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TRANSACTIONS

OF THE
ROYAL SOCIETY OF EDINBURGH.

VOL. VII.



EDINBURGH:

PRINTED FOR ARCHIBALD CONSTABLE & CO. EDINBURGH,
AND CADELL AND DAVIES, LONDON.

1815.

THE BRITISH MUSEUM



CONTENTS

OF

VOLUME SEVENTH.

(PART FIRST.)

- I. *Some Account of a Boy born Blind and Deaf, collected from authentic Sources of Information; with a few Remarks and Comments.* By Dugald Stewart, Esq; F. R. S. Edin. - - - Page 5
- II. *On the Vertical Position and Convolutions of certain Strata, and their relation with Granite.* By Sir James Hall, Baronet, Pr. R. S. Edin. & F. R. S. Lond. - - - 79
- III. *Remarks on the Transition Rocks of Werner.* By Thomas Allan, Esq; F. R. S. Edin. - - - 109
- IV. *On the Revolutions of the Earth's Surface.* Part I. By Sir James Hall, Bart. - - - 139
- V. *On the Revolutions of the Earth's Surface.* Part II. By Sir James Hall, Bart. - - - 169
- VI. *An Account of some Geological Facts observed in the Faroe Islands.* By Sir George Mackenzie, Baronet, Pr. Ph. Cl. R. S. Edin. - - - 213
- VII. *An Account of the Mineralogy of the Faroe Islands.* By Thomas Allan, Esq; F. R. S. Edin. - - - 229

PART

(PART SECOND.)

- VIII. *Account of the Structure of the Table Mountain, and other Parts of the Peninsula of the Cape. Drawn up by Professor Playfair, from Observations made by Captain Basil Hall, R. N. F. R. S. Edin.* - Page 269
- IX. *Comparison of the North Polar Distances of Thirty-eight Principal Fixed Stars, on the 1st of January 1800, as determined by Observations made at Greenwich, Armagh, Palermo, Westbury, Dublin, and Blackheath. By S. Groombridge, Esq. Blackheath, F. R. S. Lond. Communicated by Dr Brewster,* - - - 279
- X. *On the Optical Properties of Sulphate of Carbon, Carbonate of Barytes, and Nitrate of Potash, with Inferences respecting the Structure of Doubly Refracting Crystals. By David Brewster, LL. D. F. R. S. Edin. & F. A. S. E.* - 285
- XI. *An Account of Observations made by Lord Webb Seymour and Professor Playfair, upon some Geological Appearances in Glen Tilt, and the adjacent Country. Drawn up by Lord Webb Seymour,* - - - 303
- XII. *On certain Appearances observed in the Dissection of the Eyes of Fishes. By James L. Drummond, M. D. of Belfast. Communicated by Dr Thomas Brown, Professor of Moral Philosophy in the University of Edinburgh,* - - - 377
- XIII. *Observations on the Theory of Language. By Henry Dewar, M. D. Fellow of the Royal College of Physicians of Edinburgh,* - - - 387
- XIV. *On the Diffusion of Heat at the Surface of the Earth. By John Murray, M. D. F. R. S. Edin.* 411
- XV. *On*

CONTENTS.

vii

- XV. *On a New Species of Coloured Fringes, produced by the Reflection of Light between Two Plates of parallel Glass of equal thickness.* By David Brewster, LL. D. F. R. S. Edin. & F. A. S. E. 435
- XVI. *An Analysis of the Mineral Waters of Cromlix, near Dunblane, and of Pitcaithly; with general Observations on the Analysis of Mineral Waters, and the Composition of Bath Water and some others.* By John Murray, M. D. F. R. S. Edin. 445
- XVII. *Biographical Account of the late John Robison, LL. D. F. R. S. Edin. and Professor of Natural Philosophy in the University of Edinburgh.* By John Playfair, F. R. S. L. & E. &c. 495

APPENDIX, containing Lists of the Office-bearers and Members elected since November 1812, 541

XV. On a New Species of Colonial Fungus, produced
 by the Impaction of Light between Two Plates of
 parallel Glass of equal thickness. By David
 Brewster, LL.D. F.R.S. Edin. & F.A.S. S.
 XVI. An Account of the Mineral Waters of Cressle
 near Dunblane, and of Peninsular and general
 Observations on the Springs of Mineral Waters,
 and the Composition of the Water and some
 other Matters. By John Hope, Esq.

DIRECTIONS TO THE BINDER.

- Plate I. II. III. IV. V. to front page 108.
- VI. VII. VIII. IX. to front page 212.
- X. XI. (marked on the engraving *Geology of Faroe*, Plate I. & II.)
to front page 220.
- XII. Map of Faroe, to front page 229.
- XIII. Sketch of Peninsula of the Cape, to front page 270.
- XIV. } Granite Veins, to front page 278.
- XV. }
- XVI. }
- XVII. }
- XVIII. } after page 368.
- XIX. }
- XX. }
- XXI. }
- XXII. after page 444.

SOME ACCOUNT
OF A
BOY BORN BLIND AND DEAF,
COLLECTED FROM AUTHENTIC SOURCES OF INFORMATION ;
WITH A
FEW REMARKS AND COMMENTS.

THE Memoir which I am about to submit to the consideration of the Royal Society, relates to the melancholy history of a boy who was born blind and deaf; and who, of consequence, has derived all his knowledge of things external from the senses of Touch, of Taste, and of Smell*.

It is now considerably more than a year since I first heard of this case from my very ingenious friend Mr WARDROP, Surgeon in London; a gentleman whose scientific attainments and professional skill it is unnecessary for me to mention to this audience.

* Since this paper was read before the Society, I have been enabled, by subsequent communications, to enlarge it considerably. I have still reason to expect farther information on the subject; but various circumstances make it desirable, that so curious an article of philosophical intelligence should not be any longer withheld from the public.

nece. The information which he *then* communicated to me was extremely general; but more than sufficient to excite all my curiosity. "I have at present (says he) a patient under my care, whose case is, I believe, *unique*. It is a boy fourteen years old, who was born *blind* and *deaf*, and of course *dumb*. His senses of touch and smell have a wonderful degree of acuteness; for by these alone he has acquired a very accurate knowledge of external things, and is able to know readily his old acquaintances from strangers. The powers of his mind are vigorous. He is evidently capable of reflection and reasoning, and is warmly attached to his parents. He has a most delicate palate, and partakes only of the most simple food. I have couched one of his eyes successfully; and he is much amused with the visible world, though he mistrusts information gained by that avenue. One day I got him a new and *gaudy* suit of clothes, which delighted him beyond description. It was the most interesting scene of *sensual* gratification I ever beheld *."

The first idea which struck me on receiving this intelligence was, that so extraordinary a combination of circumstances might perhaps afford a favourable opportunity of verifying or of correcting, in an unequivocal manner, some of those details in CHESELDEN's celebrated narrative, about which considerable doubts have been lately entertained, in consequence of their disagreement with the results of MR WARE's experience †.

A

* This letter was dated October 4. 1810.

† Mr WARE's paper here alluded to, is to be found in the *Philosophical Transactions* for 1801. The argument which it has been supposed to afford against CHESELDEN (founded on the case of Master W.) has always appeared to me to prove nothing, in consequence of its aiming to prove too much. Of this patient, (a boy who was restored to sight at seven years of age, after he had been blind from very early infancy), we are told, that two days after the
operation,

A repetition of such observations and experiments as CHESELDEN made, would, I imagined, be greatly facilitated by the total *deafness* of the patient in question; the judgments which a blind man is enabled to form of distances (at least of *small* distances) by the ear, approaching, in

operation, the handkerchief which was tied over his eyes having slipped upward, he distinguished the table, by the side of which his mother was sitting. "It was about a yard and a half from him; and he observed, *that it was covered with a green cloth*, (which was really the case), and that it was a little farther off than he was able to reach."

Mr WARE afterwards informs us, that "he held a letter before his patient, at the distance of about twelve inches, when he told him, after a short hesitation, that *it was a piece of paper*; that it was square, which he knew by its corners, and that it was longer in one direction than it was in the other."—"I then (says he) shewed him a small oblong band-box, covered with red leather; which he said was *red*, and square, and pointed at once to its four corners. The observation, however, which appeared to me most remarkable; was, that which related to a white stone-mug; which he first called a *white* bason, but, soon after, recollecting himself, said it was a mug, because it had a handle."

Of the correctness and fidelity of this statement, I have not the slightest doubt. But the only inference which can, with certainty, be deduced from it is, that the patient saw too well *before* the operation, to make his perceptions *afterwards* of any value for deciding the point in question. If he was able to recognise a *green cloth*, and a *piece of white paper*, the very moment that the bandage was removed, the degree of sight which he possessed previous to Mr WARE's acquaintance with him, *must* have been such as to furnish him with a variety of *sensations*, quite sufficient to serve as materials for an imperfect *visual language*;—a language, if not accurately significant of comparative distances from the eye, at least fully adequate to convey, through the channel of that organ, the intimation of *distance in general*, or of what BERKELEY calls *outness*;—perhaps, also, some indistinct perception of diversities of *visible figure*. The slightest, and, to us, the most evanescent shades of difference in these sensations, will, we may be assured, become in the case of such an individual, *signs* of all the various changes in the state of surrounding objects, with which they have any connection.

Having mentioned, on this occasion, the name of Mr WARE, I think it but justice to him to add, that he does not appear to me to be himself disposed to

push

point of accuracy, very nearly to those which we are accustomed to form by means of the eye. I had once occasion to witness the precision with which Mr GOUGH of Kendal (by far the most intelligent and ingenious person, born blind, whom I have happened to meet with) guessed at the dimensions of a large room, a few minutes after he entered it. The error he committed was a mere trifle; not exceeding what
 might

push his argument against CHESELDEN so far as has been apprehended by some later writers. In the following passages, he not only seems to admit the truth of that optical principle which he has been generally understood to controvert, but even points at the same explanation which I have already suggested, of the apparent inconsistency between his own experience and that of his predecessor.

“ I beg leave (says he) to add further, that on making inquiries of two children, between seven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is sufficient to enable them to tell whether coloured objects be brought nearer to, or carried farther from them; for instance, whether they are at the distance of two inches or four inches from their eyes.

.....

“ I am aware, that these observations not only differ from those that are related of Mr CHESELDEN's patient, but appear, on the first statement, to oppose a principle in optics, which I believe is commonly and justly admitted, that the senses of sight and feeling have no other connection but that which is formed by experience; and therefore, that the ideas derived from feeling, can have no power to direct the judgment, with respect either to the distance or form of visible objects. It should be recollected, however, that persons who have cataracts in their eyes, are not, in strictness of speech, blind, though they are deprived of all useful sight. The instances I have adduced prove, that the knowledge they have of colours is sufficient to give them some idea of distance, even in their darkest state. When, therefore, their sight is cleared by the removal of the opaque crystalline which intercepted the light, and the colour of objects is thereby made to appear stronger, will it be difficult or unphilosophical to conceive, that their ideas of distance will be strengthened, and so far extended, as to give them a knowledge even of the outline and figure of those objects with the colour of which they were previously acquainted?”

might have been expected from the practised eye of a joiner or of an architect. It is not every operator, however dextrous in his own art, who can be expected to attend sufficiently to these collateral circumstances, or to be fully aware of the difficulty which a blind person, suddenly put in possession of a new sense, must experience, when he attempts to distinguish, in his estimates of distances, the perceptions of the eye from those of the ear or of the nostrils. Something of the same kind, indeed, or at least strikingly analogous to it, happens every moment to ourselves, in the judgments we pronounce on the beauty or deformity of visible objects, without any suspicion, on our part, how much these judgments are influenced by co-existent impressions of odour or of sound.

In consequence of this view of the subject, I had been led by the first general outline which I received of this occurrence, to indulge a hope that the peculiarities of the case might offer some facilities which had not been before experienced, for establishing by palpable and incontestible proofs, the distinction between the original and the acquired perceptions of sight; while, at the same time, the inability of the patient to answer, by speech, the queries which might be proposed to him with respect to the new world to which he had been so recently introduced, would, I conceived, by drawing the attention of those around him to other signs of a less ambiguous nature, place the results of their observations beyond the reach of controversy.—Not that, even upon *this* supposition, every difficulty would have been removed; inasmuch as intimations concerning *distance* may be occasionally conveyed to a blind man, not only by the sense of smell, but by some of those *feelings* which are commonly referred to the sense of Touch*. In observing,

* The blind man of *Puisseaux* (mentioned by DIDEROT) judged of his distance from the fire-place by the degree of heat; and of his approach to any solid

erving, accordingly, the first visual perceptions even of a patient born deaf as well as blind, some very nice attentions would be necessary for ascertaining the truth. But what proportion do these bear to the numerous and refined precautions which become indispensable, where the patient is reminded by every query which is addressed to his ear, of the distance and relative position of the questioner? Justly might DIDEROT say,—“ *Preparer et interroger un aveugle né, n'eût point été une occupation indigne des talens réunis de NEWTON, DESCARTES, LOCKE, et LEIBNITZ.*”—I mention this, because, from the great degree of perfection to which this branch of surgery has been lately carried, the increasing number of such cases may be expected to multiply daily the opportunities of philosophical experiment; and it is of importance, that those who may have the good fortune to enjoy them, should be fully apprized of the delicacy and the complexity of the phenomena which they have to observe and to record*.

In giving way to these speculations, I had proceeded on the supposition, that the blindness of the patient was complete; not sufficiently attending to (what was long ago remarked by CHESELDEN) the qualified sense in which the word *blindness* is understood by surgical operators. “Though this gentleman was *blind*,” (says CHESELDEN, speaking of the patient whose case he has so well described), “*as is said of all persons who have ripe cataracts, yet they are never so blind, from that* cause,

lid obstacle, by the action or pulse of the air upon his face. The same thing is recorded of Dr SANDERSON by his successor Mr COLSON.

* For the assistance of those to whom such a subject of observation may occur, some judicious hints are suggested in the *Lettre sur les Aveugles à l'usage de ceux qui voient.*

cause, but that they can discern day from night ; and, for the most part, in a strong light, distinguish black, white, and scarlet ; but they cannot perceive the shape of any thing. Thus it was with this young gentleman." The case I have since found to have been the same, and in a degree considerably greater, with the boy who has given occasion to this memoir ; inso-much that his condition seems to have approached much nearer to that of Mr WARE's patient than to that of CHESELDEN's. " At the time of life" (Mr WARDROP observes) when this boy began to walk, he seemed to be attracted by bright and dazzling colours ; and though every thing connected with his history appears to prove that he derived little *information* from the organ of sight, yet he received from it much *sensual gratification*.

" He used to hold between his eye and luminous objects, such bodies as he had found to increase, by their interposition, the quantity of light ; and it was one of his chief amusements, to concentrate the sun's rays by means of pieces of glass, transparent pebbles, or similar substances, which he held between his eye and the light, and turned about in various directions. These, too, he would often break with his teeth, and give them that form which seemed to please him most. There were other modes by which he was in the habit of gratifying this fondness for light. He would retire to any out-house, or to any room within his reach, shut the windows and doors, and remain there for some considerable time, with his eyes fixed on some small hole or chink which admitted the sun's rays, eagerly catching them. He would also, during the winter-nights, often retire to a dark corner of the room, and kindle a light for his amusement. On these occasions, as well as in the gratification of his other senses, his countenance and gestures displayed a most interesting avidity and curiosity.

" It

“ It was difficult, if not impossible, to ascertain with precision, the degree of sight which he enjoyed ; but from the preternatural acuteness which his senses of touch and smell had acquired, in consequence of their being habitually employed to collect that information for which the sight is peculiarly adapted, it may be presumed with confidence, that he derived little, if any assistance from his eyes, as organs of vision. The appearances of disease, besides, in the eyes, were such as to render it in the highest degree probable, that they enabled him merely to distinguish colours, and differences in the intensity of light.”

From this history of the patient's previous situation, it appeared evident that his case was not of such a sort as to afford an opportunity of bringing CHESELDEN's conclusions to the test. On the contrary, his habits of observation, and even of *experiment*, on his visual sensations, combined with the singular acuteness and discrimination of his olfactory perceptions, rendered it almost certain that the results of a successful operation on his eyes would be similar to those described in Mr WARE's paper. Such, accordingly, has, in point of fact, been the issue of this new experiment ;—in describing which, however, I must remark, to the honour of Mr WARDROP, as a cautious and philosophical observer, he has abstained from drawing the slightest inference to the prejudice of CHESELDEN's statement ;—a statement which nothing can disprove till a case shall occur of a patient cured of *total*, or *almost total* blindness ; and till this case shall be observed and examined with all the nice precautions which so delicate and complicated a phenomenon demands.

I shall not follow Mr WARDROP through the details of the surgical operation ; in performing which, he was forced, by the peculiar circumstances of his patient, to employ a mechanical apparatus, for fixing his body and head in an immoveable posture. I flatter myself that he will soon communicate to the public a history of the whole case ; and I should be sorry to deprive his memoir of any part of its interest. The general results alone are connected with the objects which I have at present in view ; and *these* I shall take the liberty to state in Mr WARDROP'S words.

“ When the operation was finished, he expressed great satisfaction ; gazed around him, and appeared to distinguish objects. This, however, could not be ascertained in a manner quite satisfactory, as it would have been prejudicial to his recovery to make any experiments ; but it could be perceived from the change in the expression of his countenance. His eye, accordingly, being covered up, he was carried home, and put to bed in a dark room ; after which he was bled in the arm.


“ On the fourth day I examined the eye accurately, and observed the state of his vision. I found that the crystalline lens (which had been pushed upwards and backwards) had altered its situation since the operation, and could be again distinguished, covering about one-fourth of the upper edge of the pupil. The other part of the pupil was quite transparent, and all the blood which was effused into the anterior chamber during the operation was now absorbed. On making trial if he could distinguish any object, he readily discovered a book, or any similar thing, placed on the coverlet of the bed ; and in many of his attempts, he seemed to judge pretty accurately of their distance.

VOL. VII. B “ On

“ On the fifth day he got out of bed, and was brought into a room having an equal and moderate light. He walked about the room readily ; and the expression of his countenance was much altered, having acquired that look which indicated the enjoyment of vision. Indeed, he always walked about, *before* the operation, with much freedom ; and even on a very rugged and unequal road, he did not appear to suffer in the least from any jolting.

“ He appeared well acquainted with the furniture of the room, having lived in it several days previous to the operation ; but though he evidently distinguished, and attempted to touch objects which were placed before him, judging pretty accurately of their distances, yet he seemed to trust little to the information given by his eye, and always turned away his head, while he felt accurately over the whole surfaces of the bodies presented to him.

“ On the sixth day he appeared stronger, and amused himself a good deal with looking out of the window, seeming to observe the carts and carriages which were passing in the street. On putting a shilling on the middle of a table, he instantly laid his hand upon it.

“ On the seventh day the inflammation was nearly gone, and he observed a piece of white paper of this size  lying

on the table. I took him into the street, and he appeared much interested in the busy scene around him ; and at times seemed frightened. A post supporting a scaffold, at the distance of two or three yards from him, chiefly attracted his notice, and he timorously approached it, groping, and stretching out his hand cautiously until he touched it.”

Of these very valuable facts Mr WARDROP has left us to form our own judgment. To myself, I must own, that, due allow-

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ances being made, 1st, for the visual *sensations* which were familiar to the patient from his infancy, and, 2dly, for the intimate and accurate acquaintance which he had acquired of things external, by a comparison of the perceptions of *smell* and of *touch*, the result appears, on the whole, as favourable as could reasonably have been expected, to the Berkeleian theory of vision : Nor am I able to observe a single circumstance of any importance, which is not perfectly reconcilable with the *general tenor* of CHESELDEN'S narrative*.

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* I have said, the “*general tenor* of CHESELDEN'S narrative,”—for there are some *expressions* ascribed by him to his patient, which must, in my opinion, be understood with a considerable degree of latitude. And, indeed, if we reflect for a moment on the astonishment and agitation likely to be produced by the sudden acquisition of a new sense, we cannot fail to be satisfied, that the authority of the narrative rests much more on the conviction which the *whole* circumstances of the case had left on CHESELDEN'S own mind, than on the *verbal* answers (intelligent and satisfactory as most of these are), which his patient gave to the queries of his attendants. It was for *this* reason, among others, that I before hinted at the advantages which he would have enjoyed, in observing and describing the *facts* before him, if his patient had been *deaf* as well as *blind*, like the subject of this memoir.

Of one expression employed by CHESELDEN'S young man, I think it proper to take some notice here, on account of the stress which Mr WARE seems disposed to lay upon it, as at variance with the language used by his patient Master W. “When the young gentleman first saw, (says CHESELDEN), he was so far from making any judgment about distances, that he thought all objects whatever touched his eyes, (*as he expressed it*), as what he felt did his skin.” It seems to me inconceivable, that CHESELDEN could have meant this last phrase to be interpreted literally; for the thing which it implies is altogether impossible. The most obvious meaning which the words convey is, that the object seemed to be *contiguous to*, or *in contact with*, the *cornea*; whereas the truth is, that the office of the *cornea* is merely to transmit the rays to the *retina*, which it does without itself receiving any sensible impression of which we are conscious. Mr SMITH, too, has objected to this mode of speaking, though on grounds somewhat different. “When the young gentleman said, (I quote Mr SMITH'S words), that the objects
which

The strong impression which Mr WARE's paper has lately made on the public mind, and the support which it is probable many readers will imagine that the argument against CHESELDEN derives from the observations of Mr WARDROP, will account sufficiently for the length to which the foregoing remarks have extended : Or, if any farther apology be necessary, I trust that

which he saw touched his eyes, he certainly could not mean that they pressed upon or resisted his eyes ; for the objects of sight never act upon the organ in any way that resembles pressure or resistance. He could mean no more than that they were close upon his eyes, *or, to speak more properly, perhaps, that they were in his eyes* †." Mr SMITH's idea in this last clause, was, I presume, that the local situation of the object was referred by the patient to the *retina*, where the image of the object is painted. Now, I confess, for my own part, that although I perfectly agree with Mr SMITH in his criticism on CHESELDEN, I am by no means satisfied, that the emendation which he has suggested of the young gentleman's description is unexceptionable ; for it does not appear to me, that the impression of a moderate light on the *retina*, is accompanied with any perception of the part of the body on which the impression is made. Where the light, indeed, is so powerful as to produce *pain*, the case comes to be different ; for a sensation of *touch* is then united with the proper sensations of *sight* ; and it is characteristic of all sensations of *touch*, that they are accompanied with a perception of the *local situation* of their exciting causes. This, however, it is well known, does not take place with respect to the sensations of smell and of sound ; nor do I imagine it to take place, prior to experience, with respect to the sensations received by the eye. And, therefore, if a patient, in such circumstances, should be led, by his first visual perceptions, to connect them *locally* with the organ by which they are received, I should be inclined rather to ascribe this to concomitant feelings of *pain*, (produced by the recent operation, or by the too sudden impression of a strong light), than to any of those sensations which are exclusively appropriated to the sense of sight. But this discussion it is unnecessary for me to prosecute at present, as the opinion we may happen to form with respect to it, (whatever that opinion may be), can never affect the truth of that clause in CHESELDEN's statement, in which he asserts, *upon the evidence of his own observations*, that " when his patient first saw, he was unable to form any judgment about distances." The remainder of the sentence is only a loose and unintelligible comment of the young man on this simple fact.

† See an *Essay on the External Senses*, by ADAM SMITH, LL. D. (published among his posthumous papers.)

that allowances will be made for my anxiety to obtain from the enlightened Operators of the present times, an additional contribution of evidence in confirmation of one of the most beautiful, and, at the same time, one of the most important theories of modern philosophy.

Mr WARDROP afterwards enters into some circumstantial and very pleasing details with respect to an incident alluded to in a passage which I have already quoted from one of his letters ;—the joy manifested by his patient when he was first dressed in a suit of *gaudy* clothes. From this part of his memoir I shall only transcribe a few lines. “ His partiality to colours seemed to depend entirely on their comparative brilliancy. He in general liked objects that were white ; and still more particularly those of a red colour. A white waistcoat or white stockings pleased him exceedingly ; and he gave always a decided preference to yellow gloves. One day I observed him to take out of his pocket a bit of red sealing-wax, which he had kept for the beauty of its colour.

A pair of green-glasses were given him, with a view of lessening the influence of the bright sun on the still irritable eye ; and from them also he derived great pleasure. Indeed, when he first put them on, he laughed aloud with delight.”

A few weeks after I had been favoured by Mr WARDROP with his first communication on this subject, I learned, through a different channel, that his patient had left London ; and, as I had never happened to make any inquiries about his connections, or the place of his nativity, I had abandoned for many months all expectations of farther intelligence with respect to him ; when he was most unexpectedly and agreeably recalled to my recollection by a letter which I received last week from Mr Professor GLENNIE, the very learned and worthy
 successor

successor of Dr BEATTIE in his academical chair at Aberdeen. In this letter Mr GLENNIE incloses "An Account of JAMES MITCHELL, a lad in the county of Moray, born blind and deaf;" drawn up, at Mr GLENNIE'S request, by a neighbouring clergyman. From the narrative it appears, that this is the very patient who was formerly under Mr WARDROP'S care; and it appears farther, that although his blindness returned again, not long after the operation was performed, the peculiarities of his case still continue to present, *under a new and very different form*, a subject of examination and inquiry, not less interesting than if Mr WARDROP'S exertions in his favour had been rewarded with permanent success.

A short extract from Mr GLENNIE'S letter will form the best introduction I can prefix to the history which is to follow.

