalk; the muzzle being always occupied with chalk, which was easily pressed into a mass of tolerable firmness, and, suffering no change in very low heats, excluded the fusible metal in the first stages of the experiment.

THE large tube, thus filled, was placed in the cradle, sometimes with the muzzle upwards, and sometimes the reverse. I

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have frequently altered my views as to that part of the arrangement, each mode possessing peculiar advantages and disadvantages. With the muzzle upwards, (as shewn in fig. 24. and 25.) the best security is afforded against the intrusion of the fusible metal; because the air, quitting the air-tube in the working position, occupies the upper part of the barrel; and the fusible metal stands as a liquid (at *q*, fig. 25.) below the muzzle of the tube, so that all communication is cut off, between the liquid metal and the inside of the tube. On the other hand, by this arrangement, the small tube, which is the fiducial part of the apparatus, is placed at a considerable distance from the breech of the barrel, so as either to undergo less heat than the upper part, or to render it necessary that the barrel be thrust high into the muffle.

WITH the muzzle of the large tube downwards, the inner tube is placed (as shewn in fig. 22.), so as still to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the small tube near to the breech of the barrel: and though there is here less security against the intrusion of liquid metal, I have found that a point of little consequence; since, when the experiment is a good one, and that the carbonic acid has been well confined, the intrusion seldom takes place in any position. In whichever of the two opposite positions the large tube was placed, a pyrometer was always introduced, so as to lie as near as possible to the small tube. Thus, in the first-mentioned position, the pyrometer was placed immediately below the large tube, and, in the other position, above it; so that, in both cases, it was separated from the carbonate by the thickness only of the two tubes.

MUCH room was unavoidably occupied by this method, which necessarily obliged me to use small quantities of carbonate,
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bonate, the subject of experiment seldom weighing more than 10 or 12 grains, and in others far less*.

ON the 11th of April 1803, with a barrel of old sable iron having a bore of 0.75 of an inch, I made an experiment in which all these arrangements were put in practice. The large tube contained two small ones; one filled with spar, and the other with chalk. I conceived that the heat had risen to 33° , or somewhat higher. On melting the metals, the cradle was thrown out with considerable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my astonishment, indicated 64° . Yet all was found. The two little tubes came out quite clean and uncontaminated. The spar had lost 17.0 *per cent.*: The chalk 10.7 *per cent.*: The spar was half sunk down, and run against the side of the little tube: Its surface was shining, its texture spongy, and it was composed of a transparent and jelly-like substance: The chalk was entirely in a state of froth. This experiment extends our power of action, by shewing, that compression, to a considerable degree, can be carried on in so great a heat as 64° . It seems likewise to prove, that, in some of the late experiments with the square barrel, the heat had been much higher than was supposed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in some of them, particularly in the last, it must have risen at least as high as in the present experiment.

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* I measured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal sand. By comparing the weight of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it corresponded to 0.00075 of a cubic inch. From these data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

ON the 21st of April 1805, a similar experiment was made with a new barrel, bored in a square bar of old sable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during several hours, and the furnace allowed to burn out during the night. The barrel had the appearance of soundness, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63° . On examination, and after beating off the smooth and even scale of oxide peculiar to the old sable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithstanding the failure of the barrel, was one of the most interesting I had made, since it afforded proof of complete fusion. The carbonate had boiled over the lips of the little tube, standing, as just described, with its mouth upwards, and had run down to within half an inch of its lower end: most of the substance was in a frothy state, with large round cavities, and a shining surface; in other parts, it was interspersed with angular masses, which have evidently been surrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervescence, and not turning red like quicklime in nitric acid, which seemed to have no effect upon it in the lump. It was probably a compound of quicklime with the substance of the tube.

WITH the same barrel repaired, and with others like it, many similar experiments were made at this time with great success; but to mention them in detail, would amount nearly to a repetition of what has been said. I shall take notice of only four of them, which, when compared together, throw much light on the theory of these operations, and likewise seem to establish a very important principle in geology. These
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four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

THE first of these experiments was made on the 27th of April 1803, in one of the large barrels of old sable, with all the above-mentioned arrangements. The heat had risen, contrary to my intention, to 78° and 79° . The tubes came out uncontaminated with fusible metal, and every thing bore the appearance of soundness. The contents of the little tube, consisting of pounded chalk, and of a small piece of lump-chalk, came out clean, and quite loose, not having adhered to the inside of the tube in the smallest degree. There was a loss of 41 *per cent.*, and the calcination seemed to be complete; the substance, when thrown into nitric acid, turning red, without effervescence at first, though, after lying a few minutes, some bubbles appeared. According to the method followed in all these experiments, and lately described at length, (and shewn in fig. 24. & 25.), the large tube was filled over the small one, with various masses of chalk, some in lump, and some rammed into it in powder; and in the cradle there lay some pieces of chalk, filling up the space, so that in the cradle there was a continued chain of carbonate of four or five inches in length. The substance was found to be less and less calcined, the more it was removed from the breech of the barrel, where the heat was greatest. A small piece of chalk, placed at the distance of half an inch from the small tube, had some saline substance in the heart, surrounded and intermixed with quicklime, distinguished by its dull white. In nitric acid, this substance became red, but effervesced pretty briskly; the effervescence continuing till the whole was dissolved. The next portion of chalk, was in a firm state of limestone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compression: the two last-mentioned pieces of chalk effervescing with violence in the acid, and shewing

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no redness when thrown into it. These facts clearly prove, that the calcination of the contents of the small tube had been internal, owing to the violent heat which had separated its acid from the most heated part of the carbonate, according to the theory already stated. The soundness of the barrel was proved by the complete state of those carbonates which lay in less heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

THE second of these experiments was made on the 29th of April, in the same barrel with the last, after it had afforded some good results. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch. The heat rose to 60° . The barrel was covered externally with a black spongy substance, the constant indication of failure, and a small drop of white metal made its appearance. The cradle was removed without any explosion or hissing. The carbonates were entirely calcined. The barrel had yielded, but had resisted well at first; for, the contents of the little tube were found in a complete state of froth, and running with the porcelain.

THE third experiment was made on the 30th of April, in another similar barrel. Every circumstance was the same as in the two last experiments, only that the air-tube was now reduced to half its last bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermost gave 41° , the other only 15° . The contents of the inner tube had lost 16 *per cent.*, and were reduced to a most beautiful state of froth, not very much injured by the internal calcination, and indicating a thinner state of fusion than had appeared.

THE fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0.0318, being less than one-thirtieth of a cubic inch. The upper pyrometer

rometer gave 25° , and the under one 16° : The lowest masses of carbonate were scarcely affected by the heat : The contents of the little tube had lost 2.9 *per cent.* ; both the lump and the pounded chalk were in a fine saline state, and, in several places had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the little tube, which, after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed, that the solid mass on which these crystals stood, was uncommonly transparent.

IN these four experiments, the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly constrained, and its fusion accomplished, in a heat between 41° and 15° . In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube, in a temperature between 25° and 16° . On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced ; since the first experiment

periment proved, that quicklime by itself, could neither be melted, nor act upon porcelain, even in the violent heat of 79° ; whereas, in the last experiment where the carbonic acid was retained, both of these effects took place in a very low temperature.

V.

Experiments in which Water was employed to increase the Elasticity of the included Air.—Cases of complete Compression.—General Observations.—Some Experiments affording interesting results; in particular, shewing a mutual action between Silica and the Carbonate of Lime.

FINDING that such benefit arose from the increase of elasticity given to the included air in the last-mentioned experiments, by the diminution of its quantity; it now occurred to me, that a suggestion formerly made by Dr KENNEDY, of using water to assist the compressing force, might be followed with advantage: That while sufficient room was allowed for the expansion of the liquid metal, a reacting force of any required amount, might thus be applied to the carbonate. In this view, I adopted the following mode, which, though attended with considerable difficulty in execution, I have often practised with success. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The piece was then dropped into a tube of porcelain of about an inch in depth, and covered with pounded chalk, which was firmly rammed upon it. The tube was then placed in the cradle along with the subject of experiment, and the whole was plunged into the fusible metal, previously poured into the barrel, and heated so as merely to render it liquid. The metal being thus suddenly cooled, the

the tube was encased in a solid mass, before the heat had reached the included moisture. The difficulty was to catch the fusible metal at the proper temperature ; for when it was so hot as not to fix in a few seconds, by the contact of the cradle and its contents, the water was heard to bubble through the metal and escape. I overcame this difficulty, however, by first heating the breech of the barrel, (containing a sufficient quantity of fusible metal), almost to redness, and then setting it into a vessel full of water, till the temperature had sunk to the proper pitch, which I knew to be the case when the hissing noise produced in the water by the heated barrel ceased ; the cradle, during the last stage of this operation, being held close to the muzzle of the barrel, and ready to be thrust into it.

ON the 2d of May, I made my first experiment in this way, using the same air-tube as in the last experiment, which was equal in capacity to one-thirtieth of a cubic inch. Half a grain of water was introduced in the manner just described. The barrel, after an hour of red-heat, was let down by a rope and pulley, which I took care to use in all experiments, in which there was any appearance of danger. All was found. The metals rushed out smartly, and a flash of flame accompanied the discharge. The upper pyrometer gave 24° , and the lower one 14° . The contents of the inner tube had lost less than 1 *per cent.*, strictly 0.84. The carbonate was in a state of good limestone ; but the heat had been too feeble : The lower part of the chalk in the little tube was not agglutinated : The chalk round the fragment of pipe-stalk (used to introduce the water), which had been more heated than the pyrometer, and the small rod, which had moulded itself in the boll of the stalk, were in a state of marble.

ON the 4th of May, I made an experiment like the last, but with the addition of 1.05 grains water. After application of heat, the

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fire was allowed to burn out till the barrel was black. The metal was discharged irregularly. Towards the end, the inflammable air produced, burnt at the muzzle, with a lambent flame; during some time, arising doubtless from hydrogen gas, more or less pure, produced by the decomposition of the water. The upper pyrometer indicated 36° , and the lower one 19° . The chalk which lay in the outer part of the large tube was in a state of marble. The inner tube was united to the outer one, by a star of fused matter, black at the edges, and spreading all round, surrounding one of the fragments of porcelain which had fallen by accident in between the tubes. The inner tube, with the starry matter adhering to it, but without the coated fragment, seemed to have sustained a loss of 12 *per cent.*, on the original carbonate introduced. But, the substance surrounding the fragment being inappreciable, it was impossible to learn what loss had been really sustained. Examining the little tube, I found its edges clean, no boiling over having taken place. The top of the small lump of chalk had sunk much. When the little tube was broken, its contents gave proof of fusion in some parts, and in others, of the nearest approach to it. A strong action of ebullition had taken place all round, at the contact of the tube with the carbonate: in the heart, the substance had a transparent granular texture, with little or no crystallization. The small piece of lump-chalk was united and blended with the rammed powder, so that they could scarcely be distinguished. In the lower part of the carbonate, where the heat must have been weaker, the rod had acted more feebly on the tube, and was detached from it: here the substance was firm, and was highly marked in the fracture with crystalline facettes. Wherever the carbonate touched the tube, the two substances exhibited, in their mixture, much greater proofs of fusion than could be found in the pure carbonate. At one place, a stream of this compound had penetrated a rent in the

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the inner tube, which it had filled completely, constituting a real vein, like those of the mineral kingdom: which is still distinctly to be seen in the specimen. It had then spread itself upon the outside of the inner tube, to the extent of half an inch in diameter, and had enveloped the fragment of porcelain already mentioned. When pieces of the compound were thrown into nitric acid, some effervesced, and some not.

I REPEATED this experiment on the same day, with two grains of water. The furnace being previously hot; I continued the fire during one half-hour with the muffle open, and another with a cover upon it. I then let the barrel down by means of the pulley. The appearance of a large longitudinal rent, made me at first conceive that the experiment was lost, and the barrel destroyed: The barrel was visibly swelled, and in swelling had burst the crust of smooth oxide with which it was surrounded; at the same time, no exudation of metal had happened, and all was found. The metals were thrown out with more suddenness and violence than in any former experiment, but the rod remained in its place, being secured by a cord. The upper pyrometer gave 27° , the lower 23° . The contents of the inner tube had lost 1.5 *per cent.* The upper end of the little lump of chalk, was rounded and glazed by fusion; and the letter which I have been in the habit of cutting on these small pieces, in order to trace the degree of action upon them, was thus quite obliterated. On the lower end of the same lump, the letter is still visible. Both the lump and the rammed chalk were in a good semitransparent state, shining a little in the fracture, but with no good facettes, and no where appearing to have acted on the tube. This last circumstance is of consequence, since it seems to shew, that this very remarkable action of heat, under compression, was performed without the assistance of the substance of the tube, by which, in many other
H 2 experiments,

experiments, a considerable additional fusibility has been communicated to the carbonate.

THESE experiments, and many others made about the same time, with the same success, clearly prove the efficacy of water in assisting the compression; and results approaching to these in quality, obtained, in some cases, by means of a very small air-tube, shew that the influence of water on this occasion has been merely mechanical.

DURING the following summer and autumn 1803, I was occupied with a different branch of this subject, which I shall soon have occasion to mention.

IN the early part of last year, 1804, I again resumed the sort of experiments lately described, having in view principally to accomplish absolute compression, in complete imitation of the natural process. In this pursuit, I did not confine myself to water, but made use of various other volatile substances, in order to assist compression; namely, carbonate of ammonia, nitrate of ammonia, gunpowder, and paper impregnated with nitre. With these I obtained some good results, but none such as to induce me to prefer any of these compressors to water. Indeed, I am convinced, that water is superior to them all. I found, in several experiments, made with a simple air-tube, without any artificial compressor, in which a very low red-heat had been applied, that the carbonate lost one or one and a half *per cent.* Now, as this must have happened in a temperature scarcely capable of inflaming gunpowder, it is clear, that such loss would not have been prevented by its presence: whereas water, beginning far below redness to assume a gaseous form, will effectually resist any calcination, in low as well as in high heats. And as the quantity of water can very easily be regulated by weight, its employment for this purpose seems liable to no objection.