"I send you inclosed an account of a clergyman's son who was born deaf and blind. The account is imperfect as yet; but it is an accurate answer to a series of questions which I put to the clergyman who has taken the trouble to draw it up. As he has very obligingly offered to answer any more queries that I make, I have prepared a good many additional questions, that the present state of the young man's mind may be ascertained with as much accuracy as possible. Much light might have been thrown on the mental faculties, if accurate experiments and observations had been made on patients in such circumstances as this unfortunate young man. I intend, if it be possible, to visit him during our summer vacation; but I am sensible, that little can be done in such a case, even in a visit of some days, compared with what may be accomplished by his constant attendants, if we could teach them to make the proper experiments. For this purpose, the only thing I can think of is, to direct the mother and sister to have recourse to the narratives of some instances not
dissimilar,

dissimilar, that their attention may be drawn to their own methods of communication, which, having become habitual, escape their notice. But I must forbear entering on a minute discussion of this case, which appears to me very interesting."

Before I proceed to read the paper alluded to in the foregoing extract, I think it proper for me to mention, that I have not been favoured with the name of the writer, and that I must therefore request, it may not be considered, in its present form, as a fair subject of discussion or of criticism. That it bears strong marks of uncommon intelligence and discrimination in the observer, must be universally acknowledged; but it reached me so very lately, that I have not had time to solicit, through Mr GLENNIE, the author's permission to communicate it to the Society*.

* I have since learned from Mr GLENNIE, that the paper in question was written by the Reverend THOMAS MACFARLANE, minister of Edinkillie, in the presbytery of Forres. Mr GLENNIE adds a sentence which I beg leave to quote, as some apology for the liberty I now take in mentioning Mr MACFARLANE'S name without his express authority. I certainly would not have presumed to do so, if I had not been fully persuaded, that all who are competent to form a judgment on such subjects, will feel much indebted to him for his very interesting and satisfactory statement.

"As I communicated to Mr MACFARLANE your wish to print his memoir, I take for granted that he has no objection to your making this use of his papers, although he has not expressed his sentiments explicitly to this purpose."

Answers

Answers to some Queries addressed to a Clergyman in the County of Moray, by Mr Professor GLENNIE of Marischal College, Aberdeen, with respect to JAMES MITCHELL, a lad sixteen years of age, who was born blind and deaf.

“ The subject of this brief notice is the son of the Reverend DONALD MITCHELL, late minister of Ardlach, a Highland parish, lying on the banks of the Findhorn. He was born 11th November 1795, and is the sixth child of his parents, being the youngest except one. All his brothers and sisters, (as were also his parents), are perfectly free from the deficiency of sight and hearing, which occurs in his case; and are healthy and well formed. His mother, who is an intelligent and sensible lady, very early discovered his unfortunate situation: she noticed that he was *blind*, from his discovering no desire to turn his eyes to the light, or to any bright object; and afterwards, (in his early infancy also), she ascertained his being *deaf*, from the circumstance that no noise, however loud, awakened him from sleep. As he grew up, he discovered a most extraordinary acuteness of the senses of touch and smell; being very soon able, by these, to distinguish strangers from the members of his own family, and any little article which was appropriated to himself, from what belonged to others. In his childhood, the most noticeable circumstance relating to him, was an eager desire to strike upon his fore-teeth any thing he could get hold of; this he would do for hours; and seemed particularly gratified if it was

a key, or any instrument that gave a *sharp sound* when struck against his teeth. This would seem to indicate that the auditory nerve was not altogether dormant.

“ In 1808, and again in 1810, his father carried him to London, where operations were performed upon his eyes by the most eminent practitioners, with *very little*, or rather with *no* (permanent) success *; while an attempt that was made at the same time, to give him the sense of hearing, by piercing the tympanum, totally failed.

“ Such is the brief *history* of this poor lad: it remains now to give some account of his appearance, behaviour, the feelings by which he seems to be actuated, the manner in which he conveys his desires, and the methods by which he is managed.

“ 1. His countenance, notwithstanding his unfortunate defects, does by no means indicate fatuity; nay, the lineaments of thought are very observable upon it. His features at times, (in church, for instance, and during the time of family prayer), are perfectly composed and sedate; when sensible of the presence

* That *one* of these operations was attended with considerable success in the *first instance*, appears not only from the extracts already copied from Mr WAR-DROP's narrative, but from the following passage in a letter to that gentleman from the Reverend Mr MITCHELL. This letter is dated 5th October 1810, about a month after Mr MITCHELL and his son had left London, to return home by sea.

“ JAMES seemed much amused with the shipping in the River, and till we passed Yarmouth Roads. During the rest of the passage, we were so far out at sea, that there was little to attract his notice, except the objects around him on deck.
His eye is now pretty free of the redness it had when he left town, and the cataract in the same moveable state, sometimes covering more and sometimes less of the pupil. Though his sight is not much increased since we left London, yet I am perfectly satisfied that what he has got is of essential service.”

sence of a stranger, or of any object which awakens his curiosity, his face appears animated; and when offended or enraged, he has a very marked ferocity of look. He is (for his age) of an athletic form, and has altogether a robust appearance.

“ 2. He behaves himself in company with much more propriety than could be expected; a circumstance owing undoubtedly to the great care of his parents, and of his elder sister. He feeds himself. When a stranger arrives, his smell immediately and invariably informs him of the circumstance, and directs him to the place where the stranger is, whom he proceeds to *survey* by the sense of touch. In the remote situation where he resides, male visitors are most frequent; and, therefore, the first thing he generally does, is to examine whether or not the stranger wears boots; if he does wear them, he immediately quits the stranger, goes to the lobby, feels for, and accurately examines his whip; then proceeds to the stable, and handles his horse with great care, and with the utmost seeming attention. It has occasionally happened, that visitors have arrived in a carriage, and, on such occasions, he has never failed to go to the place where the carriage stood, examined the whole of it with much anxiety, and tried innumerable times the elasticity of the springs. In all this he is undoubtedly guided by the smell and touch only, without any assistance from sight; for, going to call lately for his mother, I passed him, near to the house, within a few feet, without his noticing me in the least; and offering him a glass of punch after dinner, he groped for it, as one in total darkness.

“ 3. The feeling by which he appears to be most powerfully actuated, (at least to a stranger), is curiosity, or an anxious desire to make himself acquainted with every thing that is new to him. He appears to feel affection to those of his family

mily very strongly;—discovered extreme sorrow on account of his father's death; laid himself upon the coffin, after his father's corpse was put into it, apparently in much grief; went frequently to his grave, and threw himself upon it, whilst he gently patted the turf, and bemoaned himself greatly. He is likewise capable of feeling mirth, and frequently laughs heartily. He is highly gratified by getting new clothes; and as tearing his clothes is the most usual expression of his anger, so the punishment he feels most is being obliged to wear them after he has torn them. He is subject to anger, upon being crossed in any of his desires, or when he finds any of his clothes, or articles with which he amuses himself, removed from the chest in which he keeps them.

“ 4. Respecting the manner in which he conveys his feelings and desires, I am much at a loss to give the information that might be expected. It is certain that those of his family know perfectly in what temper of mind he is, and what he wants to have; and these intimations he conveys to them in the presence of strangers, without these last being sensible of his doing so. When he is hungry, he approaches his mother or sisters, touches them in an expressive manner, and points towards the apartment where the victuals are usually kept. If he wants dry stockings, he points to his legs; and in a similar way, intimates his wishes upon other occasions. A pair of shoes were lately brought to him, and on putting them on he found them too small. His mother then took them, and put them into a small closet; soon after a thought seemed to strike him, and he contrived to obtain the key of the closet, opened the door, took the shoes, and put them upon the feet of a young lad who attends him, whom they suited exactly. This action of his implies con-

siderable reflection, and shews that he must have made some accurate examinations, though unnoticed at the time. When he is sick and feverish, which sometimes happens, he points to his head, or takes his mother's hand and places it opposite to his heart, seemingly with an intention that she may observe its beating more quickly than usual. He never attempts to express his feelings by utterance, except when angry, when he bellows in a most uncouth manner. Satisfaction or complacency he expresses by patting the person or object which excites that feeling. His smell being wonderfully acute, he is frequently offended through that sense, when other persons near to him smell nothing unpleasant; he expresses his dissatisfaction on such occasions, by putting his hand to his nose, and retreating rapidly. His taste seems also to be exquisite, and he expresses much pleasure by laughing and smacking his lips, when any savoury victuals are laid before him.

“ 5. His father, when alive, was at much pains in directing him, as his mother still is; but his elder sister seems to have a much greater ascendancy over him, and more power of managing him than any other person. Touching his head with her hand seems to be the principal method which she employs in signifying her wishes to him respecting his conduct; this she does with various degrees of force, and in different manners; and he seems readily to understand the intimation intended to be conveyed. In short, by gratifying him when he acts properly, and withholding from him the objects of his complacency when he has done amiss, he has been taught a sense of what is becoming in manners, and proper in conduct, much stronger than it could be otherwise believed; that any person, in his singularly unfortunate situation, could acquire.”

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SINCE the foregoing narrative reached me, I have had the good fortune to receive a most important and authentic supplement to it, from Dr JOHN GORDON; a gentleman, on whose recent admission into our number, I beg leave to congratulate the Society. Having communicated to him, on the suggestion of our colleague Dr JOHN THOMSON, Mr GLENNIE's letter with the inclosed statement, he most obligingly undertook, on a very short notice, to add to it whatever particulars relative to the same subject had fallen under his own personal knowledge. Of the ability with which he has executed this task, amidst his various professional avocations, I have no doubt that the Society will think as highly as I do.

Supplement to the foregoing Account of JAMES MITCHELL, by
JOHN GORDON, M. D.

“ The boy who is the subject of the above interesting communication, was brought by his father to visit me at Forres in the autumn of 1808.

“ I found on examination that he had a cataract in each eye. In both, the crystalline lens had a pearly colour, and appeared to be of a firm consistence; but the pupils exhibited very perceptible contraction and dilatation, when the quantity of light was suddenly increased or diminished. The auricle or external part of each ear, and the tube leading from it to the tympanum, were of their natural size and form; and nothing unusual.

usual could be discovered in the conformation of the parts about the fauces.

“ From the motions which were produced in the iris, by varying the quantity of light admitted to the eyes, I should alone have been inclined to hope, that the retina was not altogether wanting, and to have urged the propriety of attempting to remove the opaque lens from the axis of vision by a surgical operation. But the following circumstances served to confirm this opinion. In the first place, Mr MITCHELL informed me, that he had often observed his son, sitting for an hour at a time, opposite to a small hole in the south wall of a hut adjoining to the manse, so as to receive the beams of the sun, which shone through the hole during part of the forenoon, directly on his eyes. The boy could have no other motive for placing himself in this situation, but to enjoy a certain agreeable sensation of light; and it is not improbable, that the particular pleasure which he seemed to derive from the light of the sun, admitted in this manner, arose from the eyes having been rendered more susceptible to impressions, by being previously directed to the darker parts of the hut. Secondly, I observed, that he very frequently turned his face towards the window of an apartment, and then pressed his finger forcibly backwards between the eyebrow and upper eyelid of one of his eyes, so as to occasion a slight degree of distortion, and a very disagreeable appearance of protrusion of the ball. I supposed, that when he compressed the eye-ball in this manner, either some change in the organ was produced, by which he obtained a more distinct impression from the light of the window, or else that the pressure on the retina simply, occasioned the sensation of a luminous ring or spot, which he had pleasure in contemplating. When I put my silver pencil into his hand, after turning it quickly round in the points of his fingers,
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and applying it to his nose, lips, and the tip of his tongue, he rattled it smartly between his fore-teeth; and his father assured me that he did so with every hard substance which he could convey to his mouth, and that he seemed to have pleasure in repeating this motion with metallic bodies in particular. This circumstance led me to conclude, that vibrations communicated through the solid parts of the head, were capable of producing in him, to a certain degree, the sensations of sound. But these sensations were obviously so very weak, when compared with those which persons who are affected with obstruction in the eustachian tubes, can at all times enjoy through the medium of the bones of the head, that I could not but fear that the deafness in this case, depended not on any want of air in the tympanum, but on some great deficiency, or radical imperfection in the structure of the auditory nerve. Although, therefore, no harm could result from piercing the membrane of the tympanum, I did not expect that the sense of hearing would be much improved by this operation.

“ With this opinion of the boy’s situation, I earnestly recommended it to his father to carry him to London, and to place him under the care of Mr WARE and Mr ASTLEY COOPER, in order that the operation of couching or extraction might be performed on one or both eyes, and that the membrane of the tympanum in each ear might be perforated.

“ In the course of a few weeks, Mr MITCHELL repaired with his son to London. Mr ASTLEY COOPER pierced the membrane of each tympanum, but without the slightest benefit; and, at the same time, the late Mr SAUNDERS operated with the needle on the left eye, and, it is to be presumed, used every effort which the violent struggles of the boy would permit, to depress the cataract; but not the least advantage resulted from the operation.

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“ In the summer of 1810, several months after his return from London, young MITCHELL was again brought by his father to visit me at Forres. When I placed him in a chair before me, and took hold of his head, with a view to examine his eyes, his situation seemed immediately to recall to his memory the painful operations with which this examination had been formerly succeeded, and he withdrew from me in his chair, panting as from a sudden alarm. By patting him gently on the cheek, however, his fears were quickly allayed. The cataract of the left eye, into which the needle had been introduced, had lost its white colour, and seemed as if broken down ; but still the lens remained opaque, and he was in every respect as blind as when I first saw him. The pupil, however, of each eye was very distinctly enlarged when I placed my hand before his face, and it again contracted when the hand was removed ; and I observed with great satisfaction, every time I practised this experiment, that when the quantity of light admitted to the eye was increased, the boy expressed his pleasure by a smile. The cataract of the right eye had the same appearance of firmness as before, and I therefore still entertained hopes, that it might be practicable to remove it entirely by the operation of extraction. On stating this opinion to Mr MITCHELL, to the honour of whose memory it ought to be remarked, that he displayed at all times the most earnest anxiety to alleviate the sad condition of his child, he immediately resolved to visit the metropolis once more ; and, in compliance with my request, to entrust the treatment of his son, entirely to the judgment and practical skill of my friend Mr WARDROP. In a few weeks Mr WARDROP wrote to me, that having resolved to attempt extracting the cataract from the right eye, he had endeavoured, by means of powerful machinery, as well as the aid of numerous assistants, to fix the boy's head in a position sufficiently
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steady for so delicate an operation ; but that his struggles were so violent as to render every effort for this purpose ineffectual. The attempt at extraction was therefore relinquished ; but, soon afterwards, I had the satisfaction of being informed by Mr WARDROP, that he had so far succeeded, by the use of the couching-needle, in breaking down the cataract, and removing it from the axis of the eye, that his young patient had been able to see a very small object of a white colour, when placed on a table before him. This partial success from Mr WARDROP's operation, led me to anticipate, with no small confidence, a still further improvement in young MITCHELL's vision; from the gradual absorption of some of the broken fragments of the opaque lens or its capsule. But in this expectation I have been altogether disappointed. In the month of June last, I saw him repeatedly at his father's house, and had ample opportunity of observing his motions with attention. When he approached any object, such as a wall, a cart, or a carriage, so large as to be in part interposed between his eyes and the horizon, he seemed to discover its vicinity by the interception of the light which it occasioned alone, and cautiously put out his hands before him, to feel for that with which he was already almost in contact. But he did not appear to be at all capable of perceiving minute objects, nor of distinguishing in the slightest degree between one colour and another. His powers of vision, therefore, so far from continuing to improve since the successful result of Mr WARDROP's operation, have but too plainly undergone a degree of failure. A fragment of the substance of the lens, or of its capsule, very white and opaque, may still be seen behind one-half of the pupil, and through the lower half, a slighter opacity is very perceptible in the parts situated farther back*.

VOL. VII.

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* " You will perceive, from the account of the state of the cataract immediately after the operation, that a part of the opaque body still hung over

“ On the whole, I am of opinion, that if this unfortunate, but very interesting boy, should at any future period be induced, either by being informed through the medium of some peculiar language, of the object in view, or from his increased knowledge in the kindness and good intentions of his relations, to submit patiently to the operation of couching or extraction, either of these operations ought to be repeated on one or both eyes. At the same time, it must be confessed, that, since the attempts of this kind already made, have not only failed to communicate to him the powers of distinct vision, but also the perception even of the more striking differences in the degrees and kinds of light, there is but too much reason to fear, that the optic nerve, although not entirely deficient, is yet imperfect in its structure.

“ I have but little to add to the full account which is given in Mr GLENNIE’s communication, of young MITCHELL’s general appearance and conduct. The knowledge which he has derived from the senses of Touch, Taste and Smell, seems fully as extensive, as what any person of the most perfect faculties might be supposed to acquire, if he could by any contrivance be prevented from using his eyes and ears for the same period of time, from the moment of his birth, and in the same retired situation of the country. The train of his thoughts seems to be regulated by the same principles as that of the soundest minds. His actions neither indicate incoherence nor fatuity; but every thing he does, appears capable of being easily traced to rational motives.

a portion of the pupil. I have been told lately, that he now sees little or none. If this be the case, I suspect it must have been from the cataract passing over the whole of the pupillar opening, instead of being altogether absorbed, or remaining out of the way, as might have been expected.”

*Extract of a letter from Mr WARDROP to Mr STEWART,
(dated August 10. 1812.)*

tives. His more pleasurable sensations are obviously enjoyed from the senses of Taste and Smell; and, indeed, I have never observed any thing disagreeable in his manner, except the keenness and voracity with which he devours his food. But he derives amusement also from the sense of Touch. His father told me, that he had often remarked him, employing many hours in selecting from the bed of the river, which runs within a few yards of the house, stones of a round shape, nearly of the same weight, and having a certain degree of smoothness. These he placed in a circular form on the bank, and then seated himself in the middle of the circle.

“ There is a certain range around the manse which he has minutely explored by his organs of Touch, and to any part of this space he seems to walk, when he pleases, fearlessly and without a guide. I believe his range does not yet extend beyond two hundred yards in any direction; but there is probably not a day elapses, during which he does not cautiously feel his way into ground which he had not explored before; and thus gradually extends his yet very circumscribed field of observation. It was in one of these excursions of discovery, that his father observed him with horror, creeping on his hands and knees along a narrow wooden bridge which crossed the river, at a point where the stream is deep and rapid. He was immediately arrested in his progress; and as his father wished to discourage him from hazarding so perilous an attempt again, a servant was directed to plunge him, as soon as he was secured, once or twice into the river. This measure has had the desired effect.

“ From a similar solicitude about his safety, the servants had been enjoined to prevent him from visiting the stable, which he never fails to do, the instant he has discovered by

the presence of an additional whip in the lobby, that the person who has arrived has brought a horse with him. I have been assured, however, that after his wishes in this respect had been repeatedly thwarted, he at last had the ingenuity to lock the door of the kitchen on the servants, in the hopes that he might then accomplish his visit to the stable unmolested.

“ His father once told me an anecdote of him, which displays in a very striking manner, both the retentiveness of his memory, and the benevolent feelings of which he is susceptible. He had received a severe wound in his foot, and during its cure, he usually sat by the fire-side, with his foot resting on a small foot-stool. More than a year afterwards, a servant-boy with whom he used to play, was obliged to confine himself to a chair from a similar cause. Young MITCHELL perceiving, that his companion remained longer in one situation than he used to do, examined him attentively, and seemed quickly to discover by the bandages on his foot, the reason of his confinement. He immediately walked up stairs to a garret, sought out, amidst several other pieces of furniture, the little foot-stool which had formerly supported his own wounded limb, brought it down in his hand to the kitchen, and gently placed the servant-boy's foot upon it*.

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* Somewhat similar to the above anecdote, is the following very pleasing fact, communicated to Mr GLENNIE by HUGH IRVINE, Esq; (son of Mr IRVINE of Drum). I give it in Mr IRVINE's own words.

“ Mr LESLIE of Darkland, a clergyman, called one day, and was taken by Miss MITCHELL to see something out of doors. When they returned, JAMES MITCHELL perceived (no doubt by the sense of smell) that his sister's shoes were wet: he then went and felt them, and would not let her rest till she changed them.”

“ The last time I saw young MITCHELL, was on the melancholy occasion of his father’s funeral, in the month of June last. According to Mr GLENNIE’s communication, it would seem, that the boy, even before his father’s interment, had expressed by sorrow and bemoaning, a knowledge of the irreparable loss he had sustained. On this point, the deep distress under which the family then laboured, prevented me from making any inquiries. But the poor lad’s behaviour on the day of the funeral, seemed to me so little expressive of grief, that I cannot help doubting in some degree the accuracy of Mr GLENNIE’s information. It will be regarded as a pleasing testimony of the sincere esteem in which Mr MITCHELL was held for his moral worth and exemplary piety, that several hundreds of his friends and parishioners assembled together, to carry his remains to the grave. While this concourse of people waited the commencement of the procession in front of the manse, young MITCHELL at one time moved rapidly among the crowd, touching almost every body, and examining some very minutely ; at another time, he amused himself opening and shutting the doors, or turning down and up the steps of the carriages ; or suddenly he would walk towards the coffin, which was placed on chairs before the door of the house, run his fingers along it with marks even of pleasure, and then trip lightly away, without the slightest expression of sorrow. He accompanied the procession to the church-yard, and returned after the interment, apparently as much unmoved as before. But on the following morning, as I have since been informed, and on several mornings afterwards, he visited the grave, patted gently the turf which had been laid over it, and at last, as if

if hopeless of his father's return, became sorrowful even to tears *."

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* Soon after this memoir was read, I informed Mr GLENNIE of the difference in the accounts given by Mr MACFARLANE and by Dr GORDON, of young MITCHELL's behaviour on the day of his father's funeral. In a letter with which he has lately favoured me (dated May 10. 1812,) there is a passage transcribed from a letter of Mr MACFARLANE's (dated May 7.) which I think it proper to subjoin to the foregoing details, as an important document with respect to this interesting point;—the only point of any consequence in which the two papers do not perfectly agree.

“ In the account which I transmitted to you of JAMES MITCHELL, I mentioned that he seemed much afflicted and very sorrowful the day of his father's funeral; and I now beg leave explicitly and positively to state, that when the coffin which enclosed his father's corpse was brought from the house, and placed upon chairs in the court before the manse, previous to the interment, I approached to the coffin, and soon after saw JAMES MITCHELL come from the house in considerable agitation. He turned about his head rapidly, and *snuffed* very much, evidently guiding himself by the sense of smell. He directly approached the coffin, smelled it most eagerly for several seconds; then laid himself down upon the lid, on his face, and embraced the coffin, while his countenance discovered marks of the most lively sorrow. I stood close by him, and after a short time, patted his head once or twice; upon which he rose, and returned into the house. This occurred immediately upon the coffin being brought out, and about twenty minutes before it was lifted, in order to be carried to the church-yard. As the accuracy of my information on this subject has been doubted, I purposely delayed writing to you, till I should have an opportunity of conversing with the Reverend PRYSE CAMPBELL, minister of Ardersier, brother-in-law to Mrs MITCHELL, who was present at the funeral, and by whose direction every thing was conducted. I fell in with this gentleman on Tuesday se'ennight, at the meeting of our Provincial Synod. I took an opportunity there of asking him, if he observed any marks of sorrow about JAMES MITCHELL on the day of his father's funeral. He replied, that he did observe the most unequivocal marks of grief in his countenance; and added a circumstance which escaped my notice, that
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THE case described in the foregoing papers is said by Mr WARDROP, in a letter of his already quoted, to be *unique*, to the best of his knowledge; and that it really is so, I am inclined to believe, as far as this can be inferred from the silence of scientific writers*. That it is, at least, a very *rare* occurrence, is demonstrated

when the coffin was about to be lifted, in order to be carried to the church-yard, JAMES MITCHELL clung to it, endeavouring to prevent its being carried away, and that he (Mr CAMPBELL) was obliged to remove him from it by force."

After quoting the above passage, Mr GLENNIE adds:—"Mr MACFARLANE, in his remarks on the apparent inconsistency between Dr GORDON's account of young MITCHELL and his own, has expressed what occurred to me immediately after I read your last letter. His words are: "I would observe, that the circumstances mentioned by Dr GORDON, of MITCHELL's running through the crowd, and touching every person, do not, in my opinion, amount to a proof, that he was insensible of the loss which he had sustained, and felt no grief on that account. In acting thus, MITCHELL (if the expression may be allowed) was merely *viewing* the assemblage of people around him. This he could not do by his eyes; but being eager to examine them, he did so by means of the senses of which he has the use. In short, he was grieved; but, in this instance, his curiosity overcame his grief." The remark certainly does honour to Mr MACFARLANE's sagacity, and, in my opinion, goes far to reconcile the two narratives. I hope to be able soon, through Dr GORDON's means, who proposes to pass a part of this summer in that neighbourhood, to obtain from the mother and sister of the young man, a still more circumstantial account of his general behaviour, and of the apparent state of his feelings at this trying crisis of his life. Some very interesting particulars, with respect to these points, (which have been already communicated to me by this gentleman) may be found in an Appendix annexed to this memoir. (May 20. 1812.)