ON

ON the 2d of January 1804, I made an experiment with marble and chalk, with the addition of 1.1 grain of water. I aimed at a low heat, and the pyrometer, though a little broken, seemed clearly to indicate 22° . Unluckily, the muzzle of the large tube, which was closed as usual with chalk, was placed uppermost, and exposed to the strongest heat. I found it rounded by fusion, and in a frothy state. The little tube came out very clean, and was so nearly of the same weight as when put in, that its contents had lost but 0.074 *per cent.* of the weight of the original carbonate. The marble was but feebly agglutinated, but the chalk was in a state of firm limestone, though it must have undergone a heat under 22° , or that of melting silver. This experiment is certainly a most remarkable one, since a heat has been applied, in which the chalk has been changed to hard limestone, with a loss less than the 1000th part of its weight, (exactly $\frac{1}{1333}$) ; while, under the same circumstances of pressure, though probably with more heat, some of the same substance had been brought to fusion. What loss of weight this fused part sustained, cannot be known.

ON the 4th of January, a similar experiment was made, likewise with 1.1 grain of water. The discharge of the metal was accompanied with a flash of flame. The pyrometer indicated 26° . The little tube came out quite clean. Its contents had been reduced from 14.53 to 14.46, difference 0.07 grains, being 0.47 *per cent.* on the original carbonate, less than one two-hundredth part of the original weight, (exactly $\frac{1}{212}$). The chalk was in a state of firm saline marble, but with no unusual qualities.

THESE two last experiments are rendered still more interesting, by another set which I made soon after, which shewed, that one essential precaution in a point of such nicety had been neglected, in not previously drying the carbonate. In several trials made in the latter end of the same month,

I found, that chalk exposed to a heat above that of boiling water, but quite short of redness, lost 0.34 *per cent.*; and in another similar trial, 0.46 *per cent.* Now, this loss of weight equals within 0.01 *per cent.* the loss in the last-mentioned experiment, that being 0.47; and far surpasses that of the last but one, which was but 0.074. There is good reason, therefore, to believe, that had the carbonate, in these two last experiments, been previously dried, it would have been found during compression to have undergone no loss.

THE result of many of the experiments lately mentioned, seems fully to explain the perplexing discordance between my experiments with porcelain tubes, and those made in barrels of iron. With the porcelain tubes, I never could succeed in a heat above 28° , or even quite up to it; yet the results were often excellent. Whereas, the iron-barrels have currently stood firm in heats of 41° or 51° , and have reached even to 70° or 80° without injury. At the same time, the results, even in those high heats, were often inferior, in point of fusion, to those obtained by low heats in porcelain. The reason of this now plainly appears. In the iron-barrels it has always been considered as necessary to use an air-tube, in consequence of which, some of the carbonic acid has been separated from the earthy basis by internal calcination: what carbonic acid remained, has been more forcibly attracted, according to M. BERTHOLLET's principle, and, of course, more easily compressed, than when of quantity sufficient to saturate the lime: but, owing to the diminished quantity of the acid, the compound has become less fusible than in the natural state, and, of course, has undergone a higher heat with less effect. The introduction of water, by furnishing a reacting force, has produced a state of things similar to that in the porcelain tubes; the carbonate sustaining little or no loss of weight,

weight, and the compound retaining its fusibility in low heats*.

IN the early part of 1804, some experiments were made with barrels, which I wished to try, with a view to another series of experiments. The results were too interesting to be passed over; for, though the carbonic acid in them was far from being completely constrained, they afforded some of the finest examples I had obtained, of the fusion of the carbonate, and of its union with flint.

ON the 13th of February, an experiment was made with pounded oyster-shell, in a heat of 33° , without any water being introduced to assist compression. The loss was apparently of 12 *per cent*. The substance of the shell had evidently been in viscid fusion: it was porous, semitransparent, shining in surface and fracture; in most parts with the gloss of fusion, in many others with facettes of crystallization. The little tube had been set with its muzzle upwards; over it, as usual, lay a fragment of porcelain, and on that a round mass of chalk. At the contact of the porcelain and the chalk, they had run together, and the chalk had been evidently in a very soft state; for, resting with its weight on the porcelain, this last had been pressed into the substance of the chalk, deeper than its own breadth, a rim of chalk being visible without the surface of the porcelain; just as when the round end of a knife is pressed upon.

* The retentive power here ascribed to the porcelain tubes, seems not to accord with what was formerly mentioned, of the carbonic acid having been driven through the substance of the tube. But the loss by this means has probably been so small, that the native properties of the carbonate have not been sensibly changed. Or, perhaps, this penetrability may not be so universal as I have been induced to think, by having met with it in all the cases which I tried. In this doubt, I strenuously recommend a further examination of this subject to gentlemen who have easy access to such porcelains as that of Dresden or of Seve.

upon a piece of soft butter. The carbonate had spread very much on the inside of the tube, and had risen round its lip, as some salts rise from their solution in water. In this manner, a small quantity of the carbonate had reached the outer tube, and had adhered to it. The black colour frequently mentioned as accompanying the union of the carbonates with the porcelain, is here very remarkable.

ON the 26th of February, I made an experiment, in which the carbonate was not weighed, and no foreign substance was introduced to assist the compression. The temperature was 46° . The pyrometer had been affected by the contact of a piece of chalk, with which it had united; and some of the carbonate must have penetrated the substance of the pyrometer, since this last had visibly yielded to pressure, as appeared by a swelling near the contact. I observed in these experiments, that the carbonate had a powerful action on the tubes of Cornish clay, more than on the pounded flint. Perhaps it has a peculiar affinity for argil, and this may lead to important consequences. The chalk had visibly first shrunk upon itself, so as to be detached from the sides, and had then begun to run by successive portions, so as still to leave a pillar in the middle, very irregularly worn away; indicating a successive liquefaction, like that of ice, not the yielding of a mass softening all at once.

ON the 28th of February, I made an experiment with oyster-shell unweighed, finely ground, and passed through the closest sieves. The pyrometer gave 40° . The piece of chalk below it had been so soft, as to sink to the depth of half an inch into the mouth of the iron air-tube, taking its impression completely. A small part of this lump was contaminated with iron, but the rest was in a fine state. The tube had a rent in it, through which the carbonate, united with the matter of the tube, had flowed in two or three places. The shell

shell had shrunk upon itself, so as to stand detached from the sides, and bore very strong marks of fusion. The external surface was quite smooth, and shining like an enamel. The internal part consisted of a mixture of large bubbles and solid parts: the inside of the bubbles had a lustre much superior to that of the outside, and equal to that of glass. The general mass was semitransparent; but small parts were visible by the lens, which were completely transparent and colourless. In several places this smooth surface had crystallized, so as to present brilliant facettes, steadily shining in certain aspects. I observed one of these facettes on the inside of an air-bubble, in which it interrupted the spherical form as if the little sphere had been pressed inwards at that spot, by the contact of a plane surface. In some chalk near the mouth of the large tube, which lay upon a stratum of filex, another very interesting circumstance occurred. Connected with its lower end, a substance was visible, which had undoubtedly resulted from the union of the carbonate with the filex. This substance was white and semitransparent, and bore the appearance of chalcedony. The mass of chalk having attached itself to that above it, had shrunk upwards, leaving an interval between it and the filex, and carrying some of the compound up with it. From thence this last had been in the act of dropping in a viscid state of fusion, as evidently appeared when the specimen was entire; having a stalactite and stalagmite corresponding accurately to each other. Unluckily I broke off the stalactite, but the stalagmite continues entire, in the form of a little cone. This new substance effervesced in acid, but not briskly. I watched its entire solution; a set of light clouds remained undissolved, and probably some jelly was formed; for I observed, that a series of air-bubbles remained in the form of the fragment, and moved together without any visible connection; thus seeming to indicate a chemical union be-

tween the filex and the carbonate. The shell, fused in the experiment, dissolved entirely in the acid, with violent effervescence.

IN the three last experiments, and in several others made at the same time, the carbonate had not been weighed; but no water being introduced to assist the compression, it is probable there was much loss by internal calcination; and owing doubtless to this, the carbonates have crumbled almost entirely to dust, while the compounds which they had formed with filex remain entire.

ON the 13th of March, I made a similar experiment, in which, besides some pounded oyster-shell, I introduced a mixture of chalk, with 10 *per cent.* of filex intermixed, and ground together in a mortar with water, in a state of cream, and then well dried. The contents of the tube when opened, were discharged with such violence, that the tube was broken to pieces; but I found a lump of chalk, then in a state of white marble, welded to the compound; which last, in its fracture, shewed that irregular black colour, interspersed roughly through a crystalline mass, that belongs to the alpine marbles, particularly to the kind called at Rome *Cipolline*. It was very hard and firm; I think unusually so. It effervesced constantly to the last atom, in diluted nitric acid, but much more sluggishly than the marble made of pure chalk. A cloudiness appeared pervading all the liquid. When the effervescence was over, a series of bubbles continued during the whole day in the acid, without any disposition to burst, or rise to the surface. After standing all next day and night, they maintained their station; and the solution being stirred, was found to be entirely agglutinated into a transparent jelly, breaking with sharp angles. This experiment affords a direct and positive proof of a chemical union having taken place between the carbonate and filex.

VI.

Experiments made in Platina,—with Spar,—with Shells,—and with Carbonate of Lime of undoubted purity.

SINCE I had the honour of laying before this Society a short sketch of the foregoing experiments, on the 30th of August last (1804), many chemists and mineralogists of eminence have favoured me with some observations on the subject, and have suggested doubts which I am anxious to remove. It has been suggested, that the fusibility of the carbonates may have been the consequence of a mixture of other substances, either originally existing in the natural carbonate, or added to it by the contact of the porcelain tube.

WITH regard to the first of these surmises, I beg leave to observe, that, granting this cause of fusion to have been the real one, a material point, perhaps all that is strictly necessary in order to maintain this part of the Huttonian Theory, was nevertheless gained. For, granting that our carbonates were impure, and that their impurity rendered them fusible, still the same is true of almost every natural carbonate; so that our experiments were, in that respect, conformable to nature. And as to the other surmise, it has been shewn, by comparing together a varied series of experiments, that the mutual action between the lime and the porcelain was occasioned entirely by the presence of the carbonic acid, since, when it was absent, no action of this kind took place. The fusion of our carbonates cannot, therefore, be ascribed to the porcelain.

BEING convinced, however, by many observations, that the fusibility of the carbonate did not depend upon impurity,

I have exerted myself to remove, by fresh experiments, every doubt that has arisen on the subject. In order to guard against natural impurities, I have applied to such of my friends as have turned their attention to chemical analysis, (a branch of the science to which I have never attended,) to furnish me with carbonate of lime of undoubted purity. To obviate the contamination arising from the contact of the porcelain tubes, I determined to confine the subject of experiment in some substance which had no disposition to unite with the carbonate. I first tried charcoal, but found it very troublesome, owing to its irregular absorption of water and air.

I THEN turned my thoughts to the construction of tubes or cups of platina for that purpose. Being unable readily to procure proper solid vessels of this substance, I made use of thin laminated plates, formed into cups. My first method was, to fold the plate exactly as we do blotting-paper to form a filter (Fig. 26.); this produced a cup capable of holding the thinnest liquid; and being covered with a lid, formed of a similar thin plate, bent at the edges, so as to overlap considerably (Fig. 28.), the carbonate it contained was secured on all sides from the contact of the porcelain tube within which it was placed. Another convenient device likewise occurred: I wrapt a piece of the plate of platina round a cylinder, so as to form a tube, each end of which was closed by a cover like that just described (Fig. 27. and 29). (In figure 26. and 27. these cups are represented upon a large scale, and in 28. and 29. nearly of their actual size). This last construction had the advantage of containing eight or nine grains of carbonate, whereas the other would only hold about a grain and a half. On the other hand, it was not fit to retain a thin liquid; but, in most cases, that circumstance was of no consequence; and I foresaw that the carbonates could not thus

thus escape without proving the main point under consideration, namely, their fusion.

THE rest of the apparatus was arranged in all respects as formerly described, the same precautions being taken to defend the platina vessel as had been used with the inner tubes of porcelain.

IN this manner I have made a number of experiments during this spring and summer, the result of which is highly satisfactory. They prove, in the first place, the propriety of the observations which led to this trial, by shewing, that the pure carbonate, thus defended from any contamination, is decidedly more refractory than chalk; since, in many experiments, the chalk has been reduced to a state of marble, while the pure carbonate, confined in the platina vessel, has been but very feebly acted upon, having only acquired the induration of a sandstone.

IN other experiments, however, I have been more successful, having obtained some results, worthy, I think, of the attention of this Society, and which I shall now submit to their inspection. The specimens are all inclosed, for safety, in glass tubes, and supported on little stands of wax, (fig. 31, 32, 33.). The specimens have, in general, been removed from the cup or tube of platina in which they were formed, these devices having the advantage of securing both the vessel and its contents, by enabling us to unwrap the folds without violence; whereas, in a solid cup or tube, it would have been difficult, after the experiment, to avoid the destruction either of the vessel or its contents, or both.

APRIL 16. 1805.—An experiment was made with pure calcareous spar from St Gothard, remarkably transparent, and having a strong double refraction. A temperature of 40° was applied; but owing to some accident, the weight was not known. The conical cup came out clean and entire, filled
not

not quite to the brim with a yellowish-grey substance, having a shining surface, with longitudinal streaks, as we sometimes see on glass. This surface was here and there interrupted by little white tufts or protuberances, disposed irregularly. On the ledge of the cup, formed by the ends of the folded platina, were several globular drops like minute pearls, visible to the naked eye, the number of which amounted to sixteen. These seem to have been formed by the entire fusion of what carbonate happened to lie on the ledge, or had been entangled amongst the extremities of the folds, drawing itself together, and uniting in drops; as we see when any substance melts under the blowpipe. This result is preserved entire, without deranging the tube. I am sorry to find that it has begun to fall to decay, in consequence, no doubt, of too great a loss of its carbonic acid. But the globules do not seem as yet to have suffered any injury.

APRIL 25.—The same spar was used, with two grains of water, and a heat of 33° . I have reason to suspect, however, that, in this and several other experiments made at this time, the metal into which the cradle was plunged, on first introduction into the barrel, had been too hot, so as to drive off the water. There was a loss of 6.4 *per cent*. The result lay in the cup without any appearance of frothing or swelling. The surface was of a clean white, but rough, having in one corner a space shining like glass. The cup being unwrapped, the substance was obtained sound and entire: where it had moulded itself on the platina, it had a small degree of lustre, with the irregular semitransparency of saline marble: when broken, it preserved that character more completely than in any result hitherto obtained; the fracture being very irregular and angular, and shining with facettes in various directions. I much regret that this beautiful specimen

no longer exists, having crumbled entirely to pieces, notwithstanding all the care I took to inclose it with glass and wax.

APRIL 26.—An experiment was made with some carbonate of lime, purified by my friend Sir GEORGE MACKENZIE. Two grains of water were introduced, but were lost, I suspect, as in the last case. The heat applied was 32° . The loss of weight was 10.6 *per cent*. Yet, though made but one day after the last-mentioned specimen, it remains as fresh and entire as at first, and promises to continue unchanged. The external surface, as seen on removing the lid of the conical cup, was found to shine all over like glass, except round the edges, which were fringed with a series of white and rough sphericles, one set of which advanced, at one spot, near to the centre. The shining surface was composed of planes, which formed obtuse angles together, and had their surface striated; the striæ bearing every appearance of a crystalline arrangement. When freed from the cup, as before, the substance moulded on the platina was found to have assumed a fine pearly surface. Some large air-bubbles appeared, which had adhered to the cup, and were laid open by its removal, whose internal surface had a beautiful lustre, and was full of striæ like the outward surface. The mass is remarkable for semitransparency, as seen particularly where the air-bubbles diminish its thickness: a small part of the mass being broken at one end, shews an internal saline structure.

APRIL 29.—A cup of platina was filled with several large pieces of a periwinkle* shell, the sharp point of the spiral being made to stand upright in the cup, (fig. 30.). A heat of 30° was applied, and no water was introduced. The carbonate lost no less than 16 *per cent*. The shell, particularly the

* Turbo terebra, LIN.

the sharp end of the periwinkle, retained its original shape in a great measure, so as to be quite discernible; but the whole was glazed over with a truly vitreous lustre. This glaze covered, at one place, a fragment of the shell which had been originally loose, and had welded the two together. All the angles are rounded by this vitrification; the space between the entire shell and the fragment being filled, and the angles of their meeting rounded, with this shining substance. The colour is a pale blue, contrasted, in the same little glass, with a natural piece of periwinkle, which is of a reddish-yellow. One of the fragments had adhered to the lid, and had been converted into a complete drop, of the size of a mustard-feed. It is fixed on the wax (at *b*), along with the other specimens of the experiment (fig. 32.). This result shews, as yet, no sign of decay, notwithstanding so great a loss of weight.

THE last experiment was repeated on the same day, and prepared in the same manner, with large fragments of shell, and the point of the periwinkle standing up in the cup. A heat of 34° was applied; a loss took place of 13° *per cent*. All the original form had disappeared, the carbonate lying in the cup as a complete liquid, with a concave surface, which did not shine, but was studded all over with the white sphericles or tufts, like those seen in the former results, without any space between them. When detached from the cup, the surface moulded on the platina, was white and pearly, with a slight gloss. The mass was quite solid; no vestige whatever appearing, of the original form of the fragments, (fig. 33.). A small piece, broken off near the apex of the cone, shewed the internal structure to be quite saline. In the act of arranging the specimen on its stand, another piece came off in a new direction, which presented to view the most perfect crystalline arrangement: the shining plane extended across the whole specimen, and was more than the tenth of an inch in all directions. This fracture, likewise,

likewise, shewed the entire internal solidity of the mass. Unfortunately, this specimen has suffered much by the same decay to which all of them are subject which have lost any considerable weight. The part next the outward surface alone remains entire. I have never been able to explain, in a satisfactory manner, this difference of durability; the last-mentioned result having lost more in proportion to its weight than this.

ABOUT the beginning of June, I received from Mr HATCHETT some pure carbonate of lime, which he was so good as to prepare, with a view to my experiments; and I have been constantly employed with it till within these few days.

MY first experiments with this substance were peculiarly unfortunate, and it seemed to be less easily acted upon than any substance of the kind I had tried. Its extreme purity, no doubt, contributed much to this, though another circumstance had likewise had some effect. The powder, owing to a crystallization which had taken place on its precipitation, was very coarse, and little susceptible of close ramming; the particles, therefore, had less advantage than when a fine powder is used, in acting upon each other, and I did not choose to run any risk of contamination, by reducing the substance to a finer powder. Whatever be the cause, it is certain, that in many experiments in which the chalk was changed to marble, this substance remained in a loose and brittle state, though consisting generally of clear and shining particles. I at last, however, succeeded in obtaining some very good results with this carbonate.

IN an experiment made with it on the 18th of June, in a strong heat, I obtained a very firm mass with a saline fracture, moulded in several places on the platina, which was now used in the cylindrical form. On the 23d, in a similar experiment, the barrel failed, and the subject of experiment was found in an entire state of froth, proving its former fluidity.

ON the 25th, in a fimilar experiment, a heat of 64° was applied without any water within the barrel. The platina tube, (having been contaminated in a former experiment with some fusible metal), melted, and the carbonate retaining its cylindrical shape, had fallen through it, so as to touch the piece of porcelain which had been placed next to the platina tube. At the point of contact, the two had run together, as a hot iron runs when touched by sulphur. The carbonate itself was very transparent, resembling a piece of snow in the act of melting.

ON the 26th of June, I made an experiment with this carbonate, which afforded a beautiful result. One grain of water was introduced with great care; yet there was a loss of 6.5 *per cent.*, and the result has fallen to decay. The pyrometer indicated 43° . On the outside of the platina cylinder, and on one of the lids, were seen a set of globules, like pearls, as once before obtained, denoting perfect fusion. When the upper lid was removed, the substance was found to have sunk almost out of sight, and had assumed a form not easily described. (I have endeavoured to represent it in fig. 31. by an ideal section of the platina-tube and its contents, made through the axis of the cylinder). The powder, first shrinking upon itself in the act of agglutination, had formed a cylindrical rod, a remnant of which (*abc*) stood up in the middle of the tube. By the continued action of heat, the summit of the rod (at *a*) had been rounded in fusion, and the mass being now softened, had sunk by its weight, and spread below, so as to mould itself in the tube, and fill its lower part completely (*dfge*). At the same time, the viscid fluid adhering to the sides (at *e* and *d*), while the middle part was sinking, had been in part left behind, and in part drawn out into a thin but tapering shape, united by a curved surface (at *b* and *c*) to the middle rod. When the platina tube was unwrapped, the thin edges (at *e* and *d*) were preserved all round, and in a state

state of beautiful semitransparency. (I have attempted to represent the entire specimen, as it stood on its cone of wax, in fig. 34.). The carbonate, where moulded on the platina, had a clean pearly whiteness, with a saline appearance externally, and in the sun, shone with facettes. Its surface was interrupted by a few scattered air-bubbles, which had lain against the tube. The intervening substance was unusually compact and hard under the knife. The whole surface (*e b a c d*, fig. 31.), and the inside of the air-bubbles, had a vitreous lustre. Thus, every thing denoted a state of viscid fluidity, like that of honey.

THESE last experiments seem to obviate every doubt that remained with respect to the fusibility of the purest carbonate, without the assistance of any foreign substance.

VII.

Measurement of the Force required to constrain the Carbonic Acid.—Apparatus with the Muzzle of the Barrel upwards, and the weight acting by a long Lever.—Apparatus with the Muzzle downwards.—Apparatus with Weight acting directly on the barrel.—Comparison of various results.

IN order to determine, within certain limits at least, what force had been exerted in the foregoing experiments, and what was necessary to ensure their success, I made a number of experiments, in a mode nearly allied to that followed by Count RUMFORD, in measuring the explosive force of gunpowder.

I BEGAN to use the following simple apparatus in June 1803. I took one of the barrels, made as above described, for the purpose of compression, having a bore of 0.75 of an

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inch,

inch *, and dressed its muzzle to a sharp edge. To this barrel was firmly screwed a collar of iron (*aa*, fig. 36.) placed at a distance of about three inches from the muzzle, having two strong bars (*bb*) projecting at right angles to the barrel, and dressed square. The barrel, thus prepared, was introduced, with its breech downwards, into the vertical muffle (fig. 35.); its length being so adjusted, that its breech should be placed in the strongest heat; the two projecting bars above described, resting on two other bars (*cc*, fig. 35.) laid upon the furnace to receive them; one upon each side of the muffle. Into the barrel, so placed, was introduced a cradle, containing carbonate, with all the arrangements formerly mentioned; the rod connected with it being of such length, as just to lie within the muzzle of the barrel. The liquid metal was then poured in till it filled the barrel, and stood at the muzzle with a convex surface; a cylinder of iron, of about an inch in diameter, and half an inch thick, was laid on the muzzle (fig. 35. and 37.), and to it a compressing weight was instantly applied. This was first done by the pressure of a bar of iron (*de*, fig. 35.), three feet in length, introduced loosely into a hole (*d*), made for the purpose in the wall against which the furnace stood; the distance between this hole and the barrel being one foot. A weight was then suspended at the extremity of the bar (*e*), and thus a compressing force was applied, equal to three times that weight. In the course of practice, a cylinder of lead was substituted for that of iron, and a piece of leather was placed between it and the muzzle of the barrel, which last being dressed to a pretty sharp edge, made an impression in the lead: to assist this effect, one smart blow of a hammer was struck upon the bar, directly over the barrel, as soon as the weight had been hung on.

IT

* This was the size of barrel used in all the following experiments, where the fact is not otherwise expressed.

It was essential, in this mode of operation, that the whole of the metal should continue in a liquid state during the action of heat ; but when I was satisfied as to its intensity and duration, I congealed the metal, either by extinguishing the furnace entirely, or by pouring water on the barrel. As soon as the heat began to act, drops of metal were seen to force themselves between the barrel and the leather, following each other with more or less rapidity, according to circumstances. In some experiments, there was little exudation ; but few of them were entirely free from it. To save the metal thus extruded, I placed a black-lead crucible, having its bottom perforated, round the barrel, and luted close to it, (fig. 37.) ; some sand being laid in this crucible, the metal was collected on its surface. On some occasions, a sound of ebullition was heard during the action of heat ; but this was a certain sign of failure.

THE results of the most important of these experiments, have been reduced to a common standard in the second table placed in the Appendix ; to which reference is made by the following numbers.

NO. I.—ON the 16th of June 1803, I made an experiment with these arrangements. I had tried to use a weight of 30 lb. producing a pressure of 90 lb., but I found this not sufficient. I then hung on a weight of 1 cwt., or 112 lb. ; by which a compressing force was applied of 3 cwt. or 336 lb. Very little metal was seen to escape, and no sound of ebullition was heard. The chalk in the body of the large tube was reduced to quicklime ; but what lay in the inner tube was pretty firm, and effervesced to the last. One or two facettes, of good appearance, were likewise found. The contents of the small tube had lost but 2.6 *per cent.* ; but there was a small visible intrusion of metal, and the result, by its appearance, indicated a greater loss. I considered this, however, as one point gained ; that being the first

first tolerable compression accomplished by a determinate force. The pyrometer indicated 22° .

THIS experiment was repeated the same day, when a still smaller quantity of metal escaped at the muzzle; but the barrel had given way below, in the manner of those that have yielded for want of sufficient air. Even this result was satisfactory, by shewing that a mechanical power, capable of forcing some of the barrels, could now be commanded. The carbonate in the little tube had lost 20 *per cent.*; but part of it was in a hard and firm state, effervescing to the last.

NO. 2.—ON the 21st June, I made an experiment with another barrel, with the same circumstances. I had left an empty space in the large tube, and had intended to introduce its muzzle downwards, meaning that space to answer as an air-tube; but it was inverted by mistake, and the tube entering with its muzzle upwards, the empty space had of course filled with metal, and thus the experiment was made without any included air. There was no pyrometer used; but the heat was guessed to be about 25° where the subject of experiment lay. The barrel, when opened, was found full of metal, and the cradle being laid flat on the table, a considerable quantity of metal ran from it, which had undoubtedly been lodged in the vacuity of the large tube. When cold, I found that vacuity still empty, with a plating of metal. The tube was very clean to appearance, and, when shaken, its contents were heard to rattle. Above the little tube, and the cylinder of chalk, I had put some borax and sand, with a little pure borax in the middle, and chalk over it. The metal had not penetrated beyond the borax and sand, by a good fortune peculiar to this experiment; the intrusion of metal in this mode of execution, being extremely troublesome. The button of chalk, was found in a state of clean white carbonate, and pretty hard, but without transparency. The little tube

tube was perfectly clean. Its weight with its contents, seemed to have suffered no change from what it had been when first introduced. Attending, however, to the balance with scrupulous nicety, a small preponderance did appear on the side of the weight. This was done away by the addition of the hundredth of a grain to the scale in which the carbonate lay, and an addition of another hundredth produced in it a decided preponderance. Perhaps, had the tube, before its introduction, been examined with the same care, as great a difference might have been detected; and it seems as if there had been no loss, at least not more than one hundredth of a grain, which on 10.95 grains, amounts to 0.0912, say 0.1 *per cent*. The carbonate was loose in the little tube, and fell out by shaking. It had a yellow colour, and compact appearance, with a stony hardness under the knife, and a stony fracture; but with very slight facettes, and little or no transparency. In some parts of the specimen, a whitish colour seemed to indicate partial calcination. On examining the fracture, I perceived, with the magnifier, a small globule of metal, not visible to the naked eye, quite insulated and single. Possibly the substance may have contained others of the same sort, which may have compensated for a small loss, but there could not be many such, from the general clean appearance of the whole. In the fracture, I saw here and there small round holes, seeming, though imperfectly, to indicate a beginning of ebullition.

I MADE a number of experiments in the same manner, that is to say, with the muzzle of the barrel upwards, in some of which I obtained very satisfactory results; but it was only by chance that the substance escaped the contamination of the fusible metal; which induced me to think of another mode of applying the compressing weight with the muzzle of the barrel downwards, by which I expected to repeat, with a determinate weight, all the experiments formerly made

made in barrels closed by congealed metal ; and that, by making use of an air-tube, the air, rising to the breech, would secure the contents of the tube from any contamination. In this view, the barrel was introduced from below into the muffle with its breech upwards, and retained in that position by means of a hook fixed to the furnace, till the collar was made to press up against the grate, by an iron lever, loaded with a weight, and resting on a support placed in front. In some experiments made in this way, the result was obtained very clean, as had been expected ; but the force had been too feeble, and when it was increased, the furnace yielded upwards by the mechanical strain.