* In DIDEROT's very ingenious and fanciful Letter on the Blind, there are various allusions to the hypothetical case of an *Aveugle-Sourd-Muet*. In one passage,

monstrated by a passage in the Abbé SICARD'S Course of Instruction for the Deaf and Dumb, where it is mentioned only as a hypothetical contingency, which had been contemplated by him and by his predecessor the Abbé de L'ÉPÉE, as a possible,

sage, he remarks, somewhat whimsically, that if a person born in these circumstances, should begin to philosophize concerning man, according to the method of DESCARTES, he would place the seat of the soul at the tips of his fingers; and, in all probability, after an effort of profound meditation, would feel his fingers ache as much as we should do our heads. From the following sentence, one would be led to suppose, that DIDEROT had actually seen or heard of persons in the same condition with MITCHELL; but if this really had been the fact, we may presume with some confidence, that he would not have contented himself with so vague and equivocal a reference to an occurrence at once so anomalous and so curious in the physical history of man. "*Faute d'une langue, la communication est entièrement rompue entre nous et ceux qui naissent sourds, aveuglés, et muets: ils croissent, mais ils restent dans un état d'imbecillité.*"

In those valleys of the Alps, indeed, where the disease of *Crétinisme* is common, examples are said frequently to occur of an almost total deprivation of all the senses; but, in such instances, the individual presents invariably, in the low and humiliating state of his intellectual capacity, a very striking contrast to the subject of this memoir. The *universal* torpor in the perceptive faculties of the *Crétin*, is plainly an effect of the same radical disorder which impairs his intellect; whereas, in the instance before us, (as in every instance where the intellect is entire), the mind, checked and confined in the exercise of one class of her powers, displays her native strength by the concentrated energy which she exhibits in others. The following description relates to *an extreme case of Crétinisme*; for it appears, that it admits of various gradations. It is taken from the most circumstantial, and apparently the most accurate, account of this local malady that has fallen in my way.

"The sensibility of the *Crétin* is extremely obtuse: he dreads neither cold nor heat, nor vermin; nor even those blows which would be insupportable to another.

"The greater part are evidently deaf and dumb; although I have happened to see a few who would shudder at the report of a pistol. These last would seem to receive some passive impression from sound; but they are certainly incapable

sible, and not altogether as an improbable event, among the various physical calamities to which our species is liable. It appears from the same ingenious author, that the Abbé de L'ÉPÉE had even gone so far, a few years before his death, as to offer, in some of the Continental Journals, with his characteristic benevolence, to undertake the charge and tuition of any child who might be brought into the world in these unfortunate circumstances; and M. SICARD has not only taken the trouble to record the general principle on which the Abbé de L'ÉPÉE intended, if this accident should occur, to have proceeded in the education of his pupil; but has added some very judicious strictures of his own, on the imperfections of the plan which his predecessor proposed, in such an instance, to follow.

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capable of *listening* to what is passing around them. The organ of Smell is insensible; and the power of Taste but imperfectly developed. The sense of seeing alone appears uninjured by the disorder; but even from *this* they derive little benefit. They gaze with indifference on the spectacle of Nature; and if they see, can hardly be said to *perceive*."

"This disease is peculiar to the human species. All the classes of animals, from the oyster to the monkey, possess a sufficient degree of *intelligence*, to procure the means of their own subsistence. The *Crétin*, on the other hand, would die of hunger, if his wants were not provided for by the attentions of others."

(*Traité du Goître et du Crétinisme, par F. E. Foderé, Ancien Médecin des Hôpitaux civils et militaires. A Paris, an VIII.*)

Since this note was written, I have received a letter from Mr GLENNIE, in which he remarks, and, in my opinion, very justly, that the case of MITCHELL is probably not so *very* rare an occurrence, as we might, at first, be disposed to imagine. "Among the various merits (he observes) of this worthy family, their superiority to such prejudices as would have precluded our getting any information about the lad's state of mind, is deserving of peculiar notice. I have reason to believe, that there are others in circumstances similar to young MITCHELL's, whose cases are, at this day, kept so secret, that they are not so much as known to the inmates and members of the family to whom they belong."

These strictures I cannot help taking this opportunity of recommending to the attention of those who may attempt the farther instruction of young MITCHELL. The following abridged translation* of a passage in the preface, may, in the mean time, suggest some useful hints:

..... “ But, if there should be found a person deaf and dumb, in whose case the use of this visible language was impracticable; if, among the afflicting exceptions and mutilations of nature, an individual should occur, deaf and blind from his birth, to what class of *signs* should we have recourse in attempting *his* education? At what an immense distance from other men would a being so cruelly degraded be placed; and how difficult to transport him across that gulf by which he is separated from the rest of his species? The means of instruction employed in ordinary instances of dumbness, would here be manifestly inapplicable; all of these means presupposing the use of sight, to which a constant reference is made, not only in the communication of physical ideas, but in typifying the processes of thought, and in rousing the dormant powers of the understanding.

“ I flatter myself, I have already proved, that, from the beginning, Man possessed, in his own bodily organs, two different *media* for conveying his ideas; and that, instead of employing *oral speech*, he might have had recourse to a *manual language*. Why, then, might we not, in the supposed case of

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* In this translation, I have not only omitted several sentences in the original, which did not appear to bear upon my present object, but have not scrupled to interpolate a few clauses of my own, which I thought might be useful in conveying the author's meaning more clearly to an English reader. The sense of the passage is rendered, to the best of my judgment, with perfect fidelity.

a blind and deaf pupil, avail ourselves of the assistance of the latter, which, if not visible to his eye, would be, at least, tangible to his hand. It is only extending farther the use of a species of signs already practised between MASSIEU and me, when, during the darkness of night, he *sees* by his own hands whatever mine would express to him. Why should not the blind and deaf pupil be taught to converse in the same manner, during the light of day?

“ Ah! if the experiment I should wish to make were to prove not altogether useless; if, as I have already done for the deaf and dumb, I should be the instrument of bestowing a *mind* on this still more unfortunate object, I should myself enjoy a degree of happiness greater than any which *he* could possibly derive from the success of my labours!

“ My illustrious predecessor had the boldness to think, that even *this* case, if it should be realised, would not present unsurmountable obstacles to an instructor. The following is an outline of the plan upon which, he told me, it was his intention to proceed.

“ An alphabet of polished steel was to be employed as the *materials* of his nomenclature for sensible objects, and for those *actions* which might be brought under the cognizance of the sense of Touch. He hoped to be able to familiarise his pupil with these characters, so as to devolve upon his hands the office of his eyes; and, for this purpose, he proposed to make him feel the object with one hand, while he was learning to distinguish its name with the other. His inventive genius would doubtless have led him, in the course of his experiments, to whatever other means were necessary for the attainment of his end.

“ I am perfectly aware, that difficulties would immediately present themselves at every step; for, how would it be possi-

ble, without any intercourse either by the eye or by the ear, to establish, in the pupil's mind, the connection between the object and its sign? I apprehend, it would be necessary here to avail ourselves of some of his animal instincts; to withhold, for example, the objects of his desires and wants, till he should recollect their names, and exhibit their characters*. This first step would perhaps be followed by a second; that of teaching him to distinguish the qualities and modes of objects. Colours and sounds would, of course, be excluded from our lessons; but the *forms* of bodies, which fall under the province of Touch, might be easily impressed on his memory; and upon this basis, what should prevent us from proceeding to rear a metaphysical structure? If those qualities which strike the sense of Sight have gradually led the deaf and dumb to the knowledge of things intellectual and moral, why should not the qualities about which the sense of Touch is conversant, be made the channel to the same sort of information? Instead of speaking to the eye, we have only to speak to the hand. In truth, the whole system of instruction explained in the following work, might be adapted to our new pupil, by presenting to him, *in relief*, the various delineations and diagrams by which it is illustrated; those slight changes being
made

* In the case of MITCHELL, the difficulties here alluded to would probably be experienced in a comparatively small degree, in consequence of the previous use of those significant pressures on his forehead, of which his sister has taught him to comprehend the meaning. If this should turn out to be the fact, she has already got over, by her own ingenuity, the first and most arduous step in the whole process of his education.

Mr WARDROP takes notice, in one of his communications, of his extreme docility, and of his obedience to the commands of his sister, who, during his stay in London, "was his constant companion and guide." "It was astonishing (he adds) how readily she could communicate to him, by signs, her wishes."

made in the method, which the circumstances of the case would readily suggest. *This* pupil, (as has often happened in the education of the deaf and dumb), would soon become the master of his teacher; and every step which was gained, would of itself, point out the next which was to be taken*.

“ May such a system of instruction remain always matter of pure speculation! God forbid, that a child should ever be brought into the world, without any substitute but the hand for the eye and for the ear! But as, unfortunately, such a deviation of nature from her usual course, is an event but too possible, let us consider beforehand what may be done, by way of remedy or of alleviation. To restore a Man to society, to his family, and to himself, would be an enjoyment too exquisite, and a conquest too proud, to permit us to abandon the undertaking in despair.”

For a comment on the above observations, I must refer to the work at large. They who read it with attention, and who enter fully into the author's views, will be at no loss to perceive the different modifications which his plan will require, in

* It is somewhat surprising, that the Abbé SICARD should have overlooked the aid which the sense of Smelling seems so peculiarly calculated to furnish, for rearing his proposed *Metaphysical Structure*. Some of the most significant words relating to the Human Mind, (the word *sagacity*, for instance), are borrowed from this very sense; and the conspicuous place which its sensations occupy in the poetical language of all nations, shew how easily and naturally they ally themselves with the refined operations of the Fancy, and with the moral emotions of the Heart. The infinite variety of modifications, besides, of which they are susceptible, might furnish useful resources, in the way of association, for prompting the memory, where it stood in need of assistance.

One of the best schools for the education of such a pupil, would probably be a well-arranged Botanical Garden.

in applying it to such a case as that of MITCHELL. His fundamental principles are general, and deeply philosophical; being, all of them, deduced from a careful study of the steps by which children gradually and insensibly acquire the use of oral speech; and of consequence, they are equally applicable to every species of *signs* by which one mind can hold intercourse with another. In the mean time, I beg leave to add to the foregoing quotation, the account given by SICARD of his first lesson to MASSIEU, as it touches on a very natural mistake, which, with a few, if any exceptions, has misled all those who have hitherto undertaken the education of the deaf and dumb; and which, in case any attempt should be made for the farther improvement of MITCHELL, it may be worth while to point out, by way of caution, to his instructors.

“ My first lesson was employed upon the alphabet. I had not yet reflected on the imperfection of this method, which, from the first outset, counteracted that analytical procedure which is natural to the mind, and by which alone the mind can be guided to the use of its faculties. What information, in reality, (as I afterwards began to question myself), can the understanding possibly derive from a series of abstract characters, arranged in a particular order by chance or caprice, and to which nothing equivalent can be exhibited in Nature? But it was thus that my illustrious Master began, and every step in his system seemed to me then indispensable and sacred.”

In a subsequent passage, M. SICARD takes notice still more explicitly, of the absurdity of teaching a pupil in such circumstances to read or to copy isolated letters, in that order which our alphabet exhibits. “ What interest, (he asks), could MASSIEU have felt about characters signifying nothing, and occupying, without any conceivable reason, a certain place in an arbitrary series? Accordingly, I directed his attention at once to
words,

words, without attempting to explain to him that the elements of these words were letters, and still less that these letters were consonants and vowels. Indeed, how was it possible for him to annex any notion to the technical terms of grammar, when he was not yet in possession of a language, and when he had only a few fugitive notions to fix and to express?"

In these extracts, M. SICARD describes, with great candour, the process of thought by which he was conducted to (what I consider as by far the most important of the many improvements which he has introduced into his art) the simple, yet luminous idea, of copying his plan of instruction, not from the example of a schoolmaster teaching a child to read, but from the example of the child itself, in acquiring the use of its mother-tongue. Of these two methods, the former, it must be owned, is by far the more obvious; and where mere articulation is the chief object of the teacher, it will probably be found the more easy and effectual in practice. But SICARD'S aim was of a different, and of a higher nature;—not to astonish the vulgar by the sudden conversion of a dumb child into a speaking *automaton*; but, by affording scope to those means which Nature herself has provided for the gradual evolution of our intellectual powers, to convert his pupil into a rational and moral being. The details of his lessons, accordingly, are not more interesting to the few, who may attempt the education of such unfortunate *exceptions* as MASSIEU or MITCHELL, than to all those who delight in tracing to their elementary principles the materials of human knowledge, and in marking the first openings of the infant mind*.

IN

* See the Note at the end of the Memoir.

IN order to complete the history of MITCHELL, I am aware that a variety of curious points still remain to be ascertained ; and, if I had not been anxious to bring it forward to public notice, even in its present imperfect state, without any farther delay, I should have been inclined to retain it in my own hands, till my information on the subject should have been a little more ample. My wish, I must acknowledge, is, That some plan could be devised for removing the young man to Edinburgh ; or rather (as he has been accustomed hitherto to enjoy the air and the freedom of the country), to some quiet residence in the neighbourhood ;—to some situation, in short, where an opportunity would be afforded for examining and recording, under the eye of this Society, the particulars of a case, to which it is to be hoped, that nothing similar will again occur in our times. Something, it would appear from Dr GORDON'S statement, may perhaps, at a future period, be attempted for the extraction of his cataracts,—in which event (should the operation succeed), I need not say, what an accession would at once be made to his own enjoyments, and to his value as an object of philosophical curiosity :—But even on the supposition that this hope should be disappointed, a subject of inquiry not less interesting than any question connected with the Theory of Vision, will still remain,—to ascertain how far it might be possible, by following out the Abbé SICARD'S hints, to cultivate the intellectual and moral faculties of a human being, destitute of the two senses which are the ordinary vehicles of all our acquired knowledge. Nor do I apprehend that this experiment would be attended with such insuperable difficulties

ties as might at first be suspected ; as I am assured by the best authority, that his eldest sister, whose good sense has already devised some imperfect modes of communication with her unfortunate brother, possesses talents which fully qualify her to carry into execution any plan that may be proposed for his farther improvement. His age, at present, only exceeds by two years, that of SICARD's celebrated pupil MASSIEU, when his education was begun ; and at that period, MASSIEU, though he had the inestimable advantage of possessing the sense of Sight, seems to have had his rational faculties as imperfectly developed as those of MITCHELL.

I must, at the same time, observe here, in justice to myself, that my expectations of the future improvement of the latter, are by no means so sanguine as those which the Abbé SICARD would probably have indulged in similar circumstances. Were it possible, indeed, to place him under the immediate tuition of that eminent man, I have little doubt that much more would be accomplished than appears to us to be practicable ; but the difference between his situation and that of MASSIEU is so immense, as to render all our conclusions founded on the history of the one, quite inapplicable (except with great modifications) to the case of the other. The slowness with which the sense of Touch proceeds, in collecting information concerning the external world, when compared with the rapid perceptions of the Eye, would, on the most favourable supposition, retard infinitely the rate of his progress in acquiring even the first elements of knowledge. This, however, furnishes no argument against the attempt ; nor does it even tend to diminish the value of the results to which it might lead. The slightest addition that could be made to his present range of ideas, by means of an improved system of

Vol. VII. For signs ;

signs; and still more, the slightest developement that could be given to any of his dormant powers, might afford not less important *data* for philosophical speculation than the most extensive acquisitions.

HAVING mentioned more than once the name of MASSIEU, I think it proper to subjoin to the preceding remarks, the description of him given by SICARD, at the time when their acquaintance commenced. It may serve to shew, that the idea of attempting the education of MITCHELL, even at the age of sixteen, is not altogether chimerical.

“ The reader will easily form an idea of the character and manners of MASSIEU, when he is told, that he was born in a cottage, six leagues from Bordeaux; and that his relations (who were the only individuals with whom he ever had any intercourse) had not even taken the trouble to communicate to him the slightest information about material objects. His childhood had been spent in tending a flock; and all his ideas were confined within the narrow circle which had fallen under his random observation. MASSIEU was a Man of the Woods; untinctured with any habits but such as were purely animal; astonished and terrified at every thing. In coming to Bordeaux, he had believed that he was only changing his place of abode; and that he would be employed there in keeping another flock: but it was towards the beloved scene which he had left, and which had witnessed the first sports of his infancy, that his imagination was incessantly turned. In every thing he saw, he apprehended some danger; in every step he was directed to take, he suspected some snare. How far was this simple boy, accustomed as he was to consider himself on a level with the animals entrusted to his care, from dreaming that he was about to be raised by education to the rank of Man! His
clouded

clouded and inexpressive countenance ; his doubtful and shifting eye ; his silly and suspicious air,—all seemed to announce, that MASSIEU was incapable of any instruction. But it was not long till he began to inspire his Teacher with the most flattering hopes.”

Abstracting, however, entirely from all conjectural speculations with respect to MITCHELL's possible attainments in future, the particulars already in our possession afford *data* for some important conclusions concerning the capacities of the Human Mind, considered in contrast with the instincts of the Brutes. For these I do not think that the Transactions of this Society afford a proper place ; and I have accordingly, all along, confined myself to the detail of *facts*, leaving the philosophical inferences to which they may lead for future consideration.

Nor is it, in this point of view alone, that his case is an object of curiosity at the *present* moment. The examination of his powers of external perception (considered merely as articles of natural history *) promise, under the peculiar circumstances of

* I cannot help quoting here a very curious observation of Mr WARDROP's, with respect to the partialities and dislikes conceived by MITCHELL, in consequence of the *moral expression*, (if I may be allowed such a phrase), which he seems to have attached to particular sensations of smell. “ When a stranger (*says* he) approached him, he eagerly began to touch some part of his body, commonly taking hold of his arm, which he held near his nose ; and after two or three strong inspirations through his nostrils, appeared decided in his opinion. If it happened to be unfavourable, he suddenly went to a distance with the appearance of disgust ; if favourable, he shewed a disposition to become more intimate, and expressed, by his countenance, more or less satisfaction.”

his condition, a field of study, of which, if it has ever occurred before in the annals of our species, no scientific use appears to have been made. How much the simultaneous exercise of our different senses obstructs the perfection of each, may be inferred from the delicate touch, and acute hearing of the blind. It remains to be ascertained, to what degrees of improvement, the perceptions of Feeling, of Taste, and of Smell, may attain in an individual possessed of these senses alone*.

I

* In one of the communications which I have received, it is said, that "MIRCHELL has been known to follow the footsteps of another person for two miles, guided merely by the sense of smelling." As this circumstance, however, is stated only on report, I have not introduced it into the text; and mention it here chiefly in the hope of obtaining more precise and authentic information upon the subject.

It would be desirable also to learn something more circumstantial and specific, both with respect to the discriminating powers of his palate, and his predilections in the article of food.

Neque inutile foret, neque ab honestissimâ sapientiâ alienum, novisse quomodo hic miserandus, jam puber factus, se habuerit quod ad res venereas; hunc appetitum an senserit necne; quâ formâ, quibus indiciis se prodiderit; fœminarum an virorum consortio adolescenti magis placeat; socii sexum an olfactu dignoscere videatur. Hæc et similia bene multa, dictu parum decora, scitu verò non indigna, si modò observandi copia data fuerit, unicuique in mentem venient cui Naturæ Humanæ scientia est cordi, quique infelicissimum et penè singularem illius statum ritè contemplabitur.

I shall only add farther, before concluding this memoir, that, in submitting it to the Royal Society, I was partly influenced by the hope, that it may possibly be the means of securing a decent provision for the individual to whom it relates;—perhaps, also, a competent independence for those members of his family on whom he has been so long a burdensome and expensive charge. I allude, not merely to his mother, whose claims must immediately force themselves on the notice of every one, but more particularly to his eldest sister, on whom the duties of superintending and educating him have chiefly devolved from his infancy. To the painful and incessant attentions which his helpless condition required, the best years of her life have been hitherto devoted; and so essential is the continuation of the same affectionate cares to his comfortable existence, that, independently of what is due to her own singular merits, she must, of necessity, be included in any arrangement, of which his improvement and happiness are the principal objects. For the purposes already mentioned in this paper, the funds of the Society, I am well aware, are altogether inadequate; but if they shall be pleased to recommend the business to the consideration of their Council, I have no doubt, that something may be suggested for the accomplishment of a measure, which, even if it should fail in adding materially to the stock of useful knowledge, would at least prevent the regrets which might afterwards be felt, if so rare an opportunity for philosophical observation and experiment should be suffered to pass before our eyes, without any attempt being made to turn it to the advantage of science.

NOTE,

NOTE, p. 39.

I HAVE been led to insist at some length on the philosophical merits of SICARD's plan of instruction for the Dumb, not only because his fundamental principles admit of an obvious application (*mutatis mutandis*) to the case of MITCHELL; but because his book does not seem to have attracted so much notice in this country as might have been expected, among those who have devoted themselves to the same profession. Of this no stronger proof can be produced, than the stress which has been laid by most of our Teachers, on the *power of articulation*, which can rarely, if ever, repay, to a person born deaf, the time and pains necessary for the acquisition. This error was, no doubt, owing, in the first instance, to a very natural, though very gross mistake, which confounds the gift of Speech with the gift of Reason; but I believe it has been prolonged and confirmed in England, not a little, by the common union of this branch of *trade* with the more lucrative one, of professing to cure organical impediments. To teach the dumb to speak, besides, (although, in fact, entitled to rank only a little higher than the art of training starlings and parrots), will always appear to the multitude, a far more wonderful feat of ingenuity, than to unfold silently the latent capacities of the understanding;—an effect which is not, like the other, palpable to sense, and of which but a few are able either to ascertain the existence, or to appreciate the value. It is not surprising, therefore, that even those Teachers who are perfectly aware of the truth of what I have now stated, should persevere in the difficult, but comparatively useless attempt, of imparting to their
pupils.

pupils that species of accomplishment, which is to furnish the only scale upon which the success of their own labours is ever likely to be measured by the public.

The example of Dr WALLIS of Oxford, the most eminent English author who has yet turned his attention to this study, has probably had considerable influence in misleading his successors. His thoughts (as he tells us himself) were originally led to it by his analytical inquiries concerning the mechanical formation of articulate sounds, a subject which he appears to have very deeply and successfully meditated; and accordingly, the first step which he took with his two most distinguished pupils (POPHAM and WHALEY) was to teach them *to speak*. He also informs us, that he had in various instances applied the same principles, in curing organical impediments. Indeed, it was evidently on this branch of his art, that he valued himself chiefly as an instructor of the dumb. In cultivating the intellectual powers of these, his success does not seem to have been such as to admit of comparison with that of the Abbé SICARD; and it is remarkable, that the pupils, of whose progress he speaks most highly, are a few with whom he carried on all his intercourse by means of writing, without wasting any of their time in communicating to them the gift of oral speech. “A-
 “lios aliquot surdos, loquelam docere non agressus sum, sed
 “solummodo ut res scriptas mediocriter intelligerent, suaque
 “sensa scripto quadantenus insinuarent: Qui tempore non lon-
 “go progressus eos fecerint, rerumque plurimarum notitiam ac-
 “quisiverint, multo ultra quam quod putabatur fieri posse a quo-
 “quam in eorum circumstantiis posito; fuerintque plane capaces
 “acquirendi (si plenius exculti) ultio rem cognitionem quæ pos-
 “sit scripto impertiri.” See WALLISII Opera Mathemat. vol. iii.
 p. 696. See also his letter to Mr BEVERLEY, in the Transactions of the Royal Society of London for 1698.—I am obliged

to

to quote from the Latin version, not having the Philosophical Transactions at hand.

After having thus paid the tribute of my sincere respect to the enlightened and benevolent exertions of a celebrated foreigner, I feel myself called on to lay hold of the only opportunity that may occur to me, of rescuing from oblivion the name of a Scottish writer, whose merits have been strangely overlooked both by his contemporaries and by his successors. The person I allude to is GEORGE DALGARNO, who, more than a hundred and thirty years ago, was led by his own sagacity, to adopt, *a priori*, the same general conclusion concerning the education of the dumb, of which the experimental discovery, and the happy application, have, in our times, reflected such merited lustre on the name of SICARD. I mentioned DALGARNO formerly, in a note annexed to the Philosophy of the Human Mind, as the author of a very ingenious tract entitled *Ars Signorum*, from which it appears indisputably that he was the precursor of Bishop WILKINS in his speculations concerning a Real Character, and a Philosophical Language; and it now appears to me equally clear, upon a farther acquaintance with the short fragments which he has left behind him, that, if he did not lead the way to the attempt made by Dr WALLIS to teach the dumb to speak, he had conceived views with respect to the means of instructing them, far more profound and comprehensive than any we meet with in the works of that learned writer, prior to the date of DALGARNO'S publications. On his claims in these two instances I forbear to enlarge at present; but I cannot deny myself the satisfaction of transcribing a few paragraphs, in justification of what I have already stated, with respect to the remarkable coincidence between some of his theoretical deductions,

deductions, and the practical results of the French academician.

“ I conceive there might be successful addresses made to a dumb child even in its cradle, when he begins—*risu cognoscere matrem*; if the mother or nurse had but as nimble a hand, as commonly they have a tongue. For instance, I doubt not but the words, *hand, foot, dog, cat, hat, &c.* written fair, and as often presented to the deaf child’s eye, pointing from the words to the things, and *vice versa*, as the blind child hears them spoken, would be known and remembered as soon by the one as the other. And as I think the eye to be as docile as the ear; so neither see I any reason, but the hand might be made as tractable an organ as the tongue; and as soon brought to form, if not fair, at least legible characters, as the tongue to imitate and echo back articulate sounds.”