I FOUND it therefore necessary to use a frame of iron, (as in fig. 38. ; the frame being represented separately in fig. 39.), by which the brick-work was relieved from the mechanical strain. This frame consisted of two bars (*ab* and *fe*, figs. 38. and 39.), fixed into the wall, (at *a* and *f*,) passing horizontally under the furnace, one on each side of the muffle, turning downwards at the front, (in *b* and *e*), and meeting at the ground, with a flat bar (*cd*) uniting the whole. In this manner, a kind of stirrup (*bcd e*) was formed in front of the furnace, upon the cross bar (*cd*) of which a block of wood (*bb*, fig. 38.), was placed, supporting an edge of iron, upon which the lever rested ; the working end of the lever (*g*) acting upwards. A strain was exerted, by means of the barrel and its collar, against the horizontal bars, (*ab* and *fe*), which was effectually resisted by the wall (at *a* and *f*) at one end of these bars, and by the upright bars (*cb* and *de*) at the other end. In this manner the whole strain was sustained by the frame, and the furnace stood without injury.

THE iron bar, at its working end, was formed into the shape of a cup, (at *g*), and half filled with lead, the smooth surface of which, was applied to the muzzle of the barrel. The lever, too, was lengthened, by joining to the bar of iron, a beam of wood, making

making the whole ten feet in length. In this manner, a pressure upwards was applied to the barrel, equal to the weight of 10 cwt.

IN the former method, in which the barrel stood with its muzzle upwards, the weight was applied while the metal was liquid. In this case, it was necessary to let it previously congeal, otherwise the contents would have run out in placing the barrel in the muffle, and to allow the liquefaction essential to these trials, to be produced by the propagation of heat from the muffle downwards. This method required, therefore, in every case, the use of an air-tube; for without it, the heat acting upon the breech, while the metal at the muzzle was still cold, would infallibly have destroyed the barrel. A great number of these experiments failed, with very considerable waste of the fusible metal, which, on these occasions was nearly all lost. But a few of them succeeded, and afforded very satisfactory results, which I shall now mention.

IN November 1803, some good experiments were made in this way, all with a bore of 0.75, and a pressure of 10 cwt.

No. 3.—ON the 19th, a good limestone was obtained in an experiment made in a temperature of 21° , with a loss of only 1.1 per cent.

No. 4.—ON the 22d, in a similar experiment, there was little exudation by the muzzle. The pyrometer gave 31° . The carbonate was in a porous, and almost frothy state.

No. 5.—IN a second experiment, made the same day, the heat rose to 37° or 41° . The substance bore strong marks of fusion, the upper part having spread on the little tube: the whole was very much shrunk, and run against one side. The mass sparkling and white, and in a very good state.

No. 6.—ON the 25th, an experiment was made with chalk, and some fragments of snail shell, with about half a grain of water. The heat had risen to near 51° or 49° . The barrel had been

L

held

held tight by the beam, but was rent and a little swelled at the breech. The rent was wide, and such as has always appeared in the strongest barrels when they failed. The carbonate was quite calcined, it had boiled over the little tube, and was entirely in a frothy state, with large and distinctly rounded air-holes. The fragments of shell which had occupied the upper part of the little tube, had lost every trace of their original shape in the act of ebullition and fusion.

No. 7.—ON the 26th a similar experiment was made, in which the barrel was thrown open, in spite of this powerful compressing force, with a report like that of a gun, (as I was told, not having been present), and the bar was found in a state of strong vibration. The carbonate was calcined, and somewhat frothy, the heart of one piece of chalk used was in a state of saline marble.

It now occurred to me to work with a compressing force, and no air-tube, trusting, as happened accidentally in one case, that the expansion of the liquid would clear itself by gentle exudation, without injury to the carbonate. In this mode, it was necessary, for reasons lately stated, to place the muzzle upwards. Various trials made thus, at this time, afforded no remarkable results. But I resumed the method, with the following alteration in the application of the weight, on the 27th of April 1804.

I CONCEIVED that some inconvenience might arise from the mode of employing the weight in the former experiments. In them it had been applied at the end of the bar, and its effect propagated along it, so as to press against the barrel at its other extremity. It occurred to me, that the propagation of motion in this way, requiring some sensible time, a considerable quantity of carbonic acid might escape by a sudden eruption, before that propagation had taken effect. I therefore thought, that more effectual work might be done, by
placing

placing a heavy mass, (fig. 40.), so as to act directly and simply upon the muzzle of the barrel; this mass being guided and commanded by means of a powerful lever, (*a b*). For this purpose, I procured an iron roller, weighing 3 cwt. 7 lb., and suspended it over the furnace, to the end of a beam of wood, resting on a support near the furnace, with a long arm guided by a rope (*c c*) and pulley (*d*), by which the weight could be raised or let down at pleasure.

WITH this apparatus I made some tolerable experiments; but I found the weight too light to afford certain and steady results of the best quality. I therefore procured at the foundry a large mass of iron (*f*), intended, I believe, for driving piles, and which, after allowing for the counterpoise of the beam, gave a direct pressure of 8.1 cwt.; and I could, at pleasure, diminish the compressing force, by placing a bucket (*e*) at the extremity of the lever, into which I introduced weights, whose effect on the ultimate great mass, was known by trial. Many barrels failed in these trials: at last, I obtained one of small bore, inch 0.54, which gave two good results on the 22d of June 1804.

NO. 8.—WISHING to ascertain the least compressing force by which the carbonate could be effectually constrained in melting heats, I first observed every thing standing firm in a heat of above 20° ; I then gradually threw weights into the bucket, till the compressing force was reduced to 2 cwt. Till then, things continued steady; but, on the pressure being still further diminished, metal began to ooze out at the muzzle, with increasing rapidity. When the pressure was reduced to $1\frac{1}{2}$ cwt. air rushed out with a hissing noise. I then stopped the experiment, by pouring water on the barrel. The piece of chalk had lost 12 *per cent*. It was white and soft on the outside, but firm and good in the heart.

No. 9.—AN experiment was made with chalk, in a little tube ; to this, one grain of water was added. I had intended to work with 4 cwt. only ; but the barrel was no sooner placed, than an exudation of metal began at the muzzle, owing, doubtless, to the elasticity of the water. I immediately increased the pressure to 8.1 cwt. by removing the weight from the bucket, when the exudation instantly ceased. I continued the fire for three quarters of an hour, during which time no exudation happened ; then all came out remarkably clean, with scarcely any contamination of metal. The loss amounted to 2.58 *per cent.* The substance was tolerably indurated, but had not acquired the character of a complete stone.

IN these two last experiments, the bore being small, a pyrometer could not be admitted.

ON the 5th of July 1804, I made three very satisfactory experiments of this kind, in a barrel with the large bore of 0.75 of an inch.

No. 10.—WAS made with a compressing force of only 3 cwt. A small eruption at the muzzle being observed, water was thrown on the barrel : the pyrometer gave 21° : the chalk was in a firm state of limestone.

No. 11.—WITH 4 cwt. The barrel stood without any eruption or exudation, till the heat rose to 25°. There was a loss of 3.6 *per cent.* : the result was superior, in hardness and transparency, to the last, having somewhat of a saline fracture.

No. 12.—WITH 5 cwt. The result, with a loss of 2.4 *per cent.*, was of a quality superior to any of those lately obtained.

THESE experiments appear to answer the end proposed, of ascertaining the least pressure, and lowest heat, in which limestone can be formed. The results, with various barrels of different sizes, agree tolerably, and tend to confirm each other. The table shews, when we compare numbers 1, 2, 8, 10, 11, 12, That a pressure of 52 atmospheres, or 1700 feet of sea, is capable

capable of forming a limestone in a proper heat : That under 86 atmospheres, answering nearly to 3000 feet, or about half a mile, a complete marble may be formed : and lastly, That with a pressure of 173 atmospheres, or 5700 feet, that is, little more than one mile of sea, the carbonate of lime is made to undergo complete fusion, and to act powerfully on other earths.

VIII.

Formation of Coal.—Accidental occurrence which led me to undertake these Experiments.—Results extracted from a former publication.—Explanation of some difficulties that have been suggested.—The Fibres of Wood in some cases obliterated, and in some preserved under compression.—Resemblance which these Results bear to a series of Natural Substances described by Mr HATCHETT.—These results seem to throw light on the history of Surturbrand.

As I intend, on some future occasion, to resume my experiments with inflammable substances, which I look upon as far from complete, I shall add but a few observations to what I have already laid before this Society, in the sketch I had the honour to read in this place on the 30th of August last.

THE following incidental occurrence led me to enter upon this subject rather prematurely, since I had determined first to satisfy myself with regard to the carbonate of lime.

OBSERVING, in many of the last-mentioned class of experiments, that the elastic matters made their escape between the muzzle of the barrel and the cylinder of lead, I was in the habit, as mentioned above, of placing a piece of leather between the lead and the barrel ; in which position, the heat to which the leather was exposed, was necessarily below that of melting lead.

lead. In an experiment, made on the 28th November 1803, in order to ascertain the power of the machinery, and the quantity of metal driven out by the expansion of the liquid, there being nothing in the barrel but metal, I observed, as soon as the compressing apparatus was removed, (which on this occasion was done while the lower part of the barrel was at its full heat, and the barrel standing brim full of liquid metal,) that all the leather which lay on the outside of the circular muzzle of the barrel, remained, being only a little browned and crumpled by the heat to which it had been exposed. What leather lay within the circle, had disappeared; and, on the surface of the liquid metal, which stood up to the lip of the barrel, I saw large drops, of a shining black liquid, which, on cooling, fixed into a crisp black substance, with a shining fracture, exactly like pitch or pure coal. It burned, though not with flame. While hot, it smelt decidedly of volatile alkali. The important circumstance here, is the different manner in which the heat has acted on the leather, without and within the rim of the barrel. The only difference consisted in compression, to which, therefore, the difference of effect must be ascribed: by its force, the volatile matter of the leather which escaped from the outward parts, had within the rim, been constrained to remain united to the rest of the composition, upon which it had acted as a flux, and the whole together had entered into a liquid state, in a very low heat. Had the pressure been continued till all was cool, these substances must have been retained, producing a real coal.

ON the 24th April 1803, a piece of leather used in a similar manner, (the compressing force being continued, however, till all was cold,) was changed to a substance like glue, owing doubtless to compression, in a heat under that of melting lead.

THESE observations led me to make a series of experiments with animal and vegetable substances, and with coal; the

the result of which I have already laid before the Society. I shall now repeat that communication, as printed in NICHOLSON'S *Journal* for October last (1804).

“ I HAVE likewise made some experiments with coal, treated in the same manner as the carbonate of lime : but I have found it much less tractable ; for the bitumen, when heat is applied to it, tends to escape by its simple elasticity, whereas the carbonic acid in marble, is in part retained by the chemical force of quicklime. I succeeded, however, in constraining the bituminous matter of the coal, to a certain degree, in red heats, so as to bring the substance into a complete fusion, and to retain its faculty of burning with flame. But, I could not accomplish this in heats capable of agglutinating the carbonate ; for I have found, where I rammed them successively into the same tube, and where the vessel has withstood the expansive force, that the carbonate has been agglutinated into a good limestone, but that the coal has lost about half its weight, together with its power of giving flame when burnt, remaining in a very compact state, with a shining fracture. Although this experiment has not afforded the desired result, it answers another purpose admirably well. It is known, that where a bed of coal is crossed by a dike of whinstone, the coal is found in a peculiar state in the immediate neighbourhood of the whin : the substance in such places being incapable of giving flame, it is distinguished by the name of *blind coal*. Dr HUTTON has explained this fact, by supposing that the bituminous matter of the coal, has been driven by the local heat of whin, into places of less intensity, where it would probably be retained by distillation. Yet the whole must have been carried on under the action of a pressure capable of constraining the carbonic acid of the calcareous spar, which occurs frequently in such rocks. In the last-mentioned experiment, we have a perfect representation

tation of the natural fact; since the coal has lost its petroleum, while the chalk in contact with it has retained its carbonic acid.

“ I HAVE made some experiments of the same kind, with vegetable and animal substances. I found their volatility much greater than that of coal, and I was compelled, with them, to work in heats below redness; for, even in the lowest red-heat, they were apt to destroy the apparatus. The animal substance I commonly used was horn, and the vegetable, saw-dust of fir. The horn was incomparably the most fusible and volatile of the two. In a very slight heat, it was converted into a yellow red substance, like oil, which penetrated the clay tubes through and through. In these experiments, I therefore made use of tubes of glass. It was only after a considerable portion of the substance had been separated from the mass, that the remainder assumed the clear black peculiar to coal. In this way I obtained coal, both from saw-dust and from horn, which yielded a bright flame in burning.

“ THE mixture of the two produced a substance having exactly the smell of foot or coal-tar. I am therefore strongly inclined to believe, that animal substance, as well as vegetable, has contributed towards the formation of our bituminous strata. This seems to confirm an opinion, advanced by Mr KEIR, which has been mentioned to me since I made this experiment. I conceive, that the coal which now remains in the world, is but a small portion of the organic matter originally deposited: the most volatile parts have been driven off by the action of heat, before the temperature had risen high enough to bring the surrounding substance into fusion, so as to confine the elastic fluids, and subject them to compression.

“ IN several of these experiments, I found that, when the pressure was not great, when equal, for instance, only to 80 atmospheres, that the horn employed was dissipated entirely, the
glass

glass tube which had contained it being left almost clean : yet undoubtedly, if exposed to heat without compression, and protected from the contact of the atmosphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here, then, it would seem as if the moderate pressure, by keeping the elements of the substance together, had promoted the general volatility, without being strong enough to resist that expansive force, and thus, that the whole had escaped. This result, which I should certainly not have foreseen in theory, may perhaps account for the absence of coal in situations where its presence might be expected on principles of general analogy.”

SINCE this publication, a very natural question has been put to me. When the inflammable substance has lost weight, or when the whole has been dissipated, in these experiments, what has become of the matter thus driven off?

I MUST own, that to answer this question with perfect confidence, more experiments are required. But, in the course of practice, two circumstances have occurred as likely, in most cases, to have occasioned the loss alluded to. I found in these experiments, particularly with horn, that the chalk, both in powder and in lump, which was used to fill vacuities in the tubes, and to fix them in the cradle, was strongly impregnated with an oily or bituminous matter, giving to the substance the qualities of a stinkstone. I conceive, that the most volatile part of the horn has been conveyed to the chalk, partly in a state of vapour, and partly by boiling over the lips of the glass tube ; the whole having been evidently in a state of very thin fluidity. Having, in some cases, found the tube, which had been introduced full of horn, entirely empty after the experiment, I was induced, as above stated, to conceive, that, under pressure, it had acquired a greater general volatility than it had in free-

dom; and I find that, in the open fire, horn yields a charcoal equal to 20 *per cent.* of the original weight. But more experiments must be made on this subject.

ANOTHER cause of the loss of weight, lay undoubtedly in the excess of heat employed in most of them, to remove the cradle from the barrel. With inflammable substances, no air-tube was used, and the heats being low, the air lodged in interstices had been sufficient to secure the barrels from destruction, by the expansion of the liquid metal. In this view, likewise, I often used lead, whose expansion in such low heats, I expected to be less than that of the fusible metal. And the lead requiring to melt it, a heat very near to that of redness, the subject of experiment was thus, on removing the cradle, exposed in freedom to a temperature which was comparatively high. But, observing that a great loss was thus occasioned, I returned to the use of the fusible metal, together with my former method of melting it, by plunging the barrel, when removed from the furnace, into a solution of muriate of lime, by which it could only receive a heat of 250° of FAHRENHEIT.

THE effect was remarkable, in the few experiments tried in this way. The horn did not, as in the other experiments, change to a hard black substance, but acquired a semifluid and viscid consistency, with a yellow-red colour, and a very offensive smell. This shews, that the substances which here occasioned both the colour and smell of the results, had been driven off in the other experiments, by the too great heat applied to the substance, when free from compression.

I FOUND that the organization of animal substance was entirely obliterated by a slight action of heat, but that a stronger heat was required to perform the entire fusion of vegetable matter. This, however, was accomplished; and in several experiments, pieces of wood were changed to a jet-black and inflammable substance, generally very porous, in which no
trace

trace could be discovered of the original organization. In others, the vegetable fibres were still visible, and are forced asunder by large and shining air-bubbles.

SINCE the publication of the sketch of my experiments, I have had the pleasure to read Mr HATCHETT's very interesting account of various natural substances, nearly allied to coal; and I could not help being struck with the resemblance which my results bear to them, through all their varieties, as brought into view by that able chemist; that resemblance affording a presumption, that the changes which, with true scientific modesty, he ascribes to an unknown cause, may have resulted from various heats acting under pressure of various force. The substance to which he has given the name of *Retinasphaltum*, seems to agree very nearly with what I have obtained from animal substance, when the barrel was opened by means of low heat. And the specimen of wood entering into fusion, but still retaining the form of its fibres, seems very similar to the intermediate substance of Bovey-coal and *Surturbrand*, which Mr HATCHETT has assimilated to each other. It is well known, that the *surturbrand* of Iceland, consists of the stems of large trees, flattened to thin plates, by some operation of nature hitherto unexplained. But the last-mentioned experiment seems to afford a plausible solution of this puzzling phenomenon.

IN all parts of the globe, we find proofs of slips, and various relative motions, having taken place amongst great masses of rock, whilst they were soft in a certain degree, and which have left unequivocal traces behind them, both in the derangement of the beds of strata, and in a smooth and shining surface, called *slickenside*, produced by the direct friction of one mass on another. During the action of subterranean heat, were a single stratum to occur, containing trees intermixed with animal substances, shell-fish, &c. these trees would be reduced, to a soft and unctuous state, similar to that of the piece of wood

in the last-mentioned experiment, whilst the substance of the contiguous strata retained a considerable degree of firmness. In this state of things, the stratum just mentioned, would very naturally become the scene of a slip, occasioned by the unequal pressure of the surrounding masses. By such a sliding motion, accompanied by great compression, a tree would be flattened, as any substance is ground in a mortar, by the combination of a lateral and direct force. At the same time, the shells along with the trees, would be flattened, like those described by BERGMAN; while those of the same species in the neighbouring limestone-rock, being protected by its inferior fusibility, would retain their natural shape.

IX.

Application of the foregoing results to Geology.—The fire employed in the Huttonian Theory is a modification of that of the Volcanoes.—This modification must take place in a lava previous to its eruption.—An Internal Lava is capable of melting Limestone.—The effects of Volcanic Fire on substances in a subterranean and submarine situation, are the same as those ascribed to Fire in the Huttonian Theory.—Our Strata were once in a similar situation, and then underwent the action of fire.—All the conditions of the Huttonian Theory being thus combined, the formation of all Rocks may be accounted for in a satisfactory manner.—Conclusion.

HAVING investigated, by means of the foregoing experiments, some of the chemical suppositions involved in the Huttonian Theory, and having endeavoured to assign a determinate limit to the power of the agents employed; I shall now apply these results to Geology, and inquire how far the events supposed

supposed anciently to have taken place, accord with the existing state of our globe.

THE most powerful and essential agent of the Huttonian Theory, is Fire, which I have always looked upon as the same with that of volcanoes, modified by circumstances which must, to a certain degree, take place in every lava previous to its eruption.

THE original source of internal fire is involved in great obscurity; and no sufficient reason occurs to me for deciding whether it proceeds by emanation from some vast central reservoir, or is generated by the local operation of some chemical process. Nor is there any necessity for such a decision: all we need to know is, that internal fire exists, which no one can doubt, who believes in the eruptions of Mount Vesuvius. To require that a man should account for the generation of internal fire, before he is allowed to employ it in geology, is no less absurd than it would be to prevent him from reasoning about the construction of a telescope, till he could explain the nature of the sun, or account for the generation of light *. But while we remain in suspense as to the prime cause of this tremendous agent, many circumstances of importance with regard to it, may fairly become the subjects of observation and discussion.

SOME authors (I conceive through ignorance of the facts) have alleged, that the fire of *Ætna* and *Vesuvius* is merely superficial. But the depth of its action is sufficiently proved, by the great distance to which the eruptive percussions are felt, and still more, by the substances thrown out uninjured by some eruptions.

* THIS topic, however, has of late been much urged against us, and an unfair advantage has been taken of what Mr PLAYFAIR has said upon it. What he gave as mere conjecture on a subject of collateral importance, has been argued upon as the basis and fundamental doctrine of the system.

eruptions of Mount Vesuvius. Some of these, as marble and gypsum, are incapable in freedom of resisting the action of fire. We have likewise granite, schistus, gneiss, and stones of every known class, besides many which have never, on any other occasion, been found at the surface of our globe. The circumstance of these substances having been thrown out, unaffected by the fire, proves, that it has proceeded from a source, not only as deep, but deeper, than their native beds; and as they exhibit specimens of every class of minerals, the formation of which we pretend to explain, we need inquire no further into the depth of the Vesuvian fire, which has thus been proved to reach below the range of our speculations.

VOLCANIC fire is subject to perpetual and irregular variations of intensity, and to sudden and violent renewal, after long periods of absolute cessation. These variations and intermissions, are likewise essential attributes of fire as employed by Dr HUTTON; for some geological scenes prove, that the indurating cause has acted repeatedly on the same substance, and that, during the intervals of that action, it had ceased entirely. This circumstance affords a complete answer to an argument lately urged against the Huttonian Theory, founded on the waste of heat which must have taken place, as it is alleged, through the surface. For if, after absolute cessation, a power of renewal exists in nature, the idea of waste by continuance is quite inapplicable.

THE external phenomena of volcanoes are sufficiently well known; but our subject leads us to inquire into their internal actions. This we are enabled to do by means of the foregoing experiments, in so far as the carbonate of lime is concerned.

SOME experiments which I formerly * laid before this Society and the public, combined with those mentioned in this paper,

* *Edinburgh Transactions*, Vol. V. Part I. p. 60—66.

paper, prove, that the feeblest exertions of volcanic fire, are of sufficient intensity to perform the agglutination, and even the entire fusion, of the carbonate of lime, when its carbonic acid is effectually confined by pressure; for though lava, after its fusion, may be made, in our experiments, to congeal into a glass, in a temperature of 16° or 18° of WEDGWOOD, in which temperature the carbonate would scarcely be affected; it must be observed, that a similar congelation is not to be looked for in nature; for the mass, even of the smallest stream of lava, is too great to admit of such rapid cooling. And, in fact, the external part of a lava is not vitreous, but consists of a substance which, as my experiments have proved, must have been congealed in a heat of melting silver, that is, in 22° of WEDGWOOD; while its internal parts bear a character indicating that they congealed in 27° or 28° of the same scale. It follows, that no part of the lava, while it remained liquid, can have been less hot than 22° of WEDGWOOD. Now, this happens to be a heat, in which I have accomplished the entire fusion of the carbonate of lime, under pressure. We must therefore conclude, that the heat of a running lava is always of sufficient intensity to perform the fusion of limestone.

IN every active volcano, a communication must exist between the summit of the mountain and the unexplored region, far below its base, where the lava has been melted, and whence it has been propelled upwards; the liquid lava rising through this internal channel, so as to fill the crater to the brim, and flow over it. On this occasion, the sides of the mountain must undergo a violent hydrostatical pressure outwards, to which they often yield by the formation of a vast rent, through which the lava is discharged in a lateral eruption, and flows in a continued stream sometimes during months. On *Ætna* most of the eruptions are so performed; few lavas flowing from the summit, but generally breaking out laterally, at very elevated stations.

At

At the place of delivery, a quantity of gaseous matter is propelled violently upwards, and, along with it, some liquid lava; which last, falling back again in a spongy state, produces one of those conical hills which we see in great number on the vast sides of Mount *Ætna*, each indicating the discharge of a particular eruption. At the same time, a jet of flame and smoke issues from the main crater, proving the internal communication between it and the lava; this discharge from the summit generally continuing, in a greater or a less degree, during the intervals between eruptions. (Fig. 41. represents an ideal section of Mount *Ætna*; *ab* is the direct channel, and *bc* is a lateral branch).

LET us now attend to the state of the lava within the mountain, during the course of the eruption; and let us suppose, that a fragment of limestone, torn from some stratum below, has been included in the fluid lava, and carried up with it. By the laws of hydrostatics, as each portion of this fluid sustains pressure in proportion to its perpendicular distance below the point of discharge, that pressure must increase with the depth. The specific gravity of solid and compact lava is nearly 2.8; and its weight, when in a liquid state, is probably little different. The table shews, that the carbonic acid of limestone cannot be constrained in heat by a pressure less than that of 1708 feet of sea, which corresponds nearly to 600 feet of liquid lava. As soon, then, as our calcareous mass rose to within 600 feet of the surface, its carbonic acid would quit the lime, and, assuming a gaseous form, would add to the eruptive effervescence. And this change would commonly begin in much greater depths, in consequence of the bubbles of carbonic acid, and other substances in a gaseous form, which, rising with the lava, and through it, would greatly diminish the weight of the column, and would render its pressure on any particular spot extremely variable. With all these irregularities, however, and interruptions, the
pressure

pressure would in all cases, especially where the depth was considerable, far surpass what it would have been under an equal depth of water. Where the depth of the stream, below its point of delivery, amounted, then, to 1708 feet, the pressure, if the heat was not of excessive intensity, would be more than sufficient to constrain the carbonic acid, and our limestone would suffer no calcination, but would enter into fusion; and if the eruption ceased at that moment, would crystallize in cooling along with the lava, and become a nodule of calcareous spar. The mass of lava, containing this nodule, would then constitute a real whinstone, and would belong to the kind called *amygdaloid*. In greater depths still, the pressure would be proportionally increased, till sulphur, and even water, might be constrained; and the carbonate of lime would continue undecomposed in the highest heats.

If, while the lava was in a liquid state, during the eruption or previous to it, a new rent (*de*, fig. 41.), formed in the solid country below the volcano, was met by our stream (at *d*), it is obvious that the lava would flow into the aperture with great rapidity, and fill it to the minutest extremity, there being no air to impede the progress of the liquid. In this manner, a stream of lava might be led from below to approach the bottom of the sea (*ff*), and to come in contact with a bed of loose shells (*gg*), lying on that bottom, but covered with beds of clay, interstratified, as usually occurs, with beds of sand, and other beds of shells. The first effect of heat would be to drive off the moisture of the lowest shell-bed, in a state of vapour, which, rising till it got beyond the reach of the heat, would be condensed into water, producing a slight motion of ebullition, like that of a vessel of water, when it begins to boil, and when it is said to simmer. The beds of clay and sand might thus undergo some heaving and partial derangement, but would still possess the power of stopping, or of very much im-

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peding,

peding, the descent of water from the sea above; so that the water which had been driven from the shells at the bottom, would not return to them, or would return but slowly; and they would be exposed dry to the action of heat*.

IN this case, one of two things would inevitably happen. Either the carbonic acid of the shells would be driven off by the heat, producing an incondensable elastic fluid, which, heaving up or penetrating the superincumbent beds, would force its way to the surface of the sea, and produce a submarine eruption, as has happened at Santorini and elsewhere; or the volatility of the carbonic acid would be repressed by the weight of the superincumbent water (*k k*), and the shell-bed, being softened or fused by the action of heat, would be converted into a stratum of limestone.

THE foregoing experiments enable us to decide in any particular case, which of these two events must take place, when the heat of the lava and the depth of the sea are known.

THE table shews, that under a sea no deeper than 1708 feet, near one-third of a mile, a limestone would be formed by proper heat; and that, in a depth of little more than one mile, it would enter into entire fusion. Now, the common soundings of mariners extend to 200 fathoms, or 1200 feet. Lord MULGRAVE † found bottom at 4680 feet, or nearly nine-tenths of a mile; and Captain ELLIS let down a sea-gage to the depth of 5346 feet ‡. It thus appears, that

* THIS situation of things, is similar to what happens when small-coal is moistened, in order to make it cake. The dust, drenched with water, is laid upon the fire, and remains long wet, while the heat below suffers little or no abatement.

† *Voyage towards the North Pole*, p. 142.

‡ *Philosophical Transactions*, 1751, p. 212.

that at the bottom of a sea, which would be founded by a line much less than double of the usual length, and less than half the depth of that founded by Lord MULGRAVE, limestone might be formed by heat; and that, at the depth reached by Captain ELLIS, the entire fusion would be accomplished, if the bed of shells were touched by a lava at the extremity of its course, when its heat was lowest. Were the heat of the lava greater, a greater depth of sea would, of course, be requisite to constrain the carbonic acid effectually; and future experiments may determine what depth is required to co-operate with any given temperature. It is enough for our present purpose to have shewn, that the result is possible in any case, and to have circumscribed the necessary force of these agents within moderate limits. At the same time it must be observed, that we have been far from stretching the known facts; for when we compare the small extent of sea in which any foundings can be found, with that of the vast unfathomed ocean, it is obvious, that in assuming a depth of one mile or two, we fall very short of the medium. M. DE LA PLACE, reasoning from the phenomena of the tides, states it as highly probable that this medium is not less than eleven English miles*.