“ The difficulties of learning to read, on the common plan, are so great, that one may justly wonder how young ones come to get over them. Now, the deaf child, under his mother’s tuition, passes securely by all these rocks and quicksands. The distinction of letters, their names, their powers, their order, the dividing words into syllables, and of them again making words, to which may be added Tone and Accent; none of these puzzling niceties hinder his progress. It is true, after he has past the discipline of the nursery, and comes to learn grammatically, then he must begin to learn to know letters written, by their figure, number, and order, &c. &c.

The same author elsewhere observes, that “ *the soul can exert her powers, by the ministry of any of the senses: And, therefore, when she is deprived of her principal Secretaries the Eye and the Ear; then she must be contented with the service of her lackeys and scullions, the other senses; which are no less true and faithful to their mistress, than the eye and the ear; but not so quick for dispatch.*”

I shall only add one other sentence, from which my readers will be enabled, without any comment of mine, to perceive with what sagacity and success, this very original thinker, had anticipated some of the most refined experimental conclusions of a more enlightened age.

“ My design is not to give a methodical system of grammatical rules ; but only such general directions, whereby an industrious Tutor may bring his deaf Pupil to the vulgar use and *ὄτι* of a language ; that so he may be the more capable of receiving instruction in the *δι' ὄτι* from the rules of grammar, when his judgment is ripe for that study : Or, more plainly ; I intend to bring the way of teaching a deaf man to read and write, as near as possible, to that of teaching young ones to speak and understand their mother-tongue.”

In prosecution of this general idea, he has treated, in one very short chapter, of *a Deaf Man's Dictionary* ; and in another, of *a Grammar for Deaf Persons* ; both of them containing (under the disadvantages of a style uncommonly pedantic and quaint) a variety of precious hints, from which, if I do not deceive myself, useful practical lights might be derived, not only by such as may undertake the instruction of such pupils as MITCHELL or MASSIEU, but by all who have any concern in the tuition of children during the first stage of their education.

The work from which these quotations are taken, is a very small volume, entitled “ *Didascalocophus, or, The Deaf and Dumb Man's Tutor*, printed at the Theater in Oxford, 1680.” As I had never happened to see the slightest reference made to it by any subsequent writer, I was altogether ignorant of its existence, when a copy of it, purchased upon a London stall, was a few years ago sent to me by a friend, who, amidst a multiplicity of more pressing engagements and pursuits, has never
lost

lost sight of the philosophical studies of his early years. I have been able to learn nothing of the author, but what is contained in the following slight notice, which I transcribe from ANTHONY WOOD. "The reader may be pleased to know, that one GEORGE DALGARNO, a Scot, wrote a book, entitled, *Ars Signorum*, &c. London, 1660. This book, before it went to press, the author communicated to Dr WILKINS, *who from thence taking a hint of greater matter*, carried it on, and brought it up to that which you see extant. This DALGARNO was born at Old Aberdeen, and bred in the University at New Aberdeen; taught a private grammar school with good success for about thirty years together, in the parishes of S. Michael and S. Mary Mag. in Oxford; wrote also, *Didascalocophus*, or, the Deaf and Dumb Man's Tutor; and dying of a fever, on the 28th of August 1687, aged sixty or more, was buried in the north body of the church of S. Mary Magdalen." (Ath. Oxon. vol. ii. p. 506-7.)

The obscurity in which DALGARNO lived, and the complete oblivion into which his name has fallen, are not a little wonderful, when we consider that he mentions among the number of his friends Dr SETH WARD, Bishop of Sarum; Dr JOHN WILKINS, Bishop of Chester; and Dr JOHN WALLIS, Professor of Astronomy at Oxford. It is still more wonderful, that no notice of him is taken in the works either of WILKINS or of WALLIS, both of whom must have derived some very important aids from his speculations.

This unfairness on the part of WILKINS, has not escaped the animadversion of one of his own biographers. "In the prefatory epistle (he observes) to the Essay towards a Real Character, Dr WILKINS mentions several persons who assisted him in this work, particularly WILLOUGHBY, RAY, and Dr WILLIAM LLOYD and others; but it is remarkable, that he does not men-

tion DALGARNO, and the more, because Dr WILKINS's own name is printed in the margin of King CHARLES II.'s letter prefixed to DALGARNO's book, as one of those who informed his Majesty of Dalgarno's design, and approved it, as a thing that might be of singular use to facilitate an intercourse between people of different languages; which prevailed with his Majesty to grant his said letters of recommendation to so many of his subjects, especially of the Clergy, as were sensible of the defectuousness of art in this particular." *Biog. Britan.* Art. WILKINS*.

That DALGARNO's suggestions with respect to the Education of the Dumb, were not altogether useless to Dr WALLIS, will, I think, be readily admitted by those who take the trouble to compare his letter to Mr BEVERLY (published eighteen years after DALGARNO's treatise) with his *Tractatus de Loquela*, published in 1653. In this letter some valuable remarks are to be found on the method of leading the dumb to the signification of words; and yet, the name of DALGARNO is not once mentioned to his correspondent.

If some of the details and digressions in this note should be censured, as foreign to the principal design of the foregoing memoir, I can only plead in excuse, my anxiety to do justice, even

* In GRAINGER's Biographical History of England, mention is made of a still earlier publication than the *Ars Signorum*, entitled, "The Universal Character, by which all Nations in the World may understand one another's conceptions, reading out of one common Writing their own Tongue. By CAVE BECK, Rector of St Helen's, in Ipswich, 1657." This book I have never seen.

The name of Dalgarno (or Dalgarus, as it has been sometimes written) is not altogether unknown on the Continent. His *Ars Signorum* is alluded to by LEIBNITZ on various occasions, and also by FONTENELLE in the *Eloge* of LEIBNITZ. His ideas with respect to the education of the Dumb, do not seem to have attracted any notice whatever. The truth is, they were much too refined and enlightened to be duly appreciated at the period when he wrote.

even at the distance of a century, to the memory of an ingenious man, neglected by his contemporaries, and already in danger of being totally forgotten by posterity. To those whose curiosity may lead them to study his book, the originality of his conceptions, and the obvious application of which some of his principles admit to the peculiarities of the case now before us, will of themselves suggest a sufficient apology.

APPENDIX,

APPENDIX, containing some additional Commu-
nications relative to JAMES MITCHELL.

NO. I.

Extract of a Letter from Dr GORDON to Mr STEWART.

Edinburgh, March 30. 1812.

. A few days after you returned to the country, I wrote to a friend of mine near Forres, putting several queries respecting MITCHELL, which I requested him to get answered by Miss MITCHELL if possible. I wished in particular to be satisfied as to the lad's behaviour on his father's death, as what I had myself seen of his conduct at the funeral, had led me to differ from Mr GLENNIE's information on this point. From Miss MITCHELL *directly*, I have obtained the following curious particulars :

At his sister's request, MITCHELL was allowed to touch his father's body. As soon as he felt it, he shrunk away. This was the first time he had ever touched a dead *human* body. He has been seen amusing himself with a dead *fowl*; placing it repeatedly on its legs, and laughing when it fell.

He has not shewn any signs of grief in consequence of his father's death.

When

When a tailor was brought to make a suit of mournings for him, the boy took him into the apartment where his father had died, stretched his own head and neck backwards, pointed to the bed, and then conducted him to the church-yard, to the grave in which his father had been interred.

Being lately very ill, he was put into the same bed where his father had died. He would not lie a moment in it, but became quite peaceable when removed to another.

On one occasion, shortly after his father's death, discovering that his mother was unwell, and in bed, he was observed to weep.

Three months after the death of his father, a clergyman being in the house, on a Sunday evening, he pointed to his father's Bible, and then made a sign that the family should kneel.

Lately, his mother being from home, his sister allayed the anxiety he shewed for her return, by laying his head gently down on a pillow, once for each night his mother was still to be away; implying, that he would sleep so many times before her return*.

Whilst

* It would appear that this is the sign which Miss MITCHELL usually employs on similar occasions; and the ready interpretation of it by her brother, implies, on his part, no inconsiderable a share of shrewdness and of reflection. I copy the following parallel incident from a paper of Mr WARDROP's now before me. (D. S.)

“ When his new clothes were all made, I solicited his father not to allow him to put them on, until I was present. It was signified to him accordingly, that in two days he should have them. This was done by shutting his eyes, and bending down his head twice, in order to intimate to him, that he must first have two sleeps.”

Whilst he was last in London, he happened to be in the house of a friend of his father's, who was in the habit of smoking; and a pipe being given him, he smoked it, and seemed much delighted. Some little time ago, a gentleman came on a visit to Ardclach, who was also in the habit of smoking, and having tobacco, wished for a pipe. Miss MITCHELL gave the boy a halfpenny, and permitted him to smell the tobacco. He understood her signs; went out to a shoemaker's house in the neighbourhood, where pipes were to be had, and returned with one only in his hand. They suspected that he had another about him, and giving him to understand as much, he at last unbuttoned his waistcoat, and, laughing heartily, brought out the second pipe. The Sunday after this occurrence, when his sister gave him a halfpenny, as usual, in church, to put into the poors'-box, he immediately placed the halfpenny between his teeth, like a pipe, and laughed; but his sister checking him, he dropped it into the box.

He is still fond of the trick of locking people into the house or the stable. The patron of the parish, Mr DUNBAR BRODIE, (a gentleman who, I have reason to believe, has exceeded all others in acts of substantial kindness to the MITCHELL family), happening lately to visit Ardclach, young MITCHELL contrived to make him a prisoner in this manner for a few minutes, laughing and jumping about all the while. On this particular occasion, it was noticed, that he applied his eye to an aperture in the door of the stable, as if to observe the motions of the person within. But although my friend writes me, that the other day upon holding out his hand to MITCHELL, the boy took hold of it; it cannot be conceived, that his sight should have suddenly so much improved, as to enable him to see any object in a dark stable, through a hole in the door, without the improvement being extremely obvious in other instances.

NO. II.

A Series of Questions respecting JAMES MITCHELL, proposed by Mr GLENNIE, and answered by Miss JANE MITCHELL.*

Q. 1. Did Mr WARDROP operate on the eyes only? or on the ears also?

A. Mr WARDROP operated only on the right eye.

Q. 2. Were the drums of the ears pierced during the first or the second visit to London?

A. The drums of the ears were pierced during the first visit; the one by Mr ASTLEY COOPER, the other by the late Mr SAUNDERS of the London Dispensary.

Q. 3. Was it the case, that a musical instrument was playing in the room when his ears were pierced? and did he attend to it?

A. Some days after his ears were pierced, in a friend's house, he applied his ear to a violin, and the sound seemed to afford him pleasure †.

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* Although some of the information contained in this paper has been already anticipated in the communications of Dr GORDON and of Mr WARDROP, I have thought it proper to insert it here at full length; on account not only of the new light which it throws on various very interesting and important points, but of the high authority which it derives from Miss MITCHELL's name. (D. S.)

† The following particulars are mentioned by Mr WARDROP with respect to the state of MITCHELL's deafness at the time when he saw him in London. (D. S.)

..... "When a ring of keys was given to him, he seized them with great avidity, and tried each separately, by suspending it loosely between two of his fingers,

Q. 4. Does he shew a strong desire to examine *all* objects by feeling?

A. He does: small objects he applies to his teeth, and feels with the tip of his tongue: larger objects he feels with his fingers carefully.

Q. 5. Is he much gratified with a *new* object?

A. Some objects do not seem to attract his attention; others do; and, where there is any mechanism, he endeavours, by handling them, to find it out: he discovers a particular fondness for locks and keys.

Q. 6. Does he discover any preference to the handling of smooth, rough, or pointed things?

A. If he does any, it is to smooth objects; when he gets a bit of rough wood, he endeavours to smooth it with his teeth, or causes the boy who attends him to smooth it with a knife.

Q. 7. Is he fond of bodily exertion?

A. He is extremely fond of running, walking, and riding.

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Q. 8.

fingers, so as to allow it to vibrate freely; and after tingling all of them amongst his teeth in this manner, he generally selected one from the others, the sound of which seemed to please him most. This, indeed, was one of his most favourite amusements, and it was surprising how long it would arrest his attention, and with what eagerness he would on all occasions renew it. Mr BROUGHAM, having observed this circumstance, brought to him a musical snuff-box (a French trinket, containing a small musical instrument, which played airs by means of a spring), and placed it between his teeth. This seemed not only to excite his wonder, but to afford him exquisite delight, and his father and sister, who were present, remarked, that they had never seen him so much interested on any former occasion. Whilst the instrument continued to play, he kept it closely between his teeth, and even when the notes were ended, he continued to hold the box to his mouth, and to examine it minutely with his fingers, expressing by his gestures and by his countenance great curiosity."

Q. 8. Does he discover any sense of danger?

A. He discovers a sense of danger from fire, water, and sharp instruments.

Q. 9. Is it necessary to follow him, to keep him from harm?

A. It is only necessary to follow him, to prevent him from wandering.

Q. 10. Has he the knowledge of the use of things?

A. He knows the use of all common things, and is pleased when the use of any thing with which he is not acquainted is communicated to him.

Q. 11. Has he learned to do any kind of work?

A. He has not; further than to assist any of the farm-servants, for whom he may have conceived an attachment, in any work in which they may be engaged; particularly in cleaning the stable. He has endeavoured to repair breaches in the farm houses; and has attempted to build small houses with turf, leaving small openings resembling windows. Means have been used to teach him to make baskets; but he wants application to finish any thing.

Q. 12. Does he go from home? and is he fond of doing so?

A. His greatest pleasure seems to consist in wandering from home; but he always returns to his meals.

Q. 13. Is he uneasy when separated from his friends or attendants?

A. He discovers much uneasiness when separated from his friends, but does not, *now*, discover uneasiness when he changes his attendants, though he did, very early in life.

Q. 14. Does he love to associate with boys, and engage in play?

A. He never associates with boys, nor discovers any inclination to join in their amusements; but sometimes wishes the
boy

boy who attends him, to assist him in floating objects on the water, &c. He is, however, fond of young children, and takes them up in his arms.

Q. 15. Has he any uneasy feeling of his unfortunate situation?

A. He is sensible that his sight is imperfect, but does not discover any uneasy feeling of his situation.

Q. 16. Is he sensible of loud sounds? or of music?

A. Of *very* loud sounds he is, and seems uneasy at the time.

Q. 17. Does he apprehend the distinction of ranks in society?

A. He prefers persons who are well dressed, to those who are not; and would not willingly eat any food in the kitchen.

Q. 18. Has he the sense of ridicule?

A. In some degree he no doubt has; for instance, he takes pleasure in locking the door on people, and confining them; and, if treated in the same manner himself, seems amused, but if too much teased, is irritated.

Q. 19. Has he any devotional feelings?

A. He cannot possibly have any; but sits quietly in church, and kneels at family-prayers.

Q. 20. Has he the sense of right and wrong?

A. He undoubtedly has; and if gentle means are used to make him sensible of his having done wrong, he shews sorrow; but if harshly treated, is irritated.

NO. III.

THE foregoing sheets were not only printed but cast off before the following letter reached me. I subjoin it, without any comment, to the papers on the same subject which I have already laid before the Society; and have only to return my thanks to the Author, for the trouble he has so judiciously taken in recording a variety of minute details, which, to a superficial observer, may appear of trifling importance, but which will be considered in a very different light by all who are able to perceive, how strongly they bear on some of the most interesting questions which relate to the characteristic endowments of the Human Mind. Solitary as MITCHELL is in the midst of society, and confined, in his intercourse with the material world, within the narrowest conceivable limits; what a contrast does he exhibit,—in those rudiments of a rational and improvable nature, which we may trace even in his childish occupations and pastimes; and more particularly, in that stock of knowledge, scanty as it is, which he has been prompted to acquire by the impulse of his own spontaneous curiosity,—to the most sagacious of the lower animals, though surrounded with all the arts of civilized Man, and in the fullest possession of all the powers of external perception!

Letter

*Letter from Dr GORDON to Mr STEWART.**Edinburgh, October 26. 1812.*

My dear Sir,

During my residence in Morayshire, in August last, I did not fail to avail myself of my vicinity to Ardcloch, to visit the MITCHELL family. I have now to communicate to you, according to promise, the additional particulars respecting the subject of your memoir, which this visit has enabled me to collect. Some of these, you will easily perceive, are the result of my own observation on the boy himself; others were obtained from conversations with his eldest sister, whom I considered myself extremely fortunate in finding at home.

Previously to my visit, report had given me reason to expect, that I should find young MITCHELL's vision considerably improved; and I had not been long in his company, before I received very satisfactory proof that this was the case. Accordingly, I was led to examine his eyes with attention. Twelve months ago, as I have stated in the supplement to Professor GLENNIE's Account, one could perceive fragments of the lens very white and opaque, behind one-half of the pupil of each eye; and through the other half, a slighter opacity, or a sort of greyish appearance, in the parts situated farther back. The only change which I could discover, sufficient to account for the improvement which has taken place in his vision, is a diminution in this *slighter opacity* in both eyes. At present, there is a very white fragment of the lens, behind the upper half of the pupil of the right eye; and behind the lower half, the humours appear almost perfectly black. In the left eye, there is a dusky-white opacity behind the lower and inner half of the pupil; and behind the upper
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and outer half, the humours are of a dark grey. The pupils contract and dilate as usual, on varying the quantity of light.

It is not easy to determine the exact degree of vision which he now enjoys. He sees those bodies only which have considerable brightness, or dark-coloured bodies placed on a bright ground. Consequently, of the various objects which usually surround him, he sees such only as are not very minute, and are placed within a short distance of his eyes. He could distinguish a crown-piece at the distance of two or three feet, and a person's face at the distance of six. But it seems obvious, that he does not perceive distinctly the *limits* of any object, however bright. For as soon as, guided by his own obscure vision, he has reached any thing with his hands, he no longer regards it with his eyes; but, as if he were yet totally blind, examines it solely with his fingers, tongue, lips, and nose.

That he can now distinguish differences in the kinds of light or in colours, seems very evident from an amusement in which, his sister told me, he sometimes indulges,—matching bodies of the same colour together. One day, for example, having a bunch of the flowers of wild mustard in his hand, he was observed to approach an officer who was near him, and, with a smile, placed the flowers in contact with the yellow part of his epaulette. Frequently, too, he is seen gathering in the fields a number of flowers of the same kind; the blue-bottle, for example, or the corn-poppy, or the marigold. It appears, however, that it is only the brighter colours he is capable of distinguishing; and of these *red* seems to be his favourite. A red object attracts his notice more, and he looks at it longer, than any other. Of the female parishioners who pass the manse on their road to church on Sunday, he is most apt to follow those who are dressed in red cloaks. Miss MIT-

CHELL

CHELL is of opinion, that he rather dislikes darkness ; for she has observed, that in moving from one part of the house to another after night has come on, his step is hurried ; and that he seems happy in reaching an apartment where there is a candle or a fire.

I observed, that he judges of the *direction* of a body by sight, with invariable accuracy ; but when an object whose real magnitude is not known to him, is placed before his eyes, he does not seem capable of estimating its *distance*, for the first time, with any degree of correctness. When I held a silver snuff-box about two feet from his face, he put out his hand exactly in the *direction* of the box, but moved it forwards very gradually until it came in contact with it. These circumstances are just what we should before-hand have expected to find ; and such also, I imagine, as may be remarked of all persons who are nearly blind, from a similar cause. The perception of the *direction* of bodies, which obviously depends on the particular part of the retina which is affected by the rays they emit, may be obtained equally (if the bodies be seen at all) from the weakest as from the most perfect vision. But MITCHELL'S vision is too obscure to enable him to perceive those *minute differences in the colour and intensity of light*, by which persons having perfect sight, judge of the *relative distance* of luminous bodies.

On the whole, it appears obvious, that his sight, although yet far too imperfect for any attempt to address him in a *visible language*, is considerably improved within these last twelve months. Did the boy's dispositions admit of it, I should now be inclined to recommend still more earnestly than before, that another attempt should be made, to remove the cataracts from his eyes, and I am much less disposed than formerly to fear, that there is any radical imperfection in the optic nerves.

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Here it may not be improper to mention, that his sister is convinced, that he sees some objects better by moon-light than during the day; a circumstance which seems to shew, that the opacity in his eyes is, as in other cases of cataract, merely local; so that when the pupil is much dilated, some rays of light reach the retina, through those more transparent parts of the humours which are farther distant from the axis of vision than the portion that is opaque.

His powers of *Hearing* remain as imperfect as ever. He still continues the practice of striking hard bodies against his teeth; but on further reflection, I think it not unlikely, that he may have another object in view in this experiment, besides that of procuring a sensation of sound. It is not at all improbable, that he discovers differences in the hardness of bodies in this manner. For there is a very distinct sensation felt towards the roots of the teeth themselves, when they are struck with a hard substance, resulting probably from an affection of the nerves of the membrane lining their inner cavity, and this sensation is different according to the hardness of the body. I have little doubt, that he could by this kind of feeling alone, very easily discover that lead was softer than steel, and steel harder than ivory; although all these substances would feel equally hard to his proper organs of touch. But even supposing that he does not avail himself of this sensation in the teeth themselves, it is probable that he strikes bodies against these organs, not so much to try whether they will cause sound at all, as to observe what *kind* of sounds they will emit; from which he may infer various other properties, which experience has taught him, are invariably connected with the particular sounds emitted.

His manner of examining any object that is new to him, is precisely the same now that it was four years ago, when I first saw him. When it is put into his hand, he runs it over with
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the points of his fingers ; then applies it to his mouth, and insinuates his tongue into all its inequalities, thus using it as an organ of Touch as well as Taste ; and, lastly, if it is a body that admits of it, he rattles it between his teeth. All this is done with singular rapidity. In fact, he loses but little time, in discovering, by the actual use of his organs of touch, taste, and smell, those qualities of bodies which we are content to infer from their visible appearance alone.

His sense of smell is unquestionably extremely acute. But I have not been able to learn any fact which could lead me to believe, that he could, in a room at least, discover a person by this sense alone, at the distance of twelve feet. It has been said, that he could follow the footsteps of another person for two miles, guided merely by smelling. But his sister assures me, that there is no foundation for this report. As to a power of determining the *direction* of an object, by some *distinct quality* in its odour, like that quality in sound by which we discover the direction of a sounding body, I could not perceive that he enjoyed any such power more than other persons. Indeed it is not likely that his faculties should differ in kind from our own, however much they may in number and degree*.

Since his sight has begun to improve, his excursions have become bolder and more extensive. He has sometimes wandered upwards

* Hic Adolescens, annum nunc agens xviii, et optimâ semper usus valetudine, vegetus est, et admodum robustus : quin et solitâ ætate pubescere visus est, partibus genitalibus ut in viris se habentibus ; neque dubitari potest quin brevi futurus sit *επιπρωγος*, labiis et mento densâ jam inumbratis lanugine. Curiosè autem percontanti famuli et amici (masculini scilicet sexûs, quos solos de his rebus interrogare fas erat) omnes mihi testabantur nihil se observâsse, unde colligerent illum Veneris stimulum unquam sensisse, vel differentiæ sexûs notionem habuisse.

upwards of three miles from home. In all these expeditions, he proceeds in a great measure without a guide. But a boy is appointed to follow him, and keep him constantly in view ; and MITCHELL has the good sense, when he perceives any thing which he imagines to be a serious obstacle in his way, to wait until his little follower comes up to his assistance.

Lately, on the road near the manse, he met a person, riding on a horse which had been bought a few weeks before from his mother ; and on coming up to the animal, and feeling it, he seemed instantly to recognise it. The person immediately dismounted, in order to see how MITCHELL would behave ; and he was much amused to find, that he led the horse to his mother's stable, took off his saddle and bridle, put corn before him, and then withdrew, locking the door, and putting the key in his pocket.

When he wishes to communicate his ideas to any one near him, he uses *natural signs* ; and it is curious to observe, that most of these signs are addressed to the *sight* of those with whom he converses. This fact, it appears to me, shews very clearly, that he is aware that the powers of vision enjoyed by others are superior to his own ; and hence it is not unreasonable to hope, that his reflecting on his inferiority in this respect, when his reason has become more matured, may be the means of inducing him to submit, more placidly than he has hitherto done, to any endeavours for the improvement of his sight.

The following are a few examples of his signs. As soon as I began to examine his eyes, opposite to a window, he turned towards his sister, and stretched out his arm to its full extent laterally from his body. This, his sister informed me, is his usual sign for *London*. It is obviously the natural expression of distance ; and there is no need of pointing out the association which must have led him to use it on this occasion.

When

When he would express that he has been on horseback, he raises his foot, and brings the fingers of each hand together under the sole, in imitation of a stirrup. He places his hand on his mouth to signify his wish for food ; and when he would go to bed, he inclines his head sideways, as if to lay it on a pillow. When I arrived at Ardclach, young MITCHELL was not at home; he had wandered to the shoemaker's, several hundred yards distant, where he was sitting in anxious expectation of a pair of new shoes. He was brought to the manse ; but after he had remained with us contentedly in the dining-room for about half-an-hour, he shewed an anxiety to get away ; and as he moved towards the door, he made use of a sign, from which no one could fail to discover whither he was going. It was, an exact imitation with his arms, of a shoemaker's motion when he pulls his thread.

All the signs employed by others in order to convey ideas to him, are addressed to his organs of *touch*. The most important, certainly, of these signs, are those which his sister has invented, to express her approbation or disapprobation, her assent or dissent. Miss MITCHELL's explanation of them is extremely satisfactory. Her brother has always been particularly attached to her, and she has always had most influence over him. He courts her good opinion. When she would signify to him her *highest approbation* of his conduct, she pats him much and cordially on the head, back, hand, or any other part of the body. This expression more sparingly and less fervently bestowed, signifies *simple assent* ; and she has only to refuse him these signs of her approbation entirely, and to repel him gently, to convey to him in the most effectual manner the notice of her *displeasure*.