If a great part or the whole of the superincumbent mass consisted, not of water, but of sand or clay, then the depth requisite to produce these effects would be lessened, in the inverse ratio of the specific gravity. If the above-mentioned occurrence took place under a mass composed of stone firmly bound together by some previous operation of nature, the power of the superincumbent mass, in opposing the escape of

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carbonic

* “ On peut donc regarder au moins comme très probable, que la profondeur moyenne de la mer n’est pas au-dessous de quatre lieues.” DE LA PLACE, *Hist. de l’Acad. Roy. des Sciences*, année 1776.

carbonic acid, would be very much increased by that union and by the stiffness or tenacity of the substance. We have seen numberless examples of this power in the course of these experiments, in which barrels, both of iron and porcelain, whose thickness did not exceed one-fourth of an inch, have exerted a force superior to the mere weight of a mile of sea. Without supposing that the substance of a rock could in any case act with the same advantage as that of a uniform and connected barrel; it seems obvious that a similar power must, in many cases, have been exerted to a certain degree.

WE know of many calcareous masses which, at this moment, are exposed to a pressure more than sufficient to accomplish their entire fusion. The mountain of Saleve, near Geneva, is 500 French fathoms, or nearly 3250 English feet, in height, from its base to its summit. Its mass consists of beds, lying nearly horizontal, of limestone filled with shells. Independently, then, of the tenacity of the mass, and taking into account its mere weight, the lowest bed of this mountain, must, at this moment, sustain a pressure of 3250 feet of limestone, the specific gravity of which is about 2.65. This pressure, therefore, is equal to that of 8612 feet of water, being nearly a mile and a half of sea, which is much more than adequate, as we have shewn, to accomplish the entire fusion of the carbonate, on the application of proper heat. Now, were an emanation from a volcano, to rise up under Saleve, and to penetrate upwards to its base, and stop there; the limestone to which the lava approached, would inevitably be softened, without being calcined, and, as the heat retired, would crystallize into a saline marble.

SOME other circumstances, relating to this subject, are very deserving of notice, and enable us still further to compare the ancient and modern operations of fire.

It

It appears, at first sight, that a lava having once penetrated the side of a mountain, all subsequent lavas should continue, as water would infallibly do, to flow through the same aperture. But there is a material difference in the two cases. As soon as the lava has ceased to flow, and the heat has begun to abate, the crevice through which the lava had been passing, remains filled with a substance, which soon agglutinates into a mass, far harder and firmer than the mountain itself. This mass, lying in a crooked bed, and being firmly welded to the sides of the crevice, must oppose a most powerful resistance to any stream tending to pursue the same course. The injury done to the mountain by the formation of the rent, will thus be much more than repaired; and in a subsequent eruption, the lava must force its way through another part of the mountain or through some part of the adjoining country. The action of heat from below, seems in most cases to have kept a channel open through the axis of the mountain, as appears by the smoke and flame which is habitually discharged at the summit during intervals of calm. On many occasions, however, this spiracle seems to have been entirely closed by the consolidation of the lava, so as to suppress all emission. This happened to Vesuvius during the middle ages. All appearance of fire had ceased for five hundred years, and the crater was covered with a forest of ancient oaks, when the volcano opened with fresh vigour in the sixteenth century.

THE eruptive force, capable of overcoming such an obstacle, must be tremendous indeed, and seems in some cases to have blown the volcano itself almost to pieces. It is impossible to see the Mountain of Somma, which, in the form of a crescent, embraces Mount Vesuvius, without being convinced that it is a fragment of a large volcano, nearly concentric
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with the present inner cone, which, in some great eruption, had been destroyed all but this fragment. In our own times, an event of no small magnitude has taken place on the same spot; the inner cone of Vesuvius having undergone so great a change during the eruption in 1794, that it now bears no resemblance to what it was when I saw it in 1785.

THE general or partial stagnation of the internal lavas at the close of each eruption seems, then, to render it necessary, that in every new discharge, the lava should begin by making a violent laceration. And this is probably the cause of those tremendous earthquakes which precede all great eruptions, and which cease as soon as the lava has found a vent. It seems but reasonable to ascribe like effects to like causes, and to believe that the earthquakes which frequently desolate countries not externally volcanic, likewise indicate the protrusion from below of matter in liquid fusion, penetrating the mass of rock.

THE injection of a whinstone-dike into a frail mass of shale and sandstone, must have produced the same effects upon it that the lava has just been stated to produce on the loose beds of volcanic scoria. One stream of liquid whin, having flowed into such an assemblage, must have given it great additional weight and strength: so that a second stream coming like the first, would be opposed by a mass, the laceration of which would produce an earthquake, if it were overcome; or by which, if it resisted, the liquid matter would be compelled to penetrate some weaker mass, perhaps at a great distance from the first. The internal fire being thus compelled perpetually to change the scene of its action, its influence might be carried to an indefinite extent: So that the intermittance in point of time, as well as the versatility in point of place, already remarked as common to the Huttonian and Volcanic fires, are accounted for on our principles.

ples. And it thus appears, that whinstone possesses all the properties which we are led by theory to ascribe to an internal lava.

THIS connection is curiously illustrated by an intermediate case between the results of external and internal fire, displayed in an actual section of the ancient part of Vesuvius, which occurs in the Mountain of Somma mentioned above. I formerly described this scene in my paper on Whinstone and Lava; and I must beg leave once more to press it upon the notice of the public, as affording to future travellers a most interesting field of geological inquiry.

THE section is seen in the bare vertical cliff, several hundred feet in height, which Somma presents to the view from the little valley, in form of a crescent, which lies between Somma and the interior cone of Vesuvius, called the *Atrio del Cavallo*. (Fig. 42. represents this scene, done from the recollection of what I saw in 1785. *abc* is the interior cone of Vesuvius; *dfg* the mountain of Somma; and *cde* the *Atrio del Cavallo*). By means of this cliff (*fd* in figure 42. and which is represented separately in fig. 44.), we see the internal structure of the mountain, composed of thick beds (*kk*) of loose scoria, which have fallen in showers; between which thin but firm streams (*mm*) of lava are interposed, which have flowed down the outward conical sides of the mountain. (Fig. 43. is an ideal section of Vesuvius and Somma, through the axis of the cones, shewing the manner in which the beds of scoria and of lava lie upon each other; the extremities of which beds are seen edgewise in the cliff at *mm* and *kk*, fig. 42, 43, and 44.).

THIS assemblage of scoria and lava is traversed abruptly and vertically, by streams of solid lava (*nn*, fig. 44.), reaching from top to bottom of the cliff. These last I conceive to have flowed in rents of the ancient mountain, which rents had acted

as pipes through which the lavas of the lateral eruptions were conveyed to the open air. This scene presents to the view of an attentive observer, a real specimen of those internal streams which we have just been considering in speculation, and they may exhibit circumstances decisive of the opinions here advanced. For, if one of these streams had formerly been connected with a lateral eruption, discharged at more than 600 feet above the *Atrio del Cavallo*, it might possibly contain the carbonate of lime. But could we suppose that depth to extend to 1708 feet, the interference of air-bubbles, and the action of a stronger heat than was merely required for the fusion of the carbonate, might have been overcome.

PERHAPS the height of Vesuvius has never been great enough for this purpose. But could we suppose *Ætna* to be cleft in two, and its structure displayed, as that of Vesuvius has just been described, there can be no doubt that internal streams of lava would be laid open, in which the pressure must have far exceeded the force required to constrain the carbonic acid of limestone; since that mountain occasionally delivers lavas from its summit, placed 10.954 feet above the level of the Mediterranean *, which washes its base. I recollect having seen, in some parts of *Ætna*, vast chasms and crags, formed by volcanic revolutions, in which vertical streams of lava, similar to those of *Somma*, were apparent. But my attention not having been turned to that object till many years afterwards, I have only now to recommend the investigation of this interesting point to future travellers.

WHAT has been said of the heat conveyed by internal volcanic streams, applies equally to that deeper and more general heat by which the lavas themselves are melted and propelled upwards.

* *Phil. Transf.* 1777, p. 595.

upwards. That they have been really so propelled, from a great internal mass of matter, in liquid fusion, seems to admit of no doubt, to whatever cause we ascribe the heat of volcanoes. It is no less obvious, that the temperature of that liquid must be of far greater intensity than the lavas, flowing from it, can retain when they reach the surface. Independently of any actual eruption, the body of heat contained in this vast mass of liquid, must diffuse itself through the surrounding substances, the intensity of the heat being diminished by slow gradations, in proportion to the distance to which it penetrates. When, by means of this progressive diffusion, the heat has reached an assemblage of loose marine deposits, subject to the pressure of a great superincumbent weight, the whole must be agglutinated into a mass, the solidity of which will vary with the chemical composition of the substance, and with the degree of heat to which each particular spot has thus been exposed. At the same time, analogy leads us to suppose, that this deep and extensive heat must be subject to vicissitudes and intermissions, like the external phenomena of volcanoes. We have endeavoured to explain some of these irregularities, and a similar reasoning may be extended to the present case. Having shewn, that small internal streams of lava tend successively to pervade every weak part of a volcanic mountain, we are led to conceive, that the great masses of heated matter just mentioned, will be successively directed to different parts of the earth; so that every loose assemblage of matter, lying in a submarine and subterranean situation, will, in its turn, be affected by the indurating cause; and the influence of internal volcanic heat will thus be circumscribed within no limits but those of the globe itself.

A SERIES of undoubted facts prove, that all our strata once lay in a situation similar in all respects to that in which the marine deposits just mentioned have been supposed to lie.

THE inhabitant of an unbroken plain, or of a country formed of horizontal strata, whose observations have been confined

ned to his native spot, can form no idea of those truths, which at every step in an alpine district force themselves on the mind of a geological observer. Unfortunately for the progress of geology, both London and Paris, are placed in countries of little interest; and those scenes by which the principles of this science are brought into view in the most striking manner, are unknown to many persons best capable of appreciating their value. The most important, and at the same time, the most astonishing truth which we learn by any geological observations, is, that rocks and mountains now placed at an elevation of more than two miles above the level of the sea, must at one period have lain at its bottom. This is undoubtedly true of those strata of limestone which contain shells; and the same conclusion must be extended to the circumjacent strata. The imagination struggles against the admission of so violent a position; but must yield to the force of unquestionable evidence; and it is proved by the example of the most eminent and cautious observers, that the conclusion is inevitable*.

ANOTHER question here occurs, which has been well treated by Mr PLAYFAIR. Has the sea retreated from the mountains? or have they risen out of the sea? He has shewn, that the balance of probability is incomparably in favour of the latter supposition; since, in order to maintain the former, we must dispose of an enormous mass of sea, whose depth is several miles, and whose base is greater than the surface of the whole sea. Whereas the elevation of a continent out of a sea like ours, would not change its level above a few feet; and even were a great derangement thus occasioned,

* SAUSSURE, *Voyages dans les Alpes*, tom. ii. p. 99.—104.

fioned, the water would easily find its level without the assistance of any extraordinary supposition. The elevation of the land, too, is evinced by what has occasionally happened in volcanic regions, and affords a complete solution of the contortion and erection of strata, which are almost universally admitted to have once lain in a plane and horizontal position.

WHATEVER opinion be adopted as to the mode in which the land and the water have been separated, no one doubts of the ancient submarine situation of the strata.

AN important series of facts proves, that they were likewise subterranean. Every thing indicates that a great quantity of matter has been removed from what now constitutes the surface of our globe, and enormous deposits of loose fragments, evidently detached from masses similar to our common rock, evince the action of some very powerful agent of destruction. Analogy too, leads us to believe, that all the primary rocks have once been covered with secondary; yet, in vast districts, no secondary rock appears. In short, geologists seem to agree in admitting the general position, that very great changes of this kind have taken place in the solid surface of the globe, however much they may differ as to their amount, and as to their causes.

DR HUTTON ascribed these changes to the action, during very long time, of those agents, which at this day continue slowly to corrode the surface of the earth; frosts, rains, the ordinary floods of rivers, &c. which he conceives to have acted always with the same force, and no more. But to this opinion I could never subscribe, having early adopted that of SAUSURE, in which he is joined by many of the continental geologists. My conviction was founded upon the inspection of those facts in the neighbourhood of Geneva, which he has adduced in support of his opinion. I was then convinced,

and I still believe, that vast torrents, of depth sufficient to overtop our mountains, have swept along the surface of the earth, excavating vallies, undermining mountains, and carrying away whatever was unable to resist such powerful corrosion. If such agents have been at work in the Alps, it is difficult to conceive that our countries should have been spared. I made it therefore my business to search for traces of similar operations here. I was not long in discovering such in great abundance ; and, with the help of several of my friends, I have traced the indications of vast torrents in this neighbourhood, as obvious as those I formerly saw on Saleve and Jura. Since I announced my opinion on this subject, in a note subjoined to my paper on Whinstone and Lava, published in the fifth volume of the *Transactions* of this Society, I have met with many confirmations of these views. The most important of these are derived from the testimony of my friend Lord SELKIRK, who has lately met with a series of similar facts in North America.

It would be difficult to compute the effects of such an agent ; but if, by means of it, or of any other cause, the whole mass of secondary strata, in great tracts of country, has been removed from above the primary, the weight of that mass alone must have been sufficient to fulfil all the conditions of the Huttonian Theory, without having recourse to the pressure of the sea. But when the two pressures were combined, how great must have been their united strength !

WE are authorised to suppose, that the materials of our strata, in this situation, underwent the action of fire. For volcanoes have burnt long before the earliest times recorded in history, as appears by the magnitude of some volcanic mountains ; and it can scarcely be doubted, that their fire has acted without any material cessation ever since the surface of our globe acquired its
present

present form. In extending that same influence to periods of still higher antiquity, when our strata lay at the bottom of the sea, we do no more than ascribe permanence to the existing laws of nature.

THE combination of heat and compression resulting from these circumstances, carries us to the full extent of the Huttonian Theory, and enables us, upon its principles, to account for the igneous formation of all rocks from loose marine deposits.

THE sand would thus be changed to sandstone; the shells to limestone; and the animal and vegetable substances to coal.

OTHER beds, consisting of a mixture of various substances, would be still more affected by the same heat. Such as contained iron, carbonate of lime, and alkali, together with a mixture of various earths, would enter into thin fusion, and, penetrating through every crevice that occurred, would, in some cases, reach what was then the surface of the earth, and constitute lava: in other cases, it would congeal in the internal rents, and constitute porphyry, basalt, greenstone, or any other of that numerous class of substances, which we comprehend under the name of *volcanic stone*. At the same time, beds of similar quality, but of composition somewhat less fusible, would enter into a state of viscosity, such as many bodies pass through in their progress towards fusion. In this state, the particles, though far from possessing the same freedom as in a liquid, are susceptible of crystalline arrangement*; and the substance

* THIS state of viscosity, with its numberless modifications, is deserving of great attention, since it affords a solution of some of the most important geological questions. The mechanical power exerted by some substances, in the act of assuming a crystalline form, is well known. I have seen a set of large and broad crystals

substance, which, in this sluggish state, would be little disposed to move, being confined in its original situation by contiguous beds of more refractory matter, would crystallize, without undergoing any change of place, and constitute one of those beds of whinstone, which frequently occur interstratified with sandstone and limestone.

IN other cases where the heat was more intense, the beds of sand, approaching more nearly to a state of fusion, would acquire such tenacity and toughness, as to allow themselves to be bent and contorted, without laceration or fracture, by the influence of local motions, and might assume the shape and character of primary schistus: the limestone would be highly crystallized, and would become marble, or, entering into thin fusion, would penetrate the minutest rents in the form of calcareous spar. Lastly, when the heat was higher still, the sand itself would be entirely melted, and might be converted, by the subsequent effects of slow cooling, into granite, sienite, &c.; in some cases, retaining traces of its original stratification, and constituting gneiss and stratified granite; in others, flowing into the crevices, and forming veins of perfect granite.

IN consequence of the action of heat, upon so great a quantity of matter, thus brought into a fluid or semifluid state, and in which, notwithstanding the great pressure, some substances would be volatilized, a powerful heaving of the superincumbent mass must have taken place; which, by repeated efforts, succeeding

crystals of ice, like the blade of a knife, formed in a mass of clay, of such stiffness, that it had just been used to make cups for chemical purposes. In many of my former experiments, I found that a fragment of glass made from whinstone or lava, when placed in a muffle heated to the melting point of silver, assumed a crystalline arrangement, and underwent a complete change of character. During this change, it became soft, so as to yield to the touch of an iron rod; yet retained such stiffness, that, lying untouched in the muffle, it preserved its shape entirely; the sharp angles of its fracture not being in the least blunted.

ceeding each other from below, would at last elevate the strata into their present situation.

THE Huttonian Theory embraces so wide a field, and comprehends the laws of so many powerful agents, exerting their influence in circumstances and in combinations hitherto untried, that many of its branches must still remain in an unfinished state, and may long be exposed to partial and plausible objections, after we are satisfied with regard to its fundamental doctrines. In the mean time I trust, that the object of our pursuit has been accomplished, in a satisfactory manner, by the fusion of limestone under pressure. This single result affords, I conceive, a strong presumption in favour of the solution which Dr HUTTON has advanced of all the geological phenomena; for, the truth of the most doubtful principle which he has assumed, has thus been established by direct experiment.

APPEN-

APPENDIX.

No. I.

SPECIFIC GRAVITY OF SOME OF THE FOREGOING RESULTS.

AS many of the artificial limestones and marbles produced in these experiments, were possessed of great hardness and compactness, and as they had visibly undergone a great diminution of bulk, and felt heavy in the hand, it seemed to me an object of some consequence to ascertain their specific gravity, compared with each other, and with the original substances from which they were formed. As the original was commonly a mass of chalk in the lump, which, on being plunged into water, begins to absorb it rapidly, and continues to do so during a long time, so as to vary the weight at every instant, it was impossible, till the absorption was complete, to obtain any certain result; and to allow for the weight thus gained, required the application of

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a method different from that usually employed in estimating specific gravity.

IN the common method, the substance is first weighed in air, and then in water ; the difference indicating the weight of water displaced, and being considered as that of a quantity of water equal in bulk to the solid body. But as chalk, when saturated with water, is heavier, by about one-fourth, than when dry, it is evident, that its apparent weight, in water, must be increased, and the apparent loss of weight diminished exactly to that amount. To have a just estimate, then, of the quantity of water displaced by the solid body, the apparent loss of weight must be increased, by adding the absorption to it.

Two distinct methods of taking specific gravity thus present themselves, which it is of importance to keep separate, as each of them is applicable to a particular class of subjects.

ONE of these methods, consists in comparing a cubic inch of a substance in its dry state, allowing its pores to have their share in constituting its bulk, with a cubic inch of water.

THE other depends upon comparing a cubic inch of the solid matter of which the substance is composed, independently of vacuities, and supposing the whole reduced to perfect solidity, with a cubic inch of water.

THUS, were an architect to compute the efficacy of a given bulk of earth, intended to load an abutment, which earth was dry, and should always remain so, he would undoubtedly follow the first of these modes : Whereas, were a farmer to compare the specific gravity of the same earth with that of any other soil, in an agricultural point of view, he would use the second mode, which is involved in that laid down by Mr DAVY.

As our object is to compare the specific density of these results, and to ascertain to what amount the particles have approached

proached each other, it seems quite evident that the first mode is suited to our purpose. This will appear most distinctly, by inspection of the following Table, which has been constructed so as to include both.

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TABLE.

TABLE OF SPECIFIC GRAVITIES.

I.	II. Weight in air, dry.	III. Weight in water.	IV. Weight in air, wet.	V. Difference between Columns II. & III.	VI. Difference between Columns II. & IV. or absorption.	VII. Absorp- tion per cent.	VIII. Sum of Columns V. and VI.	IX. Specific gravity by com- mon mode.	X. Specific gravity by new mode.
1.	125.90	77.55	135.65	47.35	9.75	7.74	57.10	2.604	2.204
2.	9.94	6.13	9.99	3.81	0.05	0.50	3.86	2.609	2.575
3.	15.98	9.70	16.02	6.28	0.04	0.25	6.32	2.544	2.528
4.	5.47	3.33	5.48	2.14	0.01	0.18	2.15	2.556	2.544
5.	18.04	10.14	18.06	7.90	0.02	0.11	7.92	2.283	2.277
6.	6.48	3.74	7.10	2.74	0.62	9.56	3.36	2.365	1.928
7.	10.32	5.97	10.36	4.35	0.04	0.39	4.39	2.372	2.350
8.	54.57	31.30	55.23	23.27	0.66	1.21	23.93	2.345	2.280
9.	72.27	41.10	76.13	31.17	3.86	5.34	35.03	2.318	2.063
10.	37.75	21.15	38.30	16.60	0.55	1.45	17.15	2.274	2.201
II.	21.21	12.55	21.26	8.66	0.05	0.24	8.71	2.449	2.435
12. Marble.	18.59	11.56	18.61	7.03	0.02	0.18	7.05	2.644	2.636
13. Chalk.	504.15	302.40	623.20	201.75	119.05	23.61	320.80	2.498	1.571
14. Average Chalk.	444.30	264.35	550.80	179.95	106.50	23.97	286.45	2.469	1.551
15. rammed Powder.	283.97	—	—	—	—	—	198.65	—	1.429

EXPLANATION.

COLUMN I. contains the number affixed to each of the specimens, whose properties are expressed in the table.

THE

THE first eleven are the same with those used in the paper read in this Society on the 30th of August 1804, and published in NICHOLSON'S *Journal* for October following, and which refer to the same specimens. No. 12. Is a specimen of yellow marble, bearing a strong resemblance to No. 3. No. 13. A specimen of chalk. No. 14. Shews the average of three trials with chalk. No. 15. Some pounded chalk, rammed in the manner followed in these experiments. In order to ascertain its specific gravity, I rammed the powder into a glass-tube, previously weighed; then, after weighing the whole, I removed the chalk, and filled the same tube with water. I thus ascertained, in a direct manner, the weight of the substance, as stated in Column II., and that of an equal bulk of water, stated in Column VIII.

COLUMN II. Weight of the substance, dry in air, after exposure, during several hours to a heat of 212° of FAHRENHEIT.

COLUMN III. Its weight in water, after lying long in the liquid, so as to perform its full absorption; and all air-bubbles being carefully removed.

COLUMN IV. Weight in air, wet. The loose external moisture being removed by the touch of a dry cloth; but no time being allowed for evaporation.

COLUMN V. Difference between Columns II. and III., or apparent weight of water displaced.

COLUMN VI. Difference between Columns II. and IV., or the absorption

COLUMN VII. Absorption reduced to a *per centage* of the dry substance.

COLUMN VIII. Sum of Columns V. and VI., or the real weight of water displaced by the body.

COLUMN IX. Specific gravity, by the common mode, resulting from the division of Column II. by Column V.

COLUMN X. Specific gravity, in the new mode, resulting from the division of Column II. by Column VIII.

THE

THE specific gravities ascertained by the new mode, and expressed in Column X. correspond very well to the idea which is formed of their comparative densities, from other circumstances, their hardness, compact appearance, susceptibility of polish, and weight in the hand.

THE case is widely different, when we attend to the results of the common method contained in Column IX. Here the specific gravity of chalk is rated at 2.498, which exceeds considerably that of a majority of the results tried. Thus, it would appear, by this method, that chalk has become lighter by the experiment, in defiance of our senses, which evince an increase of density.

THIS singular result arises, I conceive, from this, that, in our specimens, the faculty of absorption has been much more decreased than the porosity. Thus, if a piece of crude chalk, whose specific gravity had previously been ascertained by the common mode, and then well dried in a heat of 212° , were dipped in varnish, which would penetrate a little way into its surface; and, the varnish having hardened, the chalk were weighed in water, it is evident, that the apparent loss of weight would now be greater by 23.61 *per cent.* of the dry weight, than it had been when the unvarnished chalk was weighed in water; because the varnish, closing the superficial pores, would quite prevent the absorption, while it added but little to the weight of the mass, and made no change on the bulk. In computing, then, the specific gravity, by means of this last result, the chalk would appear very much lighter than at first, though its density had, in fact, been increased by means of the varnish.

A SIMILAR effect seems to have been produced in some of these results, by the agglutination or partial fusion of part of the substance, by which some of the pores have been shut out from the water.

THIS

THIS view derives some confirmation from an inspection of Columns VI. and VII.; the first of which expresses the absorption; and the second, that result, reduced to a *per centage* of the original weight. It there appears, that whereas chalk absorbs 23.97 *per cent.*, some of our results absorb only 0.5, or so low as 0.11 *per cent.* So that the power of absorption has been reduced from about one-fourth, to less than the five hundredth of the weight.

I HAVE measured the diminution of bulk in many cases, particularly in that of No. 11. The chalk, when crude, ran to the 75th degree of WEDGWOOD's gage, and shrank so much during the experiment, that it ran to the 161st; the difference amounting to 86 degrees. Now, I find, that WEDGWOOD's gage tapers in breadth, from 0.5 at zero of the scale, to 0.3 at the 240th degree. Hence, we have for one degree 0.000833. Consequently, the width, at the 75th degree, amounts to 0.437525; and at the 161st, to 0.365887. These numbers, denoting the linear measure of the crude chalk, and of its result under heat and compression, are as 100 to 83.8; or, in solid bulk, as 100 to 57.5. Computing the densities from this source, they are as 1 to 1.73. The specific gravities in the Table, of the chalk, and of this result, are as 1.551 : 2.435; that is, as 1 to 1.57. These conclusions do not correspond very exactly; but the chalk employed in this experiment, was not one of those employed in determining average specific gravity in the Table; and other circumstances may have contributed to produce irregularity. Comparing this chalk with result second, we have 1.551 : 2.575 so 1 : 1.6602.

TABLE

No. II.

TABLE,

CONTAINING THE REDUCTION OF THE FORCES MENTIONED
IN CHAP. VII. TO A COMMON STANDARD.

I. Number of experiment referred to in Chap. VII.	II. Bore, in de- cimals of an inch.	III. Pressure in hundred weights.	IV. Tempera- ture by WEDG- WOOD'S pyrometer.	V. Depth of sea in feet.	VI Ditto in miles.	VII. Pressure, ex- pressed in at- mospheres
1	0.75	3	22	1708.05	0.3235	51.87
2	0.75	3	25	1708.05	0.3235	51.87
3	0.75	10	20	5693.52	1.0783	172.92
4	0.75	10	31	5693.52	1.0783	172.92
5	0.75	10	41	5693.52	1.0783	172.92
6	0.75	10	51	5693.52	1.0783	172.92
7	0.75	10	—	5693.52	1.0783	172.92
8	0.54	2	—	2196.57	0.4160	66.71
9	0.54	{ 4 8.1	—	4393.14	0.8320	133.43
			—	8896.12	1.6848	270.19
10	0.75	3	21	1708.05	0.3235	51.87
11	0.75	4	25	2277.41	0.4313	69.70
12	0.75	5	—	2846.76	0.5396	86.46

EXPLANATION.

EXPLANATION.

COLUMN I. contains the number of the experiment, as referred to in the text. Column II. The bore of the barrel used, in decimals of an inch. Column III. The absolute force applied to the barrel, in hundred-weights. Column IV. The temperature, in WEDGWOOD's scale. Column V. The depth of sea at which a force of compression would be exerted equal to that sustained by the carbonate in each experiment, expressed in feet. Column VI. The same in miles. Column VII. Compressing force, expressed in atmospheres.

BOTH Tables were computed separately, by a friend, Mr J. JARDINE, and myself.

THE following data were employed.

AREA of a circle of which the diameter is unity, 0.785398.

WEIGHT of a cubic foot of distilled water, according to Professor ROBISON, 998.74 ounces avoirdupois.

MEAN specific gravity of sea-water, according to BLADH, 1.0272.

MEAN height of the barometer at the level of the sea 29.91196 English inches, according to LAPLACE.

SPECIFIC gravity of mercury, according to CAVENDISH and BRISSON, 13.568.



TABLE
EXPLANATION

Column I contains the number of the experiment as referred to in the text. Column II the date of the trial. Column III the absolute force applied to the bar, in pounds weight. Column IV the temperature in Fahrenheit. Column V the length of the bar. A force of compression was exerted equal to that of the bar by the carbonate in each experiment, expressed in feet. Column VI the time in minutes. Column VII Compressing force, expressed in atmospheres. Column VIII Barometer, and weight.

The following data were employed.
The diameter of the bar is .72358.
The weight of the bar of distilled water, according to the
Sutton's table, .087 ounce avoirdupois.
The specific gravity of the water, according to Sutton,
is 1.000.
The length of the barometer at the time of the trial,
29.95 English inches, according to Barak.
The specific gravity of mercury, according to Cavendish and
Barak, 13.596.





Fig. 2.



Fig. 3.

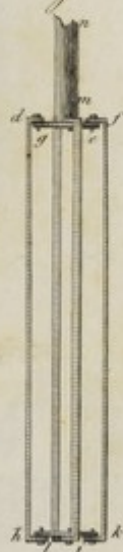


Fig. 4.