When I suggested to Miss MITCHELL that it would be a highly interesting task, though doubtless a difficult and tedious

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one, to teach her brother the meaning of *written words*, and mentioned briefly the outlines of a plan for that purpose; she expressed the utmost willingness to undertake any attempt of this kind, but anticipated the chief obstacle to the design from his want of application. Still I cannot help thinking, that this obstacle would probably diminish, as soon as he felt the magic power of a few words. Perhaps a trial might be made according to some such plan as the following.

First, young MITCHELL might be provided with a *horn-book*, on which the letters of the alphabet have been cut *in relief*. His sister might then begin, by tempting him with the prospect of some article of luxury, a piece of sugar for example, or a toy; but before gratifying him with the possession of it, she might take hold of his fore-finger, and conduct the point of it over all the letters composing the name of the article. This being frequently repeated, I have little doubt that he would soon point to the same letters when the same object was held in prospect; and at last, use the sign to procure the luxury. Were one step of this kind gained, it is not unreasonable to expect, that he might in time be made to understand the meaning of every word in our language, whether expressive of one or many ideas. Such words are of course excepted, as express ideas which he cannot possibly have experienced, from the imperfections of his sight and hearing. An advantage would attend the use of the *horn-book* proposed, that if the letters were painted black, MITCHELL might communicate by means of it with persons at a considerable distance. Supposing him to have acquired a language of this kind, two others, if necessary, might afterwards be connected with it. The first would consist in tracing the letters of words on the palm of his hand, with the point of one's finger; and the

the second, of the common speech on the fingers. But how great an acquisition would the principal language alone be, without any such auxiliaries!

Several circumstances occurred, during my visit, which shew, how perfectly susceptible he is of pleasure from joking, or playing with him, or from any thing ludicrous in the ideas communicated to him. Twice or thrice when his sister perceived that he was crossing the room to go away, she stepped to the door unperceived by him, to prevent his escape. When he found her there before him, he stepped back smiling, and seemed to take this sort of teasing in perfect good humour. I had given him my whip, with which he seemed pleased; and once or twice his sister took him by surprise, and pulled it smartly out of his hand. He immediately shewed by his smiles that he knew who had robbed him; and quickly catching his sister, he endeavoured to wrest the whip from her. The joke obviously amused him very much; but Miss MITCHELL assured me, that it would have given him offence to have repeated it more frequently. An uncommonly large Newfoundland dog, belonging to a gentleman who accompanied me, had got into the room; and nothing could be more expressive of surprise than young MITCHELL's countenance, when he first felt this animal. His sister observing this, immediately, with great quickness, took hold of his arm, and stretched it above his head, a sign which, it seems, he uses to denote *mounting a horse*. He instantly understood her meaning, and laughing, made a motion as if he would bestride the dog.

New clothes are still among MITCHELL's greatest sources of delight. After his measure has been taken, it would seem that every hour is full of anxiety until the new suit is in his possession.

possession. Nothing else appears to occupy his mind. He literally persecutes the tailor or the shoemaker, until his shoes or his coat is finished. He is their guest morning, noon, and night, until the last stitch is drawn.

Before leaving Ardelach, I took an opportunity of conversing very fully with Miss MITCHELL relative to her brother's conduct at the period of his father's death. Her answers to my inquiries on this point, corresponded exactly with the information she was so kind as communicate to me through my friend Mr LAUDER DICK of Relugas, in March last, and which I transmitted to you immediately on receiving it. She told me, that when her brother was permitted, by her direction, to touch his father's dead body, he shrunk from it with surprise, but without expressing the slightest signs of sorrow. She assures me also, that he felt the body after it was placed in the coffin, but without betraying any emotions of grief. On the evening, however, after her father's funeral, she herself saw him go down to the grave, and pat the turf with *both* his hands; but whether he did this from affection, or intended it merely as an imitation of *beating down the turf*, she feels unable to decide, as she was not near enough to him to discern the expression of his countenance. For several days afterwards, it would appear that he returned repeatedly to the grave; but gradually discontinued his visits. It is worthy of remark, however, that he has regularly attended every funeral that has since taken place in the same church-yard. The report, therefore, which I have stated at the conclusion of the supplement to Professor GLENNIE'S Account, of his having shed tears over his father's grave, seems entirely without foundation. Miss MITCHELL authorises me to say, that neither on this nor on any other occasion, has she *herself* seen her brother shew any unequivocal marks of sorrow for his father's death. Yet her
friend,

friend, the Reverend Mr CAMPBELL of Ardersier, lately informed her, that he saw her brother standing in the porch shedding tears, immediately after quitting the apartment in which his father's body was lying, previous to the funeral.

On the whole, however, I have not been able to discover the slightest reason for altering the opinion I have always entertained respecting the state of young MITCHELL's feelings on the day of the funeral. It was my strong conviction of the truth of this opinion, and thinking that Professor GLENNIE might have been furnished with the materials of his Account from some one who had not enjoyed the same opportunity of judging as myself, that led me, in the supplement to that Account, to doubt in some degree the accuracy of his information on this point. I have since found, however, that the whole of Professor GLENNIE's memoir was communicated by my friend Mr MACFARLANE, who was present, as well as myself, on that melancholy occasion. I would now observe, therefore, that though I am sorry to differ in opinion from a gentleman who has written so able a detail of some other parts of MITCHELL's history, my perfect knowledge of his candour and liberality embolden me to say, that I think he is mistaken in this particular; and that he has interpreted into expressions of grief in young MITCHELL, what were merely expressions of curiosity. On this subject I have communicated with my friends Mr LAUDER DICK of Relugas, Mr SMYTH of Earlsmill, and the medical attendant of the family, Mr STRAITH, surgeon at Forres,—gentlemen who also were present at the funeral, and who are more familiar even than I am with young MITCHELL's countenance and expression; and I find, that their opinion coincides exactly with mine. His motions at the coffin were equally visible to us all. But we did not attribute his placing his arms around it, to any emotion of sorrow, of which there appeared to us not the slightest

trace in his countenance, but to the same motive that led him the very next moment, to trip lightly towards us, and smilingly feel our clothes all over—the pleasure he experienced in the examination of objects that were new to him. My friend Mr LAUDER DICK, who has accompanied me in all my visits to Ardclach, and whose interest in the family, and kindness towards them, have been equally great, has favoured me with a few remarks, in a letter on this subject, which appear to me so just, that I shall take the liberty to quote them. “From my observations,” he writes, “made at the time, with all the attention which an extreme interest in the boy could excite, my opinion certainly is, that he was occupied with the coffin merely as being a body of a shape and surface different from any thing he had before met with ; and that he betrayed no emotions of grief. When the procession moved onwards, all his gestures seemed more those of a playful boy in good spirits, than those of an afflicted youth, conscious of the awful change which had taken place upon his parent. As it is certain that he had never felt a dead body, nor had any opportunity of learning the object of burial before ; it appears to me, that we cannot imagine him to have experienced any emotion of grief at his father’s funeral, without also supposing him to have had an innate idea of death.”

I am, my dear Sir, with great regard, yours truly,

JOHN GORDON.

Postscript.

Before sending you this letter, I transmitted a copy of it to Miss MITCHELL, for her perusal and correction ; and I have much pleasure in adding the following extracts from her very obliging and satisfactory reply.

“ Agreeably

“ Agreeably to your request, I have read your letter to Professor STEWART with as much attention as the short time it has been in my possession would admit of; and I certainly think you have stated those facts I informed you of, respecting my brother, most correctly.

“ My brother seems to be very well pleased with his change of residence *, and goes on much in the same way he did at Ardclach; that is to say, wandering for several miles round the small town we live in, or amusing himself by visiting the different carpenters’ or other tradesmen’s shops within his reach, and handling their implements, or trying to discover what they are engaged about. He has not yet discovered any anxiety to return to Ardclach, and is, I think, quite as happy as when there.”

* Mrs MITCHELL and her family have within these few months left Ardclach to reside at Nairn.

NO. IV.

WHILE employed in revising this concluding sheet, I had the pleasure of receiving the following letter from my friend Sir JAMES MACKINTOSH. It is unnecessary for me to mention the satisfaction I feel in attracting that notice to the Subject of my Memoir which his name cannot fail to ensure.

Letter from Sir JAMES MACKINTOSH to Mr STEWART.

Edinburgh, 5th November 1812.

My dear Sir,

In consequence of our conversation at Kinneil in August, I called on Mrs MITCHELL after my arrival in Nairnshire, and on the 9th of October I had an interview with JAMES MITCHELL, and his sister Miss MITCHELL, which lasted for several hours. I directed my inquiries to every point which seemed important, in the corporeal or mental state of this unfortunately interesting young man.

The result, however, is little more than a needless corroboration of the accounts which you have already received; especially those from Dr GORDON, who seems to have conducted his observations with much philosophical discernment and accuracy.

During the vacancy in his father's parish, the parishioners assembled on Sunday for public worship and mutual instruction, and one of the elders prayed with a loud and shrill voice, which was observed to give great uneasiness to MIT-

CHELL.

CHÉLL. This occurred several times, so that there appears no reason to consider it as an accidental coincidence.

Though his ordinary conduct be decorous, it seems to be influenced by habit and instruction rather than by feelings of delicacy. When the females of his family are undressing, he has been observed to turn aside. There are no males in the house. But in an opportunity which has lately occurred, he has been thought to shew a similar disposition in the case of males.

I have seldom seen an imperfection of the senses attended by so little an air of defect in the countenance. Singular as it may seem, I should even venture to call his features intelligent. He handled every part of the room in which we sat, with indications of an inquisitive mind.

His sister is a young woman of most pleasing appearance and manners, distinguished by a very uncommon degree of modesty, caution, and precision, in her accounts of him ; and probably one of the most intelligent, as well as kindest companions, that ever guided a being doomed to such unusual, if not unexampled privations.

You will not think me fantastic for adding, that the habitual exercise of ingenious benevolence seems to me to have left its traces on her countenance, and to have bestowed on her naturally agreeable features, an expression more delightful than beauty. Her aversion from exaggeration, and her singular superiority to the pleasure of inspiring wonder, make it important to the purposes of Philosophy as well as of Humanity, that she should continue to attend her brother. Separation from her would indeed be an irreparable calamity to this unfortunate youth. By her own unaided ingenuity, she has conquered the obstacles which seemed for ever to preclude all intercourse between him and other minds ; and what is still more important, by the firm and gentle exertion of her well-earned

ed ascendant over him, she spares him much of the pain which he must otherwise have suffered from the occasional violences of a temper irritated by a fruitless struggle to give utterance to his thoughts and wishes; disturbed still farther by the vehemence of those gestures which he employs to supply the deficiency of his signs, and released from that restraint on anger which we experience when we see and hear its excesses disapproved by our fellow-creatures.

I am, my dear Sir, with the truest esteem,

Yours most faithfully,

J. MACKINTOSH.

II. *On the Vertical Position and Convolutions of certain Strata, and their relation with Granite.* By SIR JAMES HALL, Bart. Pr. R. S. ED. & F. R. S. LOND.

[Read February 3. 1812.]

THE tract of country extending across this island, from the sea-coast of Galloway to that of Berwickshire, consists, with little interruption, of that species of rock, which has of late been most generally known by the German name of *Grauwacke*. But as this rock does not differ essentially from what in Cornwall is called *Killas*, I am disposed, in concurrence with several members of this Society, in particular with Mr ALLAN, who has of late been in Cornwall, and has paid particular attention to that subject, to reject the uncouth term *Grauwacke*, and adopt that of *Killas*, as being more congenial to our language.

The continuation is so unbroken, that I believe it would be practicable to walk from one sea to the other, without treading upon any rock but *killas*; and as its character is identical throughout, we seem to be authorised, in every view of mineralogy, to look upon the whole as one with respect to origin
and

and history ; so that observations made at any one part of the range, may, with confidence, be applied to the rest.

There are but few exceptions to this general rule, which have come to my knowledge. At the Rae Quarry in Peeblesshire, near the Crook Inn, a Limestone containing shells occurs, interstratified with killas ; and in the stewartry of Kirkcudbright, there are three Granitic districts in the midst of this rock, whose relations with it exhibit some interesting facts, which will occupy the second part of this paper. A granite mass also occurs within the range of this same mass of killas, at Priestlaw on the Water of Fasnet, in the mountainous part of East Lothian.

The killas consists everywhere of an assemblage of strata of various thickness, from several yards to the minutest leaf of slate. It is in general of a dark-blue colour, and, when examined, is found to consist of a congeries of fragments, which bear the most undoubted proofs of having been deposited in an horizontal position. The strata lie parallel to each other, but are everywhere far from being horizontal, their prevailing and best known position being vertical, or nearly so. They are often bent, however, at various angles, frequently very acute, and sometimes with the strata nearly doubled upon themselves. In all the inland part of this range, the rock appears so partially, shewing itself only in river-courses, or in quarries, that it is difficult to obtain any correct information as to the position of the strata, which frequently exhibit great seeming irregularity, and which cannot be described, without making use of language which at first sight bears an appearance of contradiction in terms. Thus it not unfrequently happens, that one set shews itself in a position, at the surface, nearly erect, but having a decided dip to the east ; and that in the immediate neighbourhood,
another

another set occurs in a position similar to the first, but having a no less decided dip to the west.

On the shore of the sea, however, where these rocks are bare, and exposed in such a manner, that our view can embrace at once a considerable extent of the mass, the general structure becomes apparent, and we are enabled to give a rational account of these seeming anomalies.

This opportunity of observation, occurs with peculiar advantage on the coast of Berwickshire, where the lofty cliffs which extend from Fast Castle eastward to Gun's Green near Eyemouth, present to view a cross section of these strata, by which their position is seen to possess much more method and regularity than the inland rocks would have led us to expect. The strata here exhibit a succession of regular bendings, and powerful undulations, reaching from top to bottom of the cliffs, two or three hundred feet in height. These are occasionally interrupted, as might be expected, by the irregularities of the coast, by shifts and dislocations of the beds, and sometimes, as happens at St Abb's Head, by the intervention of whinstone; or occasionally of porphyry.

Notwithstanding these interruptions, I reckoned, (in an excursion to that coast, made last summer with my son Lieutenant BASIL HALL of this Society), sixteen distinct bendings, in the course of about six miles, each of the largest size, and reaching from top to bottom of the cliffs, their curvature being alternately concave and convex upwards. Plates I, II and III. are from drawings made on the spot. Fig. 1. Plate I. shews a general view, taken at sea, at some distance off the point of Fast Castle, which appears upon the right hand. Plate II. shews a near view of part of the same scene, representing a spot called the Brander Cove, in which one of these convolutions, concave downwards, is conspicuously seen. The rock

upon the right, on which Fast Castle stands in fig. 1. Plate I., exhibits also one of the convolutions concave downwards, and similar to that of the Brander Cove, seen on the left. Plate III. represents another rock of the same coast, at a place called Whapness, near Gun's Green, in the neighbourhood of Eyemouth. Here there are four bendings; two convex upwards, and two convex downwards, lying close to each other; and fig. 2. Plate I., represents a rock in the same neighbourhood, in which a variety of convolutions are distinctly seen.

We have thus a specimen of that part of the coast which consists of killas; the whole being a succession of similar bendings, alternately concave and convex upwards; and the curvature of the mass is, in general, (as Mr PLAYFAIR has well observed, *Illustrations*, art. 204.) simple; that is to say, these bendings are performed in one direction only, and round axes that seem to lie horizontally, and parallel to each other.

These strata, in common with all those of killas, seem to have been originally deposited in a position nearly horizontal, and many of the particular beds represented in these sketches, possess that peculiar undulation on their surface which we meet with on a sandy beach, when the tide has left it, and which affords the most unequivocal indication of aqueous deposition. There is reason to believe also, that the strata, constituting these convolutions, though now detached from each other, have at one period lain in continuity, and horizontally; that by the exertions of some powerful mechanical force, they have been compelled to assume their present contorted shape; that their continuity still exists below, and would be seen, could we penetrate into the mass under the level of the sea; the interruption of their continuity upwards, having arisen from a removal of part of the rock, by some of those revolutions which
have

have every where agitated and corroded the surface of our globe.

In order more fully to illustrate this arrangement, I have drawn in figure 1st, Plate IV. an ideal portion of a coast similar to that which we have been describing; and in figure 2d, the same has been exactly repeated in black lines. But in this last figure, I have introduced a continuation of each of the strata in dotted lines; so that every one of them is rendered completely continuous from end to end.

This theoretical completion of these forms, may be of service in accounting for the anomalous circumstances already mentioned, as belonging to the strata of killas. In particular, we may thus readily account for the abrupt change of dip from east to west. Thus, in figure 1. Plate IV. we see the strata *a b*, and *c d*, dipping rapidly to the east, and those at *e f*, and *g h*, as rapidly to the west; yet, at their *outgoings*, or appearances at the surface, they are very little removed from each other; and if the middle point, where the convolution takes place at *m*, were hid from the view or removed, the appearance would be completely paradoxical.

Making allowance for shifts, and various interruptions, great part of the coast may be thus explained: but this simple curvature, though general, is by no means universal in the killas; as appears in some places upon this coast, to the eastward of Eyemouth, at Gun's Green, where the axis of convolution is very irregular, and is sometimes vertical; and also in Galloway, where the strata present to view much more irregularity. But these anomalies, though more complicated, seem all to be of the same class, and to denote the influence of similar actions, as I shall endeavour to shew in the course of this paper.

In reducing these irregular forms into system and connection, one object, of no small consequence in geology, seems to be obtained ; but it would be desirable, if possible, to go a step farther, and to discover by what means this peculiar arrangement has been brought about. For this purpose, it will be necessary to shew, first, That this peculiar conformation may be given to a set of horizontal beds by a mechanical force of sufficient strength ; and, secondly, That there are rational grounds for believing, that such a force has been actually exerted in this case. I have now, and formerly, tried to establish the first point by experiment ; and I shall endeavour to vindicate the second by a train of geological reasoning, founded upon some volcanic phenomena.

In the year 1788, when I had the pleasure of visiting the coast of Berwickshire, in company with Dr HUTTON and Mr PLAYFAIR, it occurred to me, that this peculiar conformation might be accounted for, by supposing that these strata, originally lying flat, and in positions as nearly level as might be expected to result from the deposition of loose sand at the bottom of the sea, had been urged when in a soft, but tough and ductile state, by a powerful force acting horizontally ; that this force had been opposed by an insurmountable resistance upon the opposite side of the beds,—or that the same effect had been produced by two forces acting in opposite directions ; at the same time that the whole was held down by a superincumbent weight, which, however, was capable of being heaved up by a sufficiently powerful exertion.

By either of these modes of action, I conceived, that two opposite extremities of each bed being made to approach, the intervening substance, could only dispose of itself in a succession of folds, which might assume considerable regularity, and would consist of a set of parallel curves, alternately convex and
concave

concave towards the centre of the earth *. At the same time, no other force being applied, any two particles which lay with respect to each other, so that the straight line joining them were horizontal, and at right angles to the direction of that active force, would retain their relative position, and of course that line would maintain its original straightness and horizontality; and thus the forces exerted being simple, or, if compound, tending, as just stated, to produce a simple result, the beds would acquire the simple curvature ascribed to them by Mr PLAYFAIR, and which belongs to them, in the immediate neighbourhood of Fast Castle; whereas, in Galloway, and in some parts of our coast, particularly near Gun's Green, to the eastward of Eyemouth, where the curvature deviates from that simple character, and becomes in the utmost degree irregular, we must conceive the force to have been more complicated, or most probably to have acted at successive periods.

This conjecture no sooner occurred, than I endeavoured to illustrate my idea by the following rude experiment, made with such materials as were at hand. Several pieces of cloth, some linen, some woollen, were spread upon a table, one above the other, each piece representing a single stratum; a door (which happened to be off the hinges) was then laid above the mass, and being loaded with weights, confined it under a considerable pressure, (fig. 3. Plate IV.), two boards being next applied vertically to the two ends of the stratum.

* I am aware, that this expression of parallel curves is irregular; but I can find no other mode of conveying the idea. It is not easy to trace, *à priori*, what form would be assumed by these beds, supposing the whole to be held down by a force so powerful as to prevent any vacuity. It is enough, however, for our present purpose, that the forms of nature correspond with those obtained in an experiment soon to be mentioned.

tified mass, were forced towards each other by repeated blows of a mallet applied horizontally. The consequence was, that the extremities were brought nearer to each other, the heavy door was gradually raised, and the strata were constrained to assume folds, (fig. 4. Plate IV.), bent up and down, which very much resembled the convoluted beds of killas, as exhibited in the craggs of Fast Castle, and illustrated the theory of their formation.

I now exhibit to the Society a machine, by which a set of pliable beds of clay are pressed together, so as to produce the same effect, fig. 5. ; and I trust, that the forms thus obtained will be found, by gentlemen accustomed to see such rocks, to bear a tolerable resemblance to those of nature, as shewn in fig. 6., copied from the forms assumed in the machine, by an assemblage of pieces of cloth of different colours.

It still remains for us to consider how this *horizontal thrust* may have been produced. It will be found, I conceive, to arise, as a natural consequence from Dr HUTTON'S original hypothesis, according to which our continents have been raised from the bottom of the sea, and elevated to their present positions, by the internal action of the same heat which shews itself externally in volcanoes.

The most obvious mode of investigating these internal actions, in pursuance of the Huttonian view, is to study the external volcanic phenomena, and to consider what variations and modifications would be produced upon these last by the circumstances attending the subterranean action of the same powers.

With this view, I beg leave once more to solicit the attention of the Society, to a scene which I have mentioned in former papers, and to refer to some plates representing it, which I have given in the sixth volume of our Transactions, in my
paper

paper "On the Effects of Heat modified by Compression." This scene, as viewed in the *Atrio del Cavallo*, (Plate V. volume VI. figs. 41, 42, 43, and 44.), exhibits in nature a complete section of the old volcano of Vesuvius, now called Somma. The mountain is there seen to be composed of a succession of beds of lava and of cinders, the lava occupying only a fourth or a fifth part of the mass, which is traversed vertically, but irregularly, by numerous rents filled with solid lava; these rents, as I have endeavoured to prove, having undoubtedly served as the pipes through which lateral eruptions have been discharged.

Each of these rents would continue open during the course of the particular eruption by which it was formed, and the lava would flow freely through it; but when the eruptive impulse ceased, it would remain full of the liquid lava, which would congeal, so as to leave the rent, as it now appears, completely filled with hard and solid rock. This new substance welding itself firmly to the extremities of the beds of lava which had been broken across, would bind them together into a species of net-work, and thus the injury done to the mountain by the formation of the rent, would be repaired, and much more than repaired; so that when a new eruption was directed to the same quarter, it would be less able to penetrate than before, and the eruption would be restrained till a fresh rent was effected in some other part of the mountain. A new eruption must thus, in every case, be an act of violence; and we see how a lateral eruption may be followed, as frequently happens, by a discharge of lava from the summit of the mountain, which could not have taken place, had the first lava continued fluid, since it would never have ceased in that case to flow through the lowest aperture, whereas, in consequence:

consequence of the congelation of its upper part, that aperture is closed.

What is true of volcanoes, must be no less true of those internal operations, which, according to the Huttonian theory, have been the means of raising all the rocks and mountains from the bottom of the sea into their present situation; and by which the unstratified substances have penetrated the strata, and filled the rents formed in them, producing the veins or dikes so common in this country, just as we have seen the lava of Mount Somma filling the rents through the beds of lava and of cinders.

It cannot be doubted, that the secondary strata must have been greatly strengthened in this way. We may be satisfied of this, by looking at any great dike of whinstone (such as that of twenty or thirty yards in breadth, now opened on the north side of Edinburgh, as a quarry for pavement), crossing and connecting substances of every variety of hardness; also at the two small dikes which appear crossing the loose shale in the bed of the Water of Leith, close to the two mineral springs.

The introduction of this new substance, and the heave of the superincumbent mass, which is its necessary consequence, have been productive of several very important results, which shall be the subject of a future communication to this Society. Let us confine ourselves at present, however, to the convolutions of the killas.

According to the Huttonian theory, that loose assemblage of sand of various qualities, which was destined to give birth to strata of every sort, from gneiss to sandstone, lying originally in a position nearly horizontal, as deposited in beds at the bottom of the sea, and being acted upon from below, on successive occasions, by a heat of great intensity, must be
conceived,

conceived, in consequence of the progress upwards of that heat, to have possessed at any particular moment a great variety of intermediate temperatures, between that intense pitch and the ordinary heat of the sea. Owing to these varieties of substance and of temperature, the utmost diversity of character in point of tenacity, from firmness and brittleness, to the most perfect pliability and ductility, must have belonged to the assemblage in various parts.