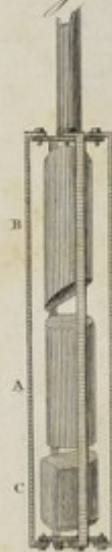


Fig. 5.

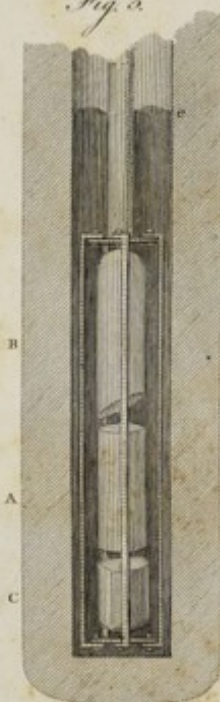


Fig. 6.

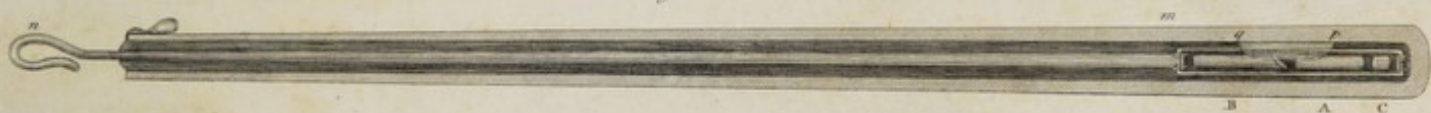


Fig. 7.



Fig. 8.



Fig. 9.



Fig. 10.

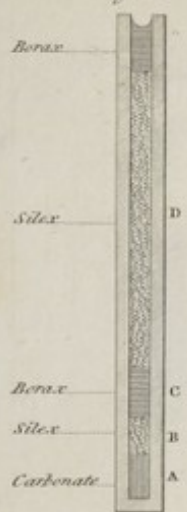


Fig. 11.



Fig. 12.



Fig. 13.



Fig. 14.

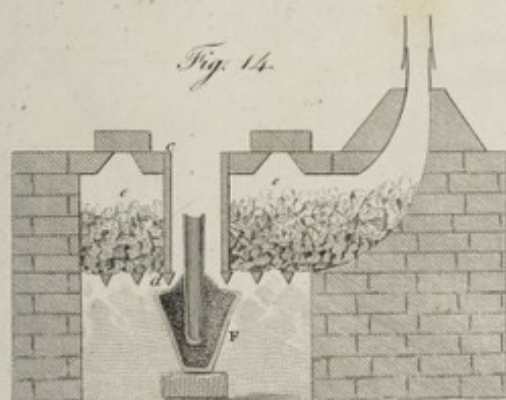


Fig. 15.

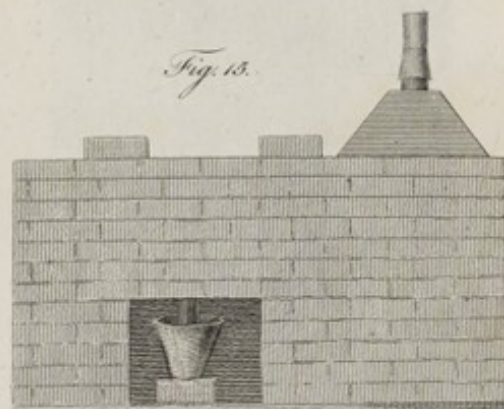


Fig. 16.

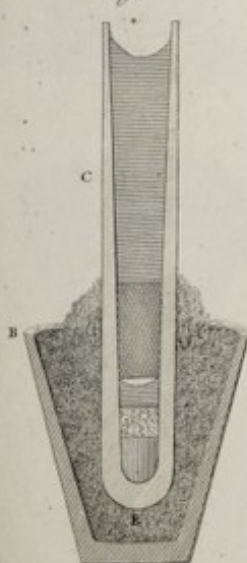


Fig. 17.

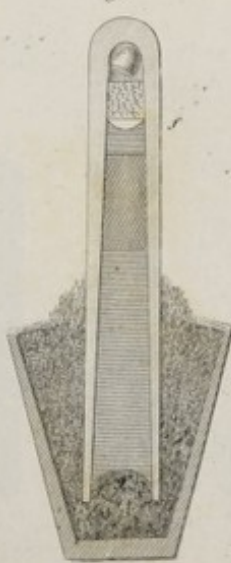


Fig. 18.

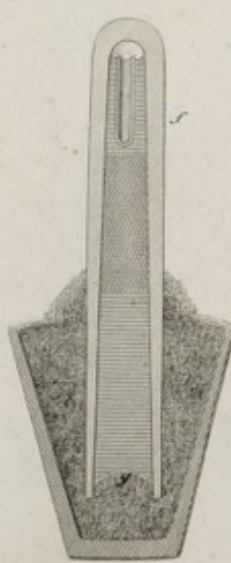
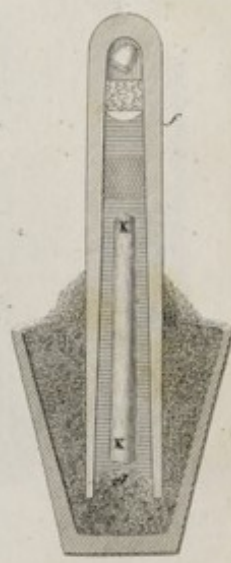
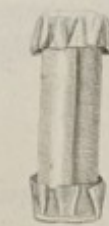
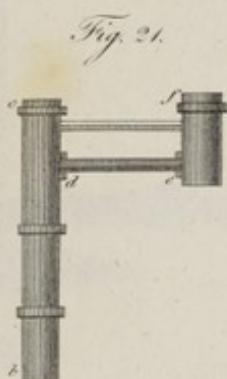
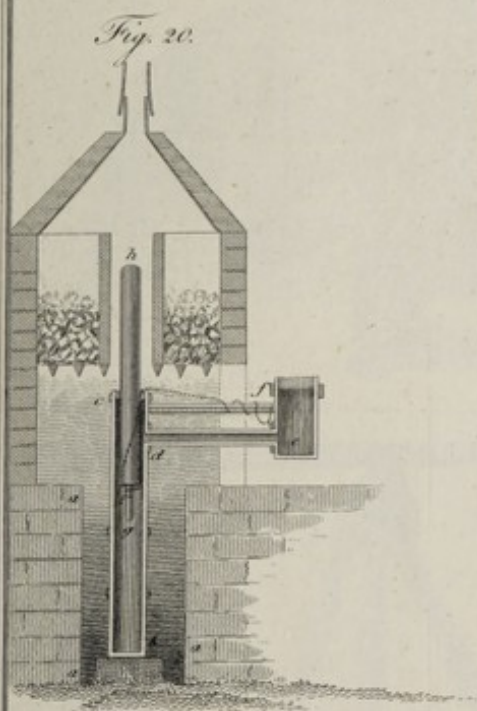
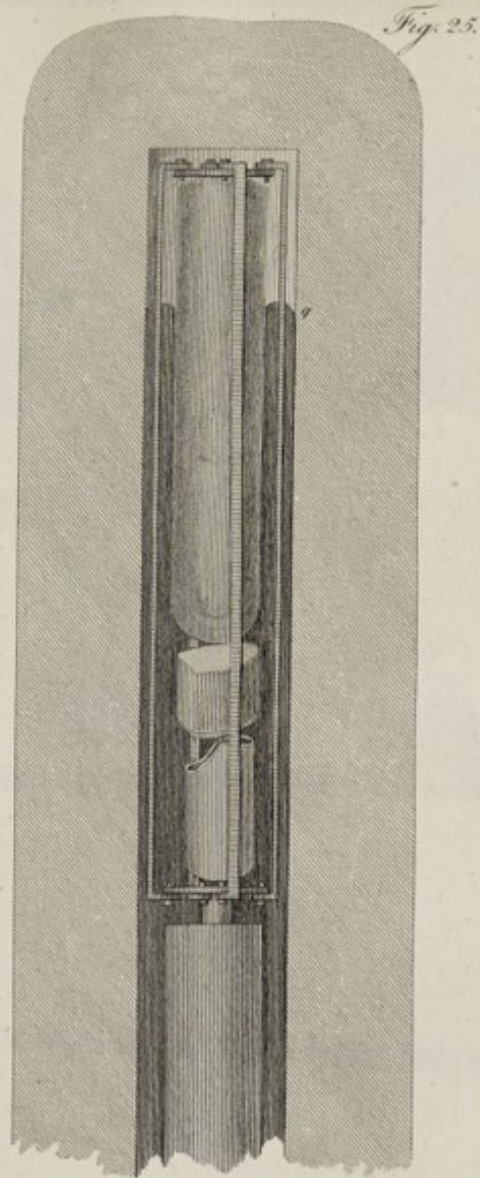
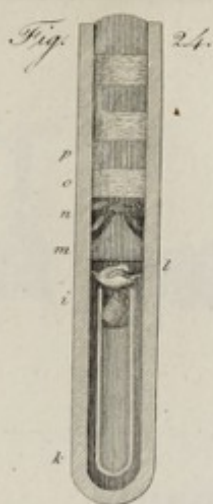
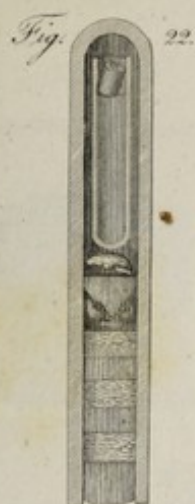


Fig. 19.





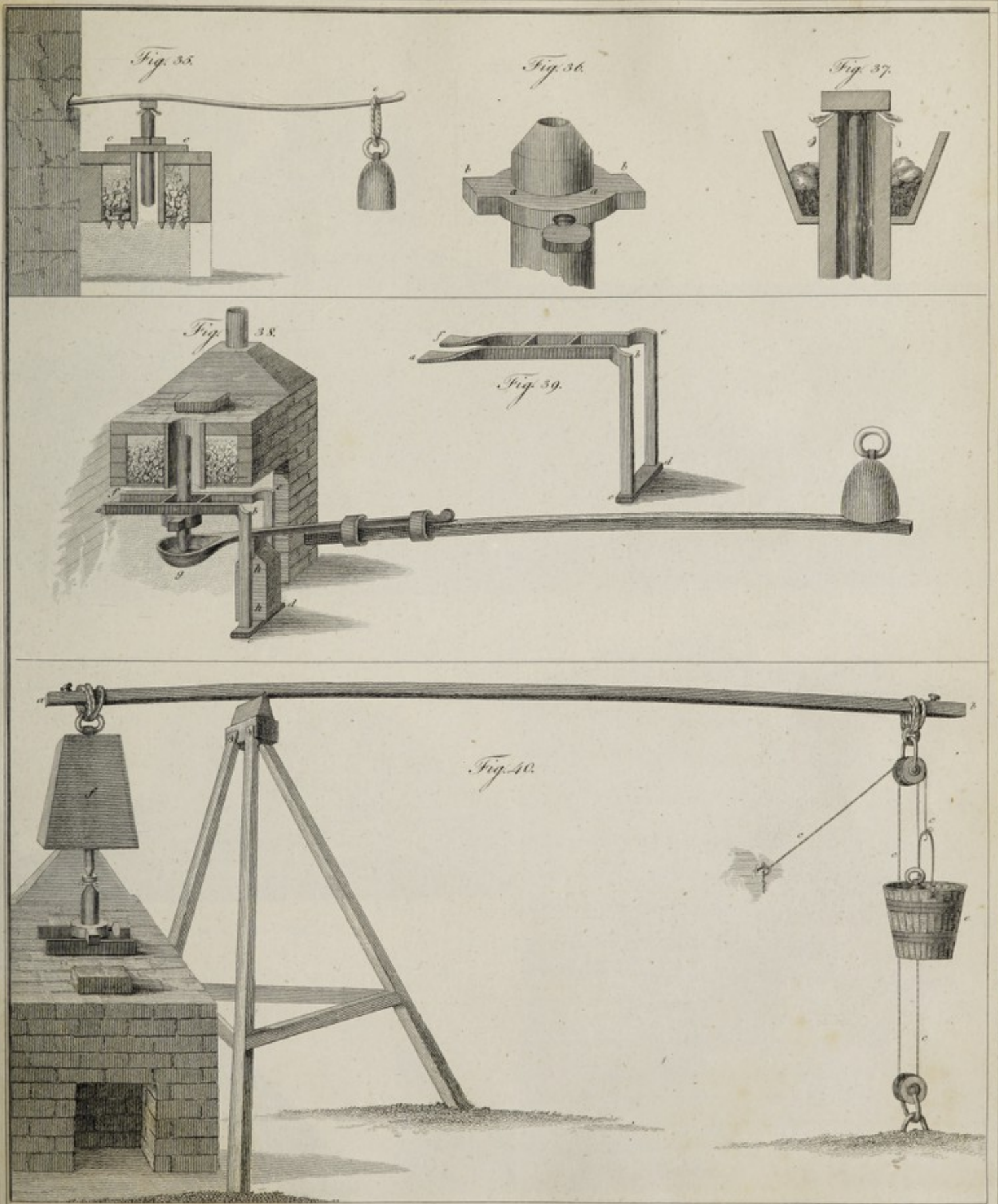


Fig. 41.



Fig. 42.



Fig. 43.

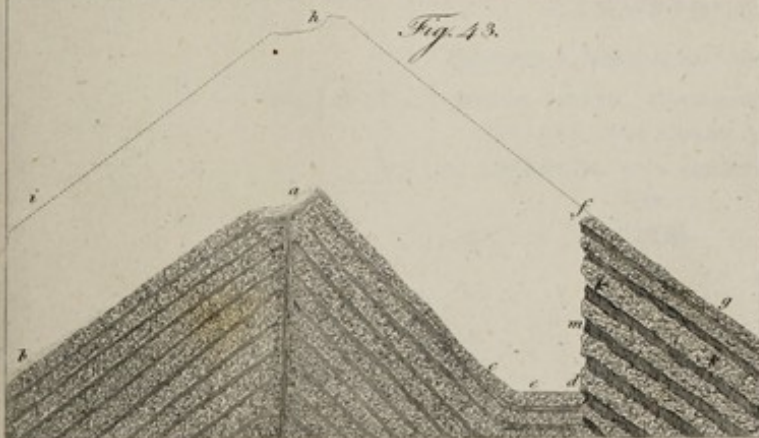


Fig. 44.