Let us now suppose a rush to have taken place from below upwards, of any of those bodies in a state of liquid fusion, which on cooling have constituted all our unstratified substances, from granite to whinstone inclusive, and that this fluid was urged by an irresistible force; the consequence must be, that the stratified mass would yield in various modes. Such beds as were in a frangible state, would yield by the formation of rents, and the others, by having their substance forced through and partly dragged upwards. Into these rents and openings the unstratified matter in fusion would enter, and would proceed upwards more or less, according to its fusibility. Whinstone, the most fusible of the set, would flow the farthest, and would even perhaps arrive at the surface, and there discharge itself in the open air as a real lava, or, breaking through the bottom of a deep sea, might constitute a submarine lava, like one of those observed in Iceland by Sir GEORGE MACKENZIE, which, with the characteristics of a lava, have their cavities studded with calcareous spar*. When I met with this ob-

VOL. VII. M observation

* M. DE LUC, in his *Elementary Treatise on Geology*, p. 365. art. 311. has undertaken to shew, that my experiments with compression are not applicable to Dr HUTTON's hypothesis. "When calcareous substances," (he says, p. 365.) "are calcined in open air, the fixed air which is produced immediately escapes,
" and

servation in Sir GEORGE MACKENZIE'S work, it recalled to me what I had seen in Sicily, where the Cyclopiian Islands, at the

“ and it continues to form until the substance is deprived of its ingredients ; but
 “ when a solid body prevents its escape, the particles first formed acquire a de-
 “ gree of condensation proportional to the resistance which they experience,
 “ and they oppose in their turn the same resistance to the formation of other
 “ particles. Under such circumstances, therefore, if the heat be increased, it
 “ produces other combinations of the *fixed air* with the calcareous earth, as in
 “ the experiments of Sir JAMES HALL. But, under *water*, which the particles
 “ of that gass can easily penetrate, in which, collecting in bubbles, they will
 “ rise rapidly, on account of their inferior specific gravity, there can be no im-
 “ pediment to their formation, any more than that of the *aqueous vapour* in wa-
 “ ter, under the pressure of the atmosphere, when the heat is sufficiently in-
 “ tense.”

When M. DE LUC says, that in my experiments a solid body preventing the escape of the *fixed air*, “ the particles first formed acquire a degree of condensation proportional to the resistance which they experience,” he must conceive, that during the first application of heat, some *fixed air* has separated from the lime, and has accumulated in the cavity left in the barrels. But if he will look again into my paper, he will find that I had foreseen this inconvenience, and had guarded against it ; that being under the necessity of leaving some cavity, in order to allow for the liquid expansion of the fusible metal, I introduced some water into the barrel, which assuming the gaseous form, and reacting with great power, before the heat had risen to the calcining point, effectually prevented the separation of any *fixed air*. And the same thing would happen at the bottom of a sea that was deep enough. In some of my experiments, made with a compressing force equal to 171 atmospheres, equal to 5693 feet, or about a mile of sea, the carbonate bore the heat of melting gold without calcination, and entered into fusion. Now, it is obvious, that the same result must take place at the bottom of a sea of this depth, and that a shell lying on its bottom, if met by a lava whose heat was equal to that of melting gold, would enter into fusion, and no *fixed air* would be separated in the form of gas. M. DE LUC'S objection, therefore, which is founded on the levity of the substance in that gaseous form, must fall to the ground.

In those experiments which I have made, with a compressing force applied by means of a known and regulated weight, the carbonate has been placed exactly

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the foot of Mount Etna, are possessed of that same character. M. DOLOMIEU considers these and others as having been covered by the sea.

Other unstratified substances would attain, in a liquid state, to positions less and less elevated, as they were more and more refractory; and the granite would be the soonest congealed, being the least fusible of the whole set. In any of these cases, when the opening above was stopt by congelation, the force from below being irresistibly powerful, the liquid, as we have said, must have found room for itself among the strata. This must have been done in one of two modes.

Either, 1st, when the strata were in a hard and inflexible state, in which case, the liquid must have forced itself between stratum and stratum, by flowing horizontally among them, by which means an arrangement would be produced, similar to that of great part of the group in this neighbourhood, consisting of Arthur's Seat and Salisbury Craigs, as well seen from the south-west, where thick beds of uniform basaltic matter, emanating on both sides from the vast massive block in the middle, which rises highest of any, are interposed between thin beds of freestone, lying parallel to each other, and inclined to the horizon at an angle of about thirty degrees.

Or, 2dly, where the strata were soft and pliable, and possessed of considerable toughness. In this case, they would yield on both sides, so as to allow the vein to become wider

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in the predicament ascribed to it by Dr HURTON, when exposed to the same heat under a sea whose pressure is equal to that force.

The fusions, then, which have taken place in my experiments, confirm Dr HURTON's theory, in so far as it depends upon the action of heat on limestone.

and wider to an indefinite extent, producing a solid unstratified mass. At the same time, they must have made room for that yielding, partly by heaving up the superincumbent mass, and partly by propagating the motion horizontally along their own beds; which last motion will be opposed by their friction and inertia. We should then have three forces more or less opposed to each other; the force of elevation of the liquid, the superincumbent weight, and the friction of the strata. The consequence would be, that the strata, to a certain limited extent, would be thrust horizontally; and so far as that action reached, would be placed in the exact predicament of the pieces of cloth in the old experiment, or of the clay in that before us, (figs. 4. and 6. Plate IV.) We have every reason to expect, then, that our experiments have been a faithful representation of what would happen in a similar case in nature. And the results we have obtained, bearing an exact resemblance to the peculiar forms of the strata of killas, seem to justify that conclusion. If we suppose two such invasions of matter in fusion, to rise parallel to each other, and both under the circumstances above described, their influence exerted in parallel, though opposite directions, would conspire, and the space between them would be still more completely convoluted than where one action alone took place; or would be carried to a greater extent, than merely the double of one of them. Were the neighbouring veins inclined at any angle towards each other, the amount of the effect produced, and the situation of the axes of convolution, would be difficult to calculate; but it is certain, that their effects would still conspire. The complication would be still farther increased along with the amount of the action, if a third vertical burst of liquid matter occurred, so as to enclose a district of the stratified mass within a triangle. We are not possessed of data by
which

which the extent of such a triangle may be limited. It seems clear, however, that the greater the superincumbent mass, and the deeper the bed of ductile strata, provided that the force from below were sufficiently powerful, the more extensive would be this influence. And the influence of such a force upon a mass which had, by previous actions, acquired a simple curvature, will account for any deviation from that simplicity. By acting upon a set of beds previously rendered vertical in part, it might thus produce convolutions having a vertical axis, as at Gun's Green.

Let us now suppose, that, after the strata had cooled to such a pitch as to lose their pliability, a fresh quantity of matter in liquid fusion was impelled against them; the whole penetration would now be effected, by rents traversing the convolutions in all directions, and the liquid stone would flow into them, the temperature of the strata not having yet fallen so much as to occasion its immediate consolidation. Some of these rents might penetrate the mass in directions horizontal or nearly so; and these, swelling by the infusion of liquid from below, would form great unstratified masses, and contribute powerfully to the general elevation.

All the junctions of stratified with unstratified matter, which took place during this second stage, would possess a different character from that in the first. In the first, the axes of convolution would of course be parallel to the neighbouring boundary, between the two substances; whereas, in the second case, no such parallelism need be looked for, since the liquid stone would flow in rents, and along surfaces which were produced by the laceration of a frangible mass, in which the axes of convolution would act no part.

According to this view, as all the substances concerned must have lain very deep at the time of the first operation, and as the mass by which this powerful horizontal thrust was performed,

performed, must have been of great bulk and consequent solidity, it seems probable, that the convoluted mass, together with the matter in a liquid state which it had received after its consolidation, would, upon a subsequent application of the elevating cause, be more easily raised than the solid unstratified mass, by whose swellings the convolutions were first effected.

It is natural, then, to expect, that in the various rents and agitations which these masses have evidently undergone, the original mover of the convolution, and the scenes of its meeting with the stratified mass, may very frequently have been left in the deep abyss. In an account which I am now preparing to lay before the Society, of circumstances relating to the revolutions of the earth's surface, I shall have occasion to state a fact, which seems to prove, that the particular mass of killas of which we have been speaking, has undergone one palpable revolution of this sort, by which the mass in its immediate neighbourhood has been left at a considerable distance below it. I must hope, however, that on some occasions, scenes exhibiting the junction of the prime mover with the strata, and in which the axes of convolution shall be found parallel to that junction, may hereafter be discovered, in situations raised up, and well exposed to view; and I recommend this as an object of great interest to geological observers.

Our attention is naturally turned in this case to Granite, as far surpassing in importance every other species of unstratified body; and I have little doubt that it has in fact been the agent of these convolutions. I have not, however, been able to discover any case in which it has performed this function. All the junctions which I have seen belong decidedly to the second class mentioned, and bear marks of an infusion of the liquid granite into hard and brittle strata; and the peculiar
junction

junction we have in view, if it does occur at the surface, will most probably be found where granite meets with gneiss or mica-slate, not where it meets with killas *. I have no scruple, however, in presenting these speculations to the Society, although I cannot produce direct evidence in support of them, because I trust that the conjecture is sufficiently plausible to merit some attention; and, above all, because it may be the means of giving rise to much interesting observation, in a department hitherto overlooked, or in which, for want of any connected system, the observations of travellers have been lost.

It will be an object of consequence, that future travellers should attend to this circumstance in the Alps, where a very long ridge of granite is bordered on both sides by strata. The ideas just stated, not having occurred to me till long after I had left that country, I can form no judgment with respect to what inferences may be drawn from the state of facts which are there to be seen. It is certain, that the strata of the Alps are very much convoluted, as mentioned in various places by M. DE SAUSSURE; but whether these forms could reasonably

* GNEISS is found to pass by insensible degrees into granite; that is, specimens of every conceivable intermediate step have been found. We may then conceive one stage more advanced towards granite than the rest, in which all character of the original stratification is removed, and the mass may have become wholly crystalline, but in which the peculiarity of each stratum may still have left a trace of its character, in the quality of the granite thus produced from it. This seems to explain the nature of a great part of the internal ridge of the Alps, which is possessed, as SAUSSURE mentions, of a stratified character. This mass may, again, be traversed from within by granite in higher heat, and in a state of complete liquidity, which would be more ready than the mass first described, to penetrate into the neighbouring masses. Accordingly, SAUSSURE observes, that those veins, and other masses which, project from the central ridge, and penetrate farthest into the neighbouring strata, are devoid of that stratified character.

be ascribed to the elevation of the central granitic ridge, or whether its arrival at its present place has been connected with their formation, does not appear from any of the facts which I have read an account of, or recollect to have seen. It is probable that the point will not be decided, till that country is visited by a person previously aware of the theoretical views which we have been considering.

A case of such convolutions is mentioned by M. DE SAUSSURE, at a spot which I remember well, the cascade called the Nant d'Arpenaz, on the road to the Glaciers of Chamouni. The strata there consist of limestone, and are bent in such a manner, as to have struck that valuable observer with the utmost astonishment; yet, in our view, they may easily be accounted for by a lateral thrust. In reasoning upon this subject, he finds himself forced to contemplate the possibility of those convolutions having been the work of subterraneous forces, occasioned by internal fire; but he abandons the idea almost as soon as he has formed it, from the reflection that this mountain, and its neighbourhood, shew no indications of the action of fire. After a good deal of argument, he at last (though with seeming reluctance), submits to the idea, that these great results may have been the work of crystallization; the insufficiency of which reasoning, Mr PLAYFAIR, in his *Illustrations*, art. 207. has clearly pointed out.

Since this paper was read in the Society, I have met with a very interesting account of a set of rocks in Argyleshire, whose position and arrangement greatly resemble those which I have been describing, and among which, one fact occurs which seems well worthy of notice. (This account is contained in the Edinburgh Encyclopædia, under the article ARGYLESHIRE, and was written by Mr ARCHIBALD CAMP-

BELL,

BELL, whose recent death is much to be lamented). The strata there described occupy a great extent of coast, nearly forty miles; they consist of strata of slate and limestone, sometimes alternating, and seem to belong to the class of killas. Their convolutions are less elevated and less abrupt than those we have been describing, but in all other respects exactly resemble them. One circumstance is mentioned by the author as a simple fact, and without view to theory, but which seems, in a striking manner, to accord with what we have endeavoured to establish. "Where the strata (he says) consist chiefly of limestone, with few, or very thin strata of slate intervening between them, the thickness of a stratum is frequently five or six times greater at the summit of the wave, and at the hollow where it begins ascending to form the next wave, than at the intermediate point, where the contrary flexure takes place."

I recollect no such difference as to thickness among our strata; but the circumstance might be expected, upon our theory, to take place, when the beds acted upon by the horizontal thrust were not only flexible and tough like cloth, but also ductile, and capable of being elongated by pressure. For supposing the thrust to have continued, after the folds had, to a certain extent, been accomplished, it is evident, that the horizontal pressure acting in some degree at right angles to the beds, where the contrary flexures took place, and of course where their position was most erect, would tend to elongate and thin them at those places, and would have a contrary effect, if any, at the summit and hollow of each arch, where the stratum for a short space occupies an horizontal position. This unexpected fact tends then, I conceive, in a striking manner, to confirm our theory.

WHEN we undertake to account for the convolutions of the killas, by the forcible invasion of granite, one material point is to show, contrary to the opinion entertained as yet, I believe, by all geologists but those of the Huttonian school, that granite has been the latest formed of the two. The scenes in Galloway, where these substances meet, prove this, I conceive, beyond dispute; but they prove it over much in one point of view, since they show the arrival of the granite at its present place, to have been posterior, not only to the formation of the strata of killas, but also to their convolutions when in a state of softness, and to their subsequent consolidation. It must, therefore, be admitted, that this mechanical effect cannot have been produced by the particular granite there exhibited; but the circumstances which the junction presents to view, authorise us to believe, that another granitic mass, acting in a former period, with the same powers, but when the killas was in a soft state, has been the agent of these convolutions. It becomes, therefore, of great importance in this inquiry, to make a clear statement of the mode in which the introduction of the granite into its present place has been effected in this authentic instance.

In that view, I shall ask permission to lay before the Society, the details of some observations, which, in general terms, I mentioned in a former communication.

In the year 1790, I read an account in this Society of my observations on one of those granitic masses which reaches from Loch Ken to the valley of Palnure, a short abstract of which has been published in the History of the Society at that period. It is there stated, that I had traced the junction of this mass with the neighbouring rock, in a complete circuit of it, which I made in company with the Honourable THOMAS

DOUGLAS

DOUGLAS (now Earl of Selkirk), and that in “ all this extent, “ where the junction of the granite with the schistus was visible, veins of the former, from fifty yards to the tenth of “ an inch in width, were to be seen, running into the latter, “ and pervading it in all directions, so as to put it beyond all “ doubt, that the granite of these veins, and consequently of “ the great body itself, which I observed forming with the “ veins one continued and uninterrupted mass, must have “ flowed in a soft or liquid state into its present position.”

I have since, on many occasions, visited the same place, and every fresh observation has confirmed my first impression, and has served more and more to convince me, that the granite is posterior in formation to the killas, and has flowed into its present position from below upwards, in a liquid state, whilst the stratified mass was hard, or at least sufficiently so as to break with sharp angles, and to allow the liquid granite to mould itself upon its fractures.

I observed every circumstance that might be expected in such a case. I saw the granite meeting the strata in every possible angle. In one case, which occurs in the bed of the river, at the High Bridge of Dee, I saw the bounding surface of the granite dipping at an angle of 45 degrees from the centre of the granitic mass, and the strata lying upon it, in what (in the Wernerian language) is called a *conformable position* to the granite, and corresponding exactly to what they have held out as the mode in which the granite always meets the strata.

The Hill of Lauren, which occupies the south-west of Loch Ken (on the side of the granitic mass, nearly opposite to the spot last mentioned) presents a junction of these bodies, whose character is as completely different as could well be conceived. At the southern extremity of the ridge, the junction is well seen, characterised by large features. The strata

are here nearly vertical, stretching from north to south. The line of junction, which occurs on the face of the hill, towards its summit, cuts the strata at various angles, sometimes nearly at right angles; and the strata thus abutting endwise against the granite, the two substances are, as it were, spliced into each other. The granite enters among the strata in several large dikes or veins, which at first are a hundred yards in wideness, but which rapidly taper away to a small breadth.

Along the whole line of junction, from this point northward to the burgh of New Galloway, at a distance perhaps of two miles, a scene of almost perpetual interest presents itself, being a repeated display of the penetration of the stratified mass by the granite; and the rock being but thinly covered with soil, these circumstances come frequently into view, and particularly at the spots known by the name of Sight Knoll, and the Hog Knoll. In 1788, I had seen an instance of a dike penetrating the strata, and distinctly emanating from the mass of granite, in such a manner as to convince me that it constituted with that mass one uninterrupted and identical substance. On my return in 1807, when the subject had acquired a peculiar degree of interest, in consequence of the discussions carried on in this Society, the progress of vegetation had been such as to conceal it entirely; but being well convinced of the reality of its existence, I determined to recover it, and employed several workmen to clear away the earth and vegetable matter from the most interesting spots. At various distances, within fifteen or twenty yards from the main granitic mass, several masses of granite, or portions of veins, made their appearance, which I conceived to be emanations from the great mass, although their junction with it was concealed. I was anxious to trace some of these to their source; and after the labour of several days, I at last succeeded with
one

one of them, though I cannot tell whether or not this was the same which we saw in 1788. Having in this instance traced the communication between the vein and the main mass, I almost exclusively devoted my attention to it, and, I trust, not fruitlessly. Besides exhibiting by a drawing, (Plate V.), the appearances of this very interesting spot, which is called the Windy Shoulder, I procured a more expressive representation of it in the form of a model, which now lies before us*.

The Windy Shoulder is situated nearly opposite to the point on the other side of the loch, at which the Shirmers Burn enters it; the entry bearing from it E. 10 N. by true bearings.

The strata, which continue here in a vertical position, meet the granite on the surface at an angle of about 45 degrees. The dike runs for about twenty-three or twenty-four feet between two of the strata; it then starts across them, and resumes nearly its first direction, which it pursues for a considerable distance more; making in all ninety feet from the granite.

I was at the greatest pains to examine the circumstances attending the exit of the vein. The earth and vegetation were carefully removed. Some parts of the surface of the rock were dressed by a mason, and a powerful temporary polish was given to that surface by water dashed upon it. All these precautions tended to confirm the identity and continuity of the two rocks, which appeared more and more conspicuously after every fresh exertion that was made to remove the influence of external actions.

Several

* I have also presented one of these models to the Geological Society of London.

Several small veins were found to cross from the dike to the main mass, having an identity of substance throughout. That which is represented in the model, incloses a triangle of killas, whose sides are seven feet by five; the strata being here moved by a small shift. Many other smaller veins occurred of the same kind, and in the same position, which have been omitted as too minute. The dike, near its exit, also exhibits a fine example of another most important and instructive circumstance in geology. The granite actually contains a mass of the stratified body included in its substance, and surrounded on all sides with granite.

In the immediate neighbourhood of the granite, to the distance of a foot or two, and not more, the stratified matter has in many instances assumed a highly micaceous character, so as to deserve the name of Mica-slate, and perhaps of Gneiss.

Every thing seems to indicate that those dikes, which appear on this hill in such abundance, and which have been shewn in this case to be continuations of the same mass, have come from below; and this opinion seems to be strengthened by the fact, that on the north-west side of the same Windy Shoulder, the granite at the junction seems to dip under the killas. From the shape of the ground, sinking rapidly below the line of junction, I expected easily to reach the granite, by blowing up with gunpowder the killas at some point beneath this line. The first blast did not succeed; but a second, near the junction, was very effectual. It rent the mass at right angles with the junction, by which the granite is seen actually to dip under the killas, as at first supposed, with an irregular line.

The surface being dressed at the emergence of one of the small veins from the great mass, the following curious fact presented itself. To the distance of about three inches within

in the junction, the progress of the granite vein was visible through the outer part of the granitic mass; the substance of which last was there in a confused and undefined state: farther in, the vein spread wider, and in the space of a few inches more, was quite lost in the general mass. I account for this by supposing, that the granite near the junction being partly cooled, and partly contaminated chemically by the contact of the strata, is rendered less liquid than elsewhere, and that a quantity of more thoroughly liquid matter occasionally and subsequently forces its way through this barrier, and through the contiguous substances.

We have thus a representation in miniature of one of those events which I conceive to have happened on a large scale in the formation of that mass, which, in its present elevated situation, constitutes the body of our island. One portion of liquid granite, forcing its way among the strata of killas, then lying low and flat at the bottom of the sea, and in a state of softness and pliability from semifusion, has, by its swelling, pressed them into a convoluted shape, and has taken its station among them. By the progress of cooling, this whole assemblage, both stratified and unstratified, has become susceptible of laceration, and has been rent by subsequent forces acting from below. A fresh stream of liquid granite has penetrated into the rents so formed, and has swelled and spread among the convoluted and broken killas, so as frequently to occupy an extent of many miles, constituting one of our external granitic masses. But this last-mentioned invasion, though doubtless producing a comparative elevation, has still, I conceive, taken place at the bottom of a deep sea, where our sandstone strata have since been deposited, on the rock both of killas and of granite; and where the fragments composing that sandstone have undergone the moderate heat by which they
have

have been converted into stone. The whole assemblage of granite, killas and sandstone, having been raised into the position which they now occupy, by revolutions of a still posterior date.

The present order of things may thus be accounted for, by a set of progressive steps of elevation, without the necessity of supposing, everywhere, an interposed submersion. Not that I am disposed to deny the occasional occurrence of such submersions; which may naturally be expected to have taken place, in consequence of the voids, which could not fail to be produced by so many undoubted elevations.

The circumstances thus stated are different, as may be observed, on the Hill of Lauren, from those of the junction on the bed of the Water of Dee, as described above; but such differences are perfectly consistent with our view of a liquid forcing its way among a set of beds previously consolidated and indurated.

The conformable junction at the Water of Dee, is the only one which, being ambiguous, does not contradict the Wernerian view of the general system; a view that is completely excluded by the perturbed junction at Lauren. Now, both of these junctions are consistent with the Huttonian theory, according to which, a diversity of this sort was to be expected in such circumstances. It is very obvious, that the substance of a vein must be of newer formation than the rock through which the vein passes; and it is no less obvious, that where the angular fragments of one substance are contained in another, the substance thus contained must be the older of the two. In these observations, we have seen that two veins of granite, which penetrate the killas, do constitute one continuous mass with the great body of granite which lies below, and are of course of contemporaneous formation with it. The conclusion,

sion, then, is irresistible, that the granitic mass of Lauren is posterior in formation to the killas which lies above it. This admits of no other rational solution, but by supposing, as Dr HUTTON has done, that the granite, in a liquid state, has flowed in its present position, and that emanations from that liquid penetrating into the rents of the strata, have formed the veins.

The same general facts which have been observed with respect to the granite of Lauren, and the district of which it makes a part, occur also in the other two granitic masses of Galloway, as I have found by a particular examination of their junctions with the strata.

I was convinced of this as to the mass which crosses Loch Doon, by a circuit which I made of that mass in 1807, accompanied by Mr JARDINE of this Society. In the island upon which the Castle of Doon stands, a fine example occurs of angular fragments of killas, included in the granitic mass near the junction. The dressing which this stone has received from the hand of nature, renders this very conspicuous, as I shall have occasion soon more particularly to state to this Society.

I have at different times, though not with the same regularity, examined various places, where the third granitic mass in Galloway, of which the Mountain of Criffel makes part, meets the surrounding strata; and I have seen dikes of granite near the junction, and other circumstances leading to the same conclusion, particularly on the sea-shore, at a place known by the name of the Needle's Eye, which approaches to the granite boundary; and also on the side of Criffel, which lies directly opposite.

According to a rough computation, each of these three granitic masses occupies a space of about six miles by four.

Their positions, and bounding lines, which are very irregular, are laid down, as nearly as we could guess, in the map of the stewartry of Kirkcudbright, now 'on the table. There is good reason, I conceive, to suppose, that all granitic masses are related in a similar manner to the neighbouring strata, where such strata are found penetrated by granitic veins. Such veins were first observed, I believe, by M. DE SAUSSURE, in the Valoisine, and also at Lyons and Semur. Dr HUTTON examined them with great care in Glentilt, in Arran, and in other parts of Scotland, and upon these observations founded his bold and original theory with respect to granite. Mr PLAYFAIR has not failed to make ample use of those which he has more recently discovered at St Michael's Mount in Cornwall. All the appearances which I have witnessed in Galloway, as to the relative situation of granite, and the contiguous rock, accord with the doctrine of Dr HUTTON, and tend, I conceive, to confirm his views.

It might be rash to extend universally to all granite those conclusions which have been established by particular observations; yet as no instance warranting a contrary inference has occurred since the subject began to be inquired into, enough seems to have been done to contradict the generality of that leading maxim of the Wernerian doctrine, that the order of position which rocks maintain with respect to the centre of the earth, denotes the order of formation.

It is an important circumstance to observe, that the substance whose formation is thus proved to be prior to that of the granite of Galloway, is the killas, or grauwacke of the German school, which, holding only a middle station among stratified bodies, as to antiquity, is considered, according to their doctrines, or of formation long subsequent to that of any kind of granite. The quality of this stratified mass,
from

from one side of the island to the other, seems to be uniform throughout, except in the immediate neighbourhood, or contact of the granite, where it assumes a micaceous character, approaching to the nature of gneiss or mica-slate. This furnishes a most notable indication of the action of heat; since the granite, by its local intensity, has performed the very effect which Dr HUTTON ascribes to the general heat below, as acting upon the lower beds, and converting them into gneiss.

Another circumstance of importance is, that at the Rae Quarry in Peeblesshire, nearly in the middle of this mass of killas or grauwacke, a bed of limestone occurs, interstratified with the killas, and of course coeval with it, which bed contains shells in abundance. Now, as this mass of killas has been proved to be of older formation than the granite of Galloway, it is obvious that granite is here found to be of a formation posterior to the existence of living animals.

The relative age of granite and of sandstone has not been decided in any case that I have had occasion to observe, and is a point of considerable importance in geology. Nothing seems as yet to prove, that granite may not have flowed up against a stratified mass, possessing, in some of its parts, the properties of sandstone; I conceive, however, that these properties will nowhere be found to belong to that part of the stratified mass which lies close to the granite. For we have seen, on the Lauren, that killas near the junction has been changed into gneiss or mica-slate by the heat of the granite, as we conceive; and there is still stronger reason for believing, that a similar change would take place on sandstone in the same circumstances. We ought, therefore, to find, that while a remote portion of the stratified mass retains the properties of sandstone, that which lies but at a little distance from the

granite, has been converted into gneiss or mica-slate, or at least into killas; and this view is confirmed by the circumstance, that no example has been produced of granite veins traversing sandstone.

I expect, then, that granite and sandstone will not be found to occur in immediate contact, unless where the latter has been deposited upon the former. In the junction which is seen in the bed of the river near Jedburgh, and in that at the Siccar point, on the coast of Berwickshire, we see that the sandstone has evidently been deposited in the state of loose sand, on killas then existing as a hard and shivered rock. We may easily conceive, that sandstone has been deposited in a similar manner on the granite of second invasion; and that, in a similar manner, it has been consolidated and elevated.

I have not met with an example of this; but such may be looked for, and will be interesting in geology. I have just learnt from a young friend, member of this Society, who has spent a few days in the course of this year at the Cape of Good Hope, that upon the side of the Table Mountain, a junction occurs of granite with killas, and that, higher up on the hill, the same granite actually meets with sandstone. I hope soon to have the means of laying before this Society the details of these observations.

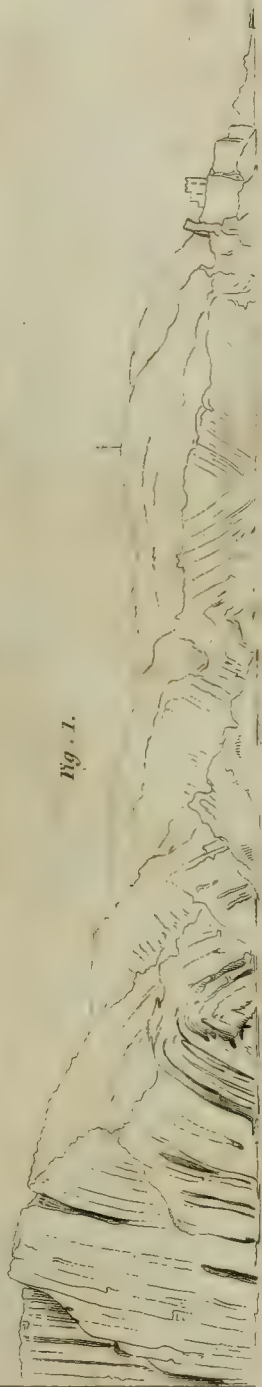
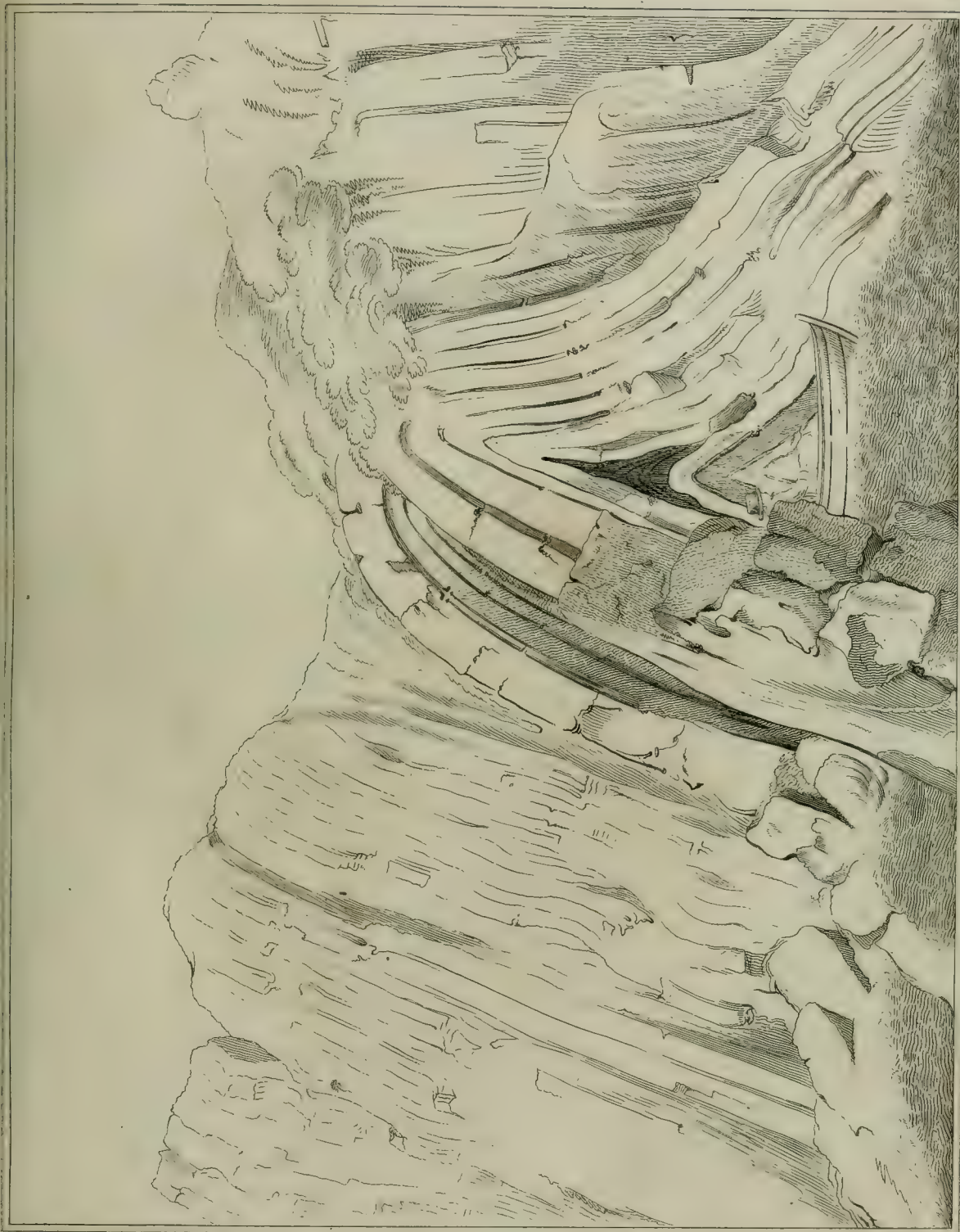


Fig. 1.



Fig. 2.







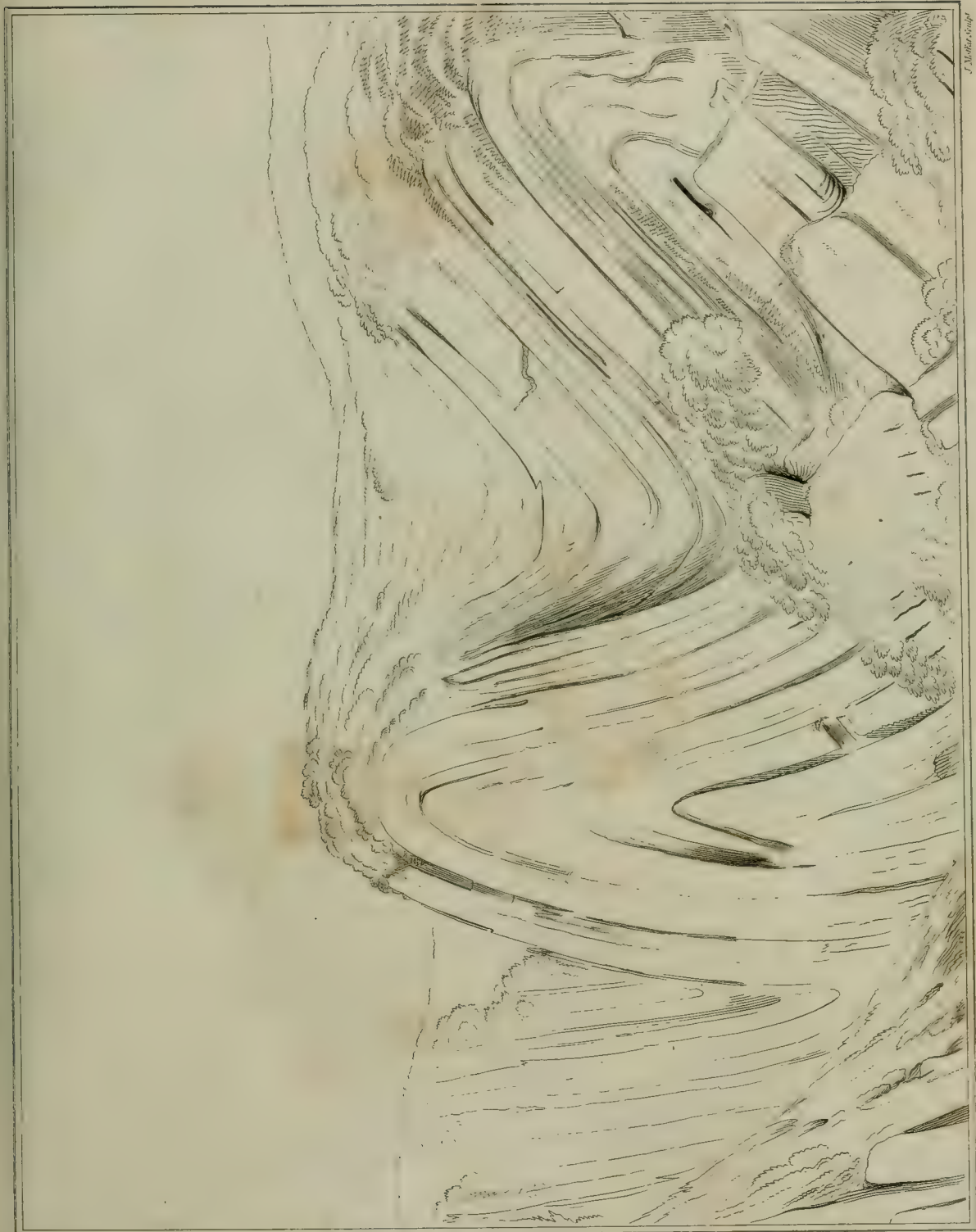




PLATE IV.

Fig. 1.

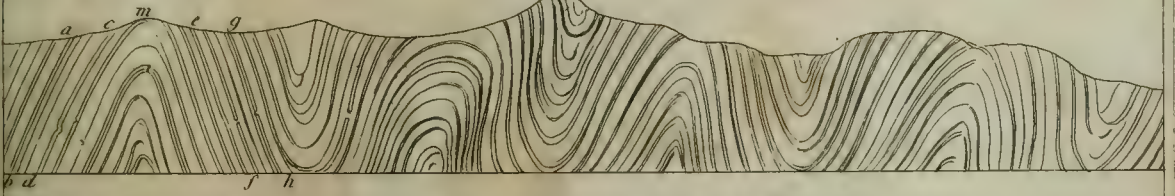


Fig. 2.

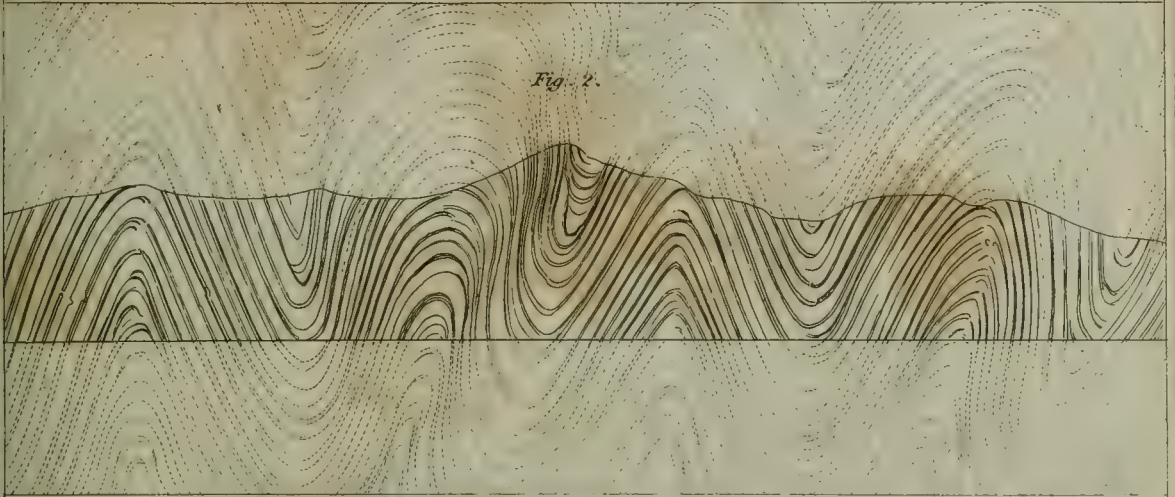


Fig. 5.

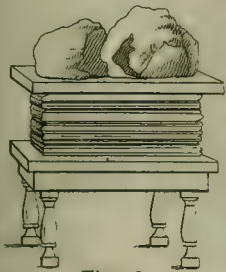
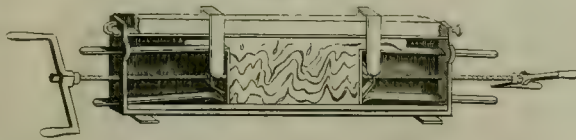


Fig. 3.

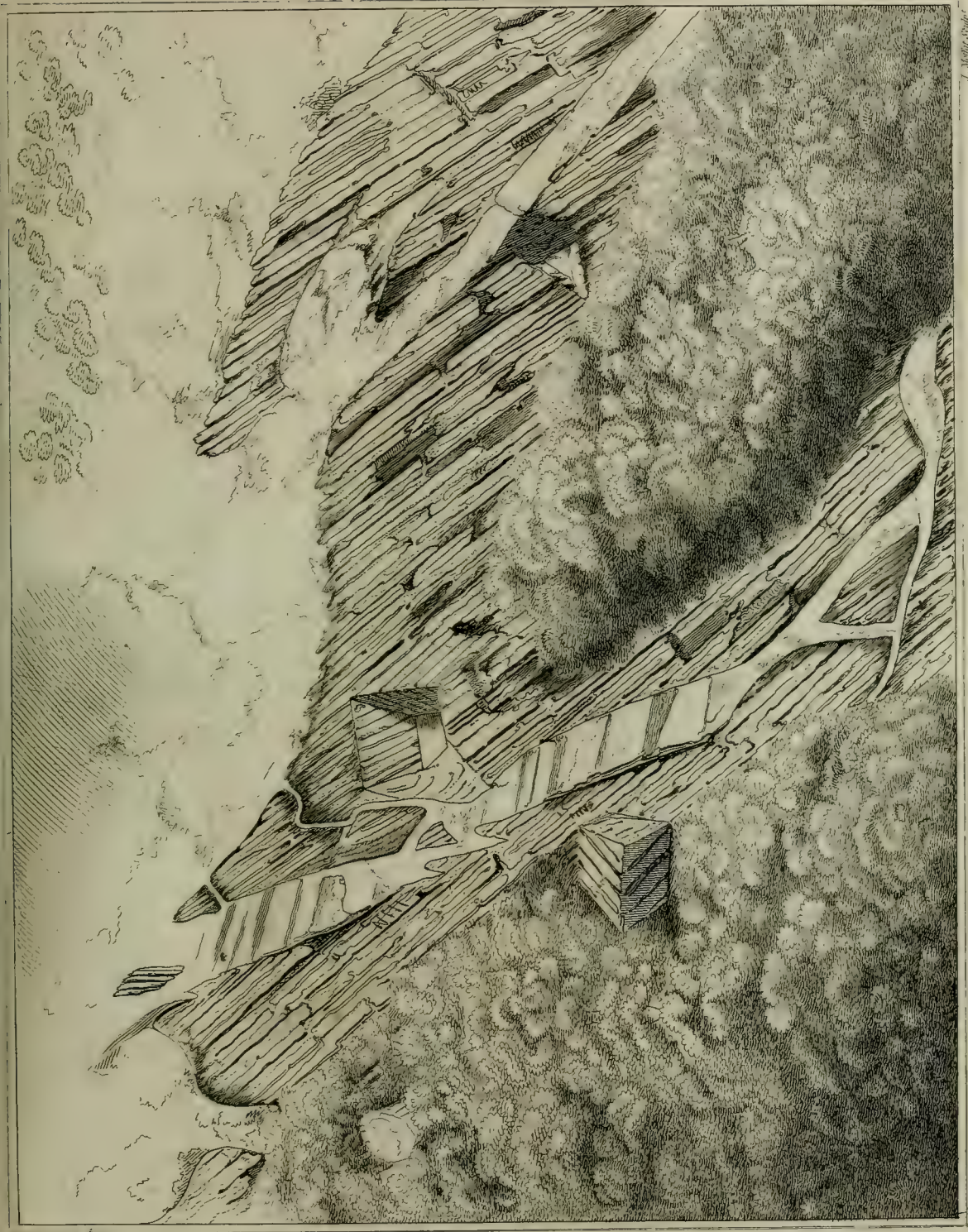


Fig. 4.

Fig. 6.







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III. *Remarks on the Transition Rocks of WERNER.*
By THOMAS ALLAN, Esq. F. R. S. EDIN.

[Read Feb. 17. 1812.]

ALTHOUGH we have many writers on geological subjects; whose works are distinguished by ingenuity of doctrine, and novelty of opinion, and, among them, some who have made advances towards arrangement; it was reserved to the celebrated WERNER, to introduce means, by which rocks might be described with some degree of precision. Many ingenious theories were invented, to account for their formation; but little or no attention was paid, to the acquirement of an accurate knowledge, either of their composition, or their relative position in nature; although these certainly appear to be the bases, on which such speculative opinions ought to be founded.

But while we acknowledge these obligations to the Professor of Freyberg, we cannot extend our unqualified approbation to the *systematic arrangement* he has introduced. It was not to be expected, that the labours of one individual, who, from peculiar circumstances, was confined within certain limits *, were sufficient

* In WERNER'S Preface to his *Theory of Veins*, he states, that his limited fortune, and the nature of his present situation, prevented him from travelling into more distant countries. ANDERSON'S Translation, xxiii.

cient to attain perfection ; nor could it reasonably be supposed, that any district, however extensive, should be so singularly favoured, as to contain all the variety of facts, that occur in other parts of the world, from which deductions are to be drawn, and elucidations afforded, investing phenomena with characters which they do not present elsewhere.

In forming his arrangement, WERNER may have exhausted the means he possessed ; he, therefore, ought not to be reproached, for although his conclusions are more general, than are warranted by the circumscribed field to which he was confined, yet he has formed a groundwork, on which the labours of future geologists may rear a system, more capable of affording satisfaction.

It is greatly to be wished, that arrangements of this kind were less dictated by theory. The pupils of the Wernerian School have been peculiarly fettered, by an ideal necessity of supporting the principles of their master ; but the blending of theory with description, is an error common to all speculative geologists ; the support of preconceived opinions being very generally the principal object in view.

Hence we find, that collections of those facts which are supposed favourable to certain doctrines, have been eagerly pursued, and others, equally interesting in themselves, entirely overlooked ; while that minute detail, which is alone capable of placing the student in a situation to draw conclusions of his own, has been totally neglected.

The part of the Wernerian System, which it is my intention to notice at present, is the class of rocks termed *Transition*. After stating the grounds on which this distinction has been established, and the particular rocks of which the series is composed, with their extent and importance, I shall endeavour
endeavour

endeavour to shew, that those which constitute its principal members, are similar in different districts ; and, finally, that they are of an older date than Granite, which maintains the first place in point of priority in the system of WERNER.

It is well known, that one of the principal arguments brought forward by Dr HUTTON, is drawn from the penetration of the stratified rocks, by veins extending from the mass of granite, which he considered as affording a decisive proof, of the subsequent formation of that rock. It must not, therefore, be supposed, that I aim at any thing original in the above assertion, or that I even wish to limit the term *Alpine Schistus*, as applied by that ingenious philosopher ; there can be no doubt, that, under this name, he included both the Primitive and Transition stratified rocks of WERNER ; but in his time no distinction had been drawn between them : it is only later discoveries that have imposed the necessity of more specific language, which may at once account for that want of precision by which his writings are so much obscured, and the deficiency of mineralogical knowledge, with which he has been so frequently charged.

WERNER, in the construction of his systematic arrangement, thought that he perceived grounds for considering all rocks, from Granite down to Clay-slate, as bearing marks of having been deposited from the original chaotic fluid, in a certain determinate order. In them no detritus, or anything like organised nature, was to be observed ; and to this point every rock remained exactly in the same state, in which it was at the period when it first acquired solidity. To these alone the title of Primitive was attached.

In the rocks immediately following, of which Limestone is said to be the first, he remarked an essential difference ; the limestone not only abounded in organic remains, but other
members

members of the series were composed of fragments, which must have existed previously in a different state: hence he inferred, that these rocks were formed at a subsequent period, which, from their constituent parts, he concluded, must have been after the creation of living animals, and nearly at the time when the earth passed from its chaotic to its habitable state *; and on these grounds he distinguished this class by the name of Transition.

To this another class succeeded, also presenting new and distinct characters, one of the most remarkable of which is position. They are never found conformable with the transition rocks; while these present an uneven or serrated outline, either from the natural contortions of the strata, or the broken edges of the highly inclined beds; the rocks which succeed, fill up the inequalities, and assume an horizontal position. To them he gave the name of Floetz rocks.

Thus the system is divided into three great classes, the Primitive, Transition, and Floetz.

Although the Transition has been known in this country as a separate class, only within a few years, yet it occupies a larger superficial extent in these islands, than any other rock-formation. But before I proceed to trace its limits, it may be proper to explain what is understood by the Transition Series.

In doing this, and, indeed, in whatever else I have stated, with respect to the Wernerian Geognosy, I beg to be understood as having taken it from that work, which I consider as containing the most authentic account of the system taught at Freyberg; I mean the third volume of Professor JAMESON'S *Mineralogy*. As WERNER has published no account of it himself, it is only from the works of his pupils that we can become acquainted with his system. After the intense labour which has
been

* JAMESON'S *Mineralogy*, vol. III. p. 146.

been bestowed on bringing it forward *, it cannot be supposed to contain any errors, according to the strict notions of WERNER ; and if his pupils find it necessary to introduce any material alterations, and so to mould it, as to suit their own subsequent observations, it will no longer be the system of that philosopher,—which the arguments in the present paper are alone intended to meet.

The Transition Series is composed of Limestone, Grauwacke, and Grauwacke-slate, Trap and Flinty-slate. Limestone is placed first, as being the oldest member, and is said to rest immediately on the newer clay-slate †. Of this we have no instance which I am acquainted with in Scotland, where, indeed, transition-limestone may be considered as rather of rare occurrence. Grauwacke, and grauwacke-slate are with us the principal members. The first of these is a stone usually of a bluish colour, passing into grey, and sometimes greyish-red ; it is composed of fragments, often of considerable size, but sometimes so minute as to be scarcely distinguishable ; these fragments are quartz, clay-slate, flinty-slate, and occasionally jasper, which are agglutinated by a basis of clay-slate, through which minute particles of mica are also sometimes dispersed.

Grauwacke-slate differs from the fine-grained grauwacke only in its minute stratification, and fissile character ; it bears so strong a resemblance to clay-slate in hand-specimens, that even an experienced eye cannot distinguish it ; in the rock it is not so easily mistaken : it usually alternates with grauwacke, and is often remarkably contorted. Both substances are traversed by quartz veins, which are sometimes of enormous dimensions, but generally very minute and abundant.

VOL. VII.

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* WERNER, “ after the most arduous and long-continued investigation, conducted with the most consummate address, discovered the general structure of the crust of the globe,” &c. JAMESON’S *Mineralogy*, vol. III. p. 42.

† JAMESON’S *Mineralogy*, vol. III. p. 147.

The only limestones of this class that I know of, are three : First, that of Rae Quarry, near Crook in Peeblesshire, where it is interstratified with grauwacke, and contains abundance of shells. The second is that of Cumberland, on the lakes of Windermere and Coniston, which also contains organised bodies. The third is the Plymouth limestone, which, according to the account of Professor PLAYFAIR, corroborated by Dr BERGER, is also transition-limestone; and in it Mr PLAYFAIR states, that he found a petrified shell *. I have not myself visited the spot, but it is of consequence to observe, that the limestones of all these different districts exhibit traces of organic remains. The other transition-rocks, are Trap and Flinty-slate †; but I have had no opportunity of observing either of them in their natural position. Such, according to WERNER, is the extent of the Transition series; but it does not comprehend all the rocks which occur in some of the transition districts, particularly that of Cumberland, although, with little exception, it is adapted to the south of Scotland, in a very remarkable manner.

I may now notice the extent of country occupied by rocks of this description; but such is our limited acquaintance even with our own island, that it can be done only in an imperfect manner. We know too little of the north of Scotland, to be able to say, what rocks occur beyond the Moray Frith; but it is by no means improbable, that when these regions have been more fully examined, the transition series will be found among them. Indeed I have learnt from Dr MACCULLOCH, that it occurs in great abundance in the north.

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* *Illustrations of the Huttonian Theory*, p. 165.

† I suspect both these abound in the mountains of Cumberland, from specimens I have picked up among the loose fragments.

I am inclined to consider, that it occupies a large proportion of Forfarshire; and if I be correct in an observation made on the banks of Loch Katrine several years ago, the transition rocks extend in that direction. I have likewise found traces of them on the right bank of the Clyde, near Dalnotter Hill in Dunbartonshire. But the transition country we are best acquainted with is that of the south of Scotland, which stretches entirely across the island.

On the one side, it begins near the boundary between East Lothian and Berwickshire, and continues along the coast, to a little beyond the river Tweed. Extending a line from the first, to a point on the west coast, between Girvan and Ballantrae; and from the second, another which shall pass, by Langholm, to a point between Annan and Carlisle, we shall find nearly the whole of the intermediate space to be Transition, excepting where granite comes in, and some partial deposits of later strata, which occupy the lower parts of the valleys of Nith, Annan, &c.

The mountainous district of Cumberland, Westmoreland, and the north of Lancashire, which is divided from the Transition of the south of Scotland only by a small proportion of *parallel strata**, belongs to the same, at least we know of none other with which it can be classed, although it contains a variety of rocks, which cannot be referred to any in the series of WERNER.

Adjoining to this, in the western part of Yorkshire, the same rocks occur: it is on these that the limestone of Ingleborough and Whernside rests. To this succeeds the extensive

P 2

district

* This term has been applied to distinguish the sandstone strata, and in that sense I now use it; it is objectionable, however; for all stratified rocks present the phenomena of parallelism, consequently, without qualification, this term affords no distinction.

district of parallel strata, including the coal-fields of Warrington and Wigan, and the great alluvial deposit of Cheshire. These bring us to the neighbourhood of the Welch mountains, which I believe are all of the same nature, some specimens having been given me by a member of this Society, taken from the summit of Snowden. Grauwacke, according to Mr Aikin, makes its appearance at Church Stretton in Shropshire* ; and near Hay, on the border of Hereford, I observed it myself.

A great part of Somerset, and, finally, the whole of Devon and Cornwall, again excepting the granite, and a small portion of serpentine, and some other rocks, are all composed of Transition strata. Thus, by extending a line almost due south, from Berwick to the English Channel, we shall find a large proportion of the country to the west composed of Transition rocks ; while, so far as I know, none occurs to the east of it ; although it is probable, that at Mount Sorrel in Leicestershire, some of the same series may be found.

We are still less acquainted with the precise limits of its extent in Ireland : we know, however, that it occupies the coast, from Belfast Lough to the mountains of Morne, which are of granite ; it also extends westward as far as Monaghan, and probably much beyond that point. From what Mr WELD states, in his account of Killarney, it appears to be the principal rock of the Kerry mountains, and I know it occurs in great abundance in the county of Cork.

Hence, even with the little information we possess respecting its exact limits, we have enough to know, that the transition rocks form a very large proportion of the superficial extent of Great Britain and Ireland, and also comprehend the principal mining districts.

Having

* *Geological Transactions*, vol. 1. p. 212.

Having thus imperfectly chalked out the boundaries, or rather localities of the transition districts in these islands, I shall endeavour to shew that some of the rocks of Cornwall are grauwacke, in all respects similar to some of the south of Scotland; and if strata may be compared to the leaves of a book, a few decided and indisputable specimens are sufficient to characterise a district.

It was in consequence of some observations during a tour through Cornwall and Devon last summer, that I was led to suspect this class stood in a very different relation in point of period, with respect to granite, from that which I had hitherto conceived; greater experience, or perhaps sufficient attention to the writings of Dr HUTTON, might have pointed out this before. Had I looked more attentively into his description of the granite district of Galloway, and at the same time attended to the nature of the stratified rock of which that country is principally composed, this fact would not have been new to me now. There were other circumstances, however, which severally contributed to prevent me from supposing that grauwacke could occur in this position.

First, The unlimited use to which Dr HUTTON applied the term *Alpine Schistus*, left us quite uncertain with respect to the species of rock he meant: secondly, the alteration induced on grauwacke, near its junction with granite,—a circumstance so strikingly exemplified in Galloway, that I own it deceived myself; and, lastly, the assertion I have so often heard repeated by the Wernerian geognosts, that granite veins never occurred excepting in rocks formed of the same constituents, alluding to gneiss and mica-slate.

Before I visited Cornwall, I knew that granite abounded in the Stannaries, and that tin and wolfram, metals which are considered nearly of the highest antiquity, were there common productions.

productions. I therefore expected to meet with a perfect epitome of the Wernerian System, containing the usual series of primitive rocks, descending from granite, through gneiss, mica-slate, and clay-slate, with all the *et ceteras* of serpentines, traps and porphyries; but in this I was mistaken.

On my approach to Exeter through Somerset, I first observed the transition strata between Bridgewater and Taunton; (Nos. 1. & 2.*) and from thence traced them, more or less distinctly, till I crossed the river Teign, which bounds Dartmore on the east. Thus far great part of the country is very flat, some of it extremely hilly as a road, but none of it mountainous. The transition strata are by no means continuous, and in many places appear only in small projections above the surface.

On the right bank of the Teign, the road winds up the side of a steep hill; and where the rock is cut, there is a considerable display of strata, having all the external appearance of grauwacke. On examining it, I found some of the strata coarser than others; but, in general, the grain was extremely fine, (Nos. 5, 6, 7.) the texture solid and compact, the colour very dark-grey: it was very tough under the hammer, it broke with a smooth, and somewhat conchoidal fracture, and did not split into the thin laminæ of the grauwacke-slate. This appearance puzzled me at first; the rock presented all the external characters of grauwacke, and yet internally it was different. I had not proceeded many paces, however, when I came upon Granite, (No. 8.) the proximity of which, as before mentioned, is always marked by a very material alteration in the consistence of the adjoining rock. This alteration, I observe, was not unnoticed by Dr BERGER, in his interesting paper † on the physical structure
of

* The numbers refer to the Appendix, at the end of this paper.

† *Geological Transactions*, vol. 1. p. 112.

of Devon and Cornwall. In mentioning grauwacke, which he distinguishes from grauwacke-slate only by its compactness, he says, "It is found higher up than the grauwacke-slate, it may be supposed to have been precipitated more slowly, and under less powerful pressure; whereby the mass has been allowed to contract, and to assume a kind of crystallization. It rests immediately on granite." The conclusions he draws are different from mine; but from the above quotation it appears, that the circumstance I observed at Teign Bridge is usual in similar situations all over Cornwall.

Near St Austle, on the road leading to Carclaze mine, I found grauwacke, (No. 23.) in my opinion extremely well characterised; also on the road to Cambourn, not far from Dolcoath, (No. 31.); likewise on the shore near Penzance (No. 42, 43.). Here it is also fine-grained, and tough under the hammer, and at no great distance from granite. Near Oakhampton, I found it along with grauwacke-slate, in the most unequivocal state, (No. 57.); and on the shores of the Bristol Channel, near Ilfracombe, the rocks are all of the same material*.

Here, on the beach, to the west of the town, I spent some hours the evening before I crossed to Swansea; and found nothing among the rocks, to lead me for a moment to question, that they were wholly composed of grauwacke. Indeed I even remarked some of the contortions which are so common in this rock. Next morning, however, when walking down to the boat, under a point where a small battery is built, I found on
the

* The specimens alluded to, were examined by the gentlemen present when this paper was read, who considered those from the road leading to Carclaze mine, and from near Oakhampton, as Grauwacke; and those from the vicinity of Penzance as Greenstone.

the trodden surface of the rock, an appearance very similar to mica-slate, for which substance it might readily be mistaken, (No. 61, 62.), but this resemblance appears to be owing to the friction of the feet, and the action of the weather, on a variety of grauwacke, containing an unusual proportion of mica.

By casting an eye over the map of Cornwall, it will be observed, that the above specimens are selected from the most remote corners of the peninsula. On examination, I think they will be found sufficiently similar to the Grauwacke of WERNER, to be entitled to be classed along with that rock. Dr BERGER, in his paper on the physical structure of Cornwall, gives them no other name, and if authority is to be qualified by experience, the opinion of one who has traced the footsteps of SAUSSURE, and who has studied the geognosy of WERNER with the utmost enthusiasm, cannot fail to be received with respect.

I have thus endeavoured to shew by the selection of specimens, and by the opinion of a very scientific observer, that the stratified rock of Cornwall is grauwacke. It would be uncandid, however, not to acknowledge, that the *general* texture of this rock was different from the grauwacke of the south of Scotland; it was more of the slaty variety, and frequently seemed, from its smooth and soft feel, to contain a large proportion of magnesian earth*.

I understand, in a course of lectures now delivering, a very material alteration has been proposed upon the Wernerian System,

* Since I read this paper, I have had occasion to pass through the transition country of Peeblesshire, &c. On former occasions, I was in the habit of searching for characteristic specimens of the grauwacke; I now looked for such as resembled the killas of Cornwall, which I found in abundance, (Nos. 62, 63.).

tem, in order to introduce this rock in a position distinct, and very distant from grauwacke. It appears to me much more simple to suppose, that rocks of the same class, in different districts, may present peculiar characters, than that the operations of nature should have been so multiplied and complicated, as to afford the endless distinctions which are thus required. Indeed, I cannot help thinking, that if the Killas of Cornwall had been sufficiently known, it would have excluded entirely the introduction of that harsh-sounding German term Grauwacke. *Killas* appears to me to be as proper a translation of that word as Specular Iron-ore is of Eisen-Glanz, and I think may be used with great propriety; distinguishing Grauwacke and Grauwacke-slate, by Amorphous and Schistose Killas.

The only other rock of any importance in Cornwall is Granite, termed *Grauen* by the common people,—a name also given to clay-porphry, a substance found pretty frequently in large veins, (Nos. 16, to 19, 28, 48.) The shades of distinction chronicled by the mineralogist, cannot be expected to attract the attention of the miner, who knows but two rocks, grauen and killas, throughout the Stannaries. It has been thought that a distinct rock was understood by the term *Elvan*; but this is a mistake, elvan may sometimes be greenstone, but in general is either killas or granite, and is so termed by the miner when he finds the rock harder to work in one place than in another.

Before I entered Cornwall, I was led to believe that it abounded in two kinds of granite, Primary and Secondary. Never having had an opportunity of comparing them *in situ*, I was anxious to do so here, and different localities were pointed out to me; these I examined with care, but could discover no grounds to justify any distinction. Dr BERGER makes no mention of secondary granite; and another gentleman, whose opinion on this, as on most subjects, will be received with the utmost deference, and who had the same ob-

ject in view, during a visit made since I was there, informs me that he could discover no distinction at all.

It is therefore of importance to ascertain whether the granite of Cornwall be new or old; which will easily be done, by comparing the appearances it presents, with the descriptions of these rocks as given in the Wernerian school; it is there taught, that three formations of granite have been ascertained.

The oldest is the basis or nucleus, round which all other rocks have been deposited. The second occurs only in veins, traversing *only* the granite of the older formation. The third rests on some of the older primitive rocks, in unconformable and overlying position. From this description of its external relations, it is evident that the granite of Cornwall can neither be the second nor third. With respect to its internal structure, we have the following definition: Granite is a granular aggregated rock, composed of felspar, quartz, and mica. These alternate from large to small, and even to very fine granular. The large and coarse granular usually belong to the oldest; the small and fine granular to the newest granite formations. Besides felspar, quartz and mica, other fossils sometimes occur in it; of these, schorl is the most frequent, then garnet and tinstone*.

At Penzance I observed some buildings constructed of a remarkably fine-grained granite; but this I nowhere saw *in situ*: otherwise, from Teign Bridge, where I first set my foot on granite, to the Land's End, it is generally of that character which entitles it to be ranked with the oldest variety, (Nos. 8, 21, 27, 34, 35, 54, 55.) In many places it has suffered to a most wonderful extent by decomposition, but where it retains its freshness, no granite can possibly be better characterised.

The

* JAMESON'S *Mineralogy*, vol. III. p. 102, &c.

The specimens which I was able to bring away, and which are now before the Society, are by no means adequate to convey an idea of the coarse texture it sometimes presents. In the granite of Dartmoor, the crystals of felspar are uncommonly large, often four inches in length. I believe it was from this neighbourhood that the flags of the footpath on Westminster Bridge were brought; in these, crystals of felspar nearly as large may be observed.

Granite countries usually present a bold and varied outline; but to this rule Cornwall is a most decided exception: its aspect is tame in the extreme, being comparatively flat,—a circumstance visibly occasioned by the corroding operations of time. Nowhere are the vestiges of degradation so remarkable as here. The enormous deposits of tin in the different stream-works, of which that of Carnon is perhaps the most extensive, clearly prove the destruction of surrounding mountains. This tin, in the shape of rounded pebbles, formed a stratum, of about a foot thick, under a deposit of granite-gravel and mud, together forming an overburthen of forty feet thick, and occupying a valley of very great extent. The lodes which furnished this tin must have existed above the level of the deposit; and from the quantity of metal deposited, they must have occupied a large tract of country. Other monuments of this general destruction may be found in the peaks which are seen in every direction in the granite districts of Cornwall. These are evidently the result of surrounding decomposition, and are formed of huge masses of rock, apparently piled on each other, with a regularity resembling masonry, and in all respects similar to the arrangement observable on the summit of every mountain in Arran, where the traces of time are also deeply furrowed.

Roach Rock, a binary compound of quartz and hornblend, is another very remarkable instance of the same fact: this rock is flat at the top, and being quite perpendicular on three sides,

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when viewed from the west, presents the appearance of a square castellated building, which is rendered more conspicuous, by being nearly of the same height as the tower of an adjoining church. There can be no doubt that this singular rock, owes its present appearance to the operations of time on the surrounding materials, which its peculiar composition has enabled it to withstand.

The killas likewise presents marks of degradation, where the country is composed of that rock. I noticed in some districts the roads mended entirely with quartz, (No. 24.) ; the brilliant white appearance of which, after a shower, had a very curious effect. I could not comprehend by what industry the accumulated heaps of this substance were obtained : at last I perceived that they were gathered from the adjoining fields, and in some places picked from the surface of a common, by means of a hoe or mattock. That fragments of quartz should occur so unmixed with any others, is only to be accounted for, by supposing that they formed the quartz veins in the killas, which, from superior tenacity, resisted decomposition, while the softer parts of the rock, yielding to the action of the weather, were reduced and carried away.

We thus find, that the granite of Cornwall possesses the characters ascribed by WERNER to that of the highest antiquity. Some inferences may likewise be drawn, in corroboration of its title to be classed with rocks of this description, from the nature of the metallic veins by which it is traversed.

In the German account of the relative ages of metals, tin is the third, and wolfram the fourth in order of antiquity*. If veins containing these metals, be considered in other countries as indicative of rocks of the oldest primitive formation, the same application must be made to those of Britain.

I may now ask, if this be not the Oldest Granite, where are we to find it? as it appears to me impossible that any substance

* JAMESON'S *Mineralogy*, vol. III. p. 275.

stance can more decidedly concur with definition. In the Alps, Dr BERGER must have learnt what primitive granite meant; yet not a doubt escapes him, of the Cornish being any thing else. Distinctions either do, or do not exist; if they do, character must be attended to; if they do not, it is quite unnecessary to add the terms Secondary and Tertiary to a substance possessing every attribute of a primary variety, merely because the structure of an adjoining rock does not accord with a specific theory.

Grauwacke, or, as I shall in future call it, Killas, I have before noticed, is a rock composed of fragments more or less comminuted, which must have existed in another state before they assumed their present arrangement. Along with the strata formed of these, beds of limestone are found, containing indications of organic remains. These are not confined solely to the limestone, they occur also in the killas; a fact which may be witnessed at any time, either in the neighbourhood of Coniston *, or on the right bank of the Blackwater, a little below Fermoy, in the county of Cork, (Nos. 66, 67.) The formation of this class of rocks was therefore subsequent to the formation of living animals, whose existence is supposed to be proved by the occurrence of organic remains in the composition of the rock.

In Cornwall, in Westmoreland, in Galloway, and in the counties of Down and Derry, this rock lies directly on granite, —a circumstance which we should at first sight be inclined to consider as indicating its subsequent formation. This thought, however, vanishes the moment we contemplate the veins of granite by which it is traversed. Of these there are many examples;

* Since I read this paper, I wrote to a friend at Coniston, requesting a few of these specimens, well characterised, might be sent me: some of which are deposited, along with the rest, in the cabinet of the Society, (Nos. 64, 65.)

amples ; but the most striking are at the Louran in Galloway, and at St Michael's Mount in Cornwall.

It is many years since Sir JAMES HALL laid before this Society an account of his observations on the granite district of Galloway, of which the Louran forms a part; and to the persevering activity of that gentleman, we are indebted for the display of one of the most interesting exhibitions of granite veins that exists. The peculiarities observable in Galloway, were first pointed out to me by him ; and as he has so lately favoured the Society with a particular account of them, it leaves me nothing to say regarding that quarter.

At St Michael's Mount, the shooting of the veins from the great mass of granite, is also most strikingly exemplified. They were here first noticed by Professor PLAYFAIR, who compares them, most aptly, to the ramifications of the vegetable root * ; for, indeed, nothing can be more illustrative of the phenomenon as it is here exhibited.

It is to be observed that granite veins, particularly when extremely minute, usually differ in texture from the mass to which they belong. While the little peak of St Michael's Mount maintains a similarity of character with all the rest of the Cornish granite, not only in point of internal structure, but with respect to the tin and copper veins which traverse it, as well as by the massive blocks, hewn by the corroding hand of time, which ornament its summit ; the veins that set off from it, gradually become finer as they recede, but still preserve the perfect character of the rock.

The importance deservedly attached by Dr HUTTON to the phenomena of granite veins, gave rise to a variety of hypotheses among those who were inclined to consider this rock as the original deposite, who have accounted for their formation in different ways.

It

* *Illustrations of the Huttonian Theory*, p. 318.

It was first stated, that they were formed of newer granite, and, if properly examined, would be found to cut the old granite as well as the rock which rested on it.

This opinion was once very strenuously supported in this country; but as facts would not bear it out, it was abandoned. I find, however, in a recent publication, something similar to it maintained by DE LUC, who asserts that the veins at St Michael's Mount are not granite, but merely quartz, which traverses the granite as well as the stratified rock. I cannot comprehend how DE LUC could have been so much deceived at this place; as simple inspection of the smallest specimen, will prove that he was mistaken.

It was next said, that the veins in question were not true veins, but such as are termed Cotemporaneous. To support which, it was boldly asserted, that they never extended beyond the limits of such rocks as were composed of the same materials, gneiss and mica-slate.

I trust it is now distinctly shewn, that they do extend beyond these limits, and likewise that they traverse rocks from which, by no method of reasoning, it can be supposed that they could possibly be formed by secretion.

The last opinion is that which has recently been brought forward by Dr BERGER*. After describing the granite veins of St Michael's Mount, he proceeds to say, that they are simply elevations on the plane of the granite existing previous to its being covered by the stratified rock; that the spaces between them were filled up as the grauwacke was deposited; and hence the abrasion of the surface, brought to light a section, which has merely an appearance of veins.

Were the devotion of Dr BERGER to his master less conspicuous in his geological disquisitions, I should be inclined, on the

* *Transactions of the Geological Society*, vol. i. p. 147.

the above statement, to call his character as an observer in question, having passed over in silence the detached masses of killas, which he could not fail to observe included in the granite, and which the above hypothesis is as far from accounting for as either of those mentioned before.

I have only a few specimens (Nos. 39, 40, 41.) to lay before the Society from the veins of St Michael's Mount; but they are equally interesting and satisfactory. One, exhibits a portion of the killas bounded on each side by granite; another, a portion of two granite veins traversing killas; and the third a mass of killas included in the granite.

Simple inspection is sufficient, in the first place, to shew that the opinion of DE LUC is groundless with respect to the substance of these veins. One of the specimens also, contains two small veins of quartz, which are of the kind called Cotemporaneous; these keep the direction of the seams of the stratified rock, and are cut off by the granite in the same line without any interruption.

To the opinion of Dr BERGER they also offer some reply. If the grauwacke had been deposited on the granite in the way he supposes, it is natural to conclude, that it would have been arranged in lines parallel to the sides of the elevations, somewhat similar to the coating of bark on the trunk of a tree: but in place of this, the seams of the killas are set at an angle of about 30° , to the planes of intersection with the granite; consequently, if deposited from a supernatant fluid, they have assumed a very different position from that which either mechanical or crystalline influence would have induced.

The hypothesis suggested to Dr HUTTON by the appearance of these veins, meets every difficulty: they conveyed to him evidence of being derived from a source of the greatest violence; and also that nothing but liquid matter injected from
below,

below, could have created the disturbance among the stratified rocks, so conspicuous when in contact with granite. As it is a self-evident position, that a rock which is cut by a true vein, must have existed in a solid state previous to the formation of that vein; so is it equally obvious, that if the vein can be traced into an adjoining mass, of which it is found to be a part, that mass must stand in the same relation, in point of period, to the rock which contains the vein, as the vein itself does: as also, that if pieces of one rock be found imbedded in another, the including rock must have been of subsequent formation to the included. No theory, however, but that of Dr HUTTON can account for these appearances: to nothing but force can the position be attributed, which the stratified rocks have assumed in the vicinity of the unstratified; and nothing but matter injected in a liquid state, could possibly have formed the shoots which traverse from the great mass of granite perforating the stratified rock, and at the same time envelope detached fragments of that rock. As the idea of violence in these operations has been so frequently combated, I cannot refrain from noticing here, a very striking mark of it I met with at Coul in Ross-shire, when visiting Sir GEORGE MACKENZIE. There the strata of gneiss are much disturbed by the invasion of granite veins: near which, on the outside curvatures of some of them I perceived rents similar to what we might expect on bending a flattened mass of clay, nearly deprived of moisture. I am fortunately enabled to present to the Society specimens illustrative of this interesting fact (Nos. 68, 69.)

In the theory of Dr HUTTON, we find also some grounds to account for the diminution of grain in the substance of the veins. The same cause to which, in a former paper, I attributed the gradation in the texture of greenstone, may be supposed to have acted here. It does not, however, ob-

serve an equal constancy, some veins of granite being as coarse-grained as the mass to which they belong.

In a former part of this paper, I had occasion to notice an alteration which appears to take place in the texture of killas, when in the vicinity of granite. This circumstance was so remarkable in Galloway, at the Louran and other places, that I took the strata so situated, for mica-slate, although I had observed no line of separation between it and the killas; I was forcibly struck with this, at the moment, but having then no time to follow it up, I was obliged to leave the country without any particular examination. It will be observed, by the specimens from St Michael's Mount, that the killas there assumes the appearance of fine-grained gneiss. At Wasseldale Crag, between Kendal and Shap, I noticed a rock, in the immediate vicinity of granite, quite similar; and I am told, that the texture of the strata, near the granite of the mountains of Morne, is altogether the same.

This alteration is always of a gradual nature; and is so imperceptible, that it affords a good example of what might be understood by the German term *Passage*, or transition from one species to another; this *Passage*, even admitting the substance altered, is of too limited a nature to constitute a distinct and totally different rock.

This alteration, if traced with attention, may lead to some very important results; but, without entering upon it at present, I shall content myself with recommending it to the notice of geologists, some of whom may consider it of too minute a nature to deserve attention. They may, however, rest assured, that it is only by an accurate examination, and a faithful detail of such objects, that we can hope to arrive ultimately at truth, the only solid basis of philosophic inquiry.

I may be accused of generalising too much in the foregoing statement, on grounds so limited; it must be remembered,
however,

however, that I have purposely confined myself to the examples of the relations which exist within my own knowledge, between the Transition rocks and Granite. The same phenomena are familiar, where gneiss and mica-slate come in contact with that rock ; but as these strata are considered to be of a very different age, the facts which I might have cited, had my object been to prove the age of granite with respect to all other rocks, were unnecessary, when my purpose was to point out the relative ages of killas and granite.

From what I have said, I consider myself warranted in finishing this paper with the following conclusions :

The Killas of Cornwall belongs to the Transition series of WERNER.

The Granite of Cornwall is possessed of every character by which the Oldest varieties are distinguished.

That Granite, the nucleus round which WERNER conceives all other rocks were deposited, is in some cases actually of a later date than the Transition series, which comprehends strata containing shells ; and that its subsequent formation is clearly evinced by the appearances at St Michael's Mount.

Hence, that the distinction of Transition rocks, is grounded on false conclusions.

And finally, That WERNER must make very material alterations on his present system, if he wishes to accommodate it to the phenomena so commonly presented in nature.

APPENDIX.

ON a former occasion, I stated as my opinion, that all geological papers ought to be accompanied with specimens of the rocks of which they treated. This is a condition not always to be complied with, unless the intention to write precedes the examination, when a collection may purposely be made; but when the idea suggests itself, after one is far removed from the district, it amounts nearly to an impossibility. In the present instance, although I be possessed of all the specimens necessary, they belong to a series which I formed for other purposes. Rather, however, than mutilate this, I have thought it better to present the whole to the Society, in whose possession I shall have an opportunity of referring to them at any time; and as they have signified their acceptance, it is necessary to add to my paper, the following brief list of the minerals I collected, which are marked and numbered, as picked up on my route, commencing in Somersetshire, where the transition rocks first make their appearance, and ending at Ilfracombe, after traversing Devon and Cornwall in different directions.

After leaving Bristol, on the road to Exeter, we traverse the limestone ridge of Mendip; to the south of which, there is an extensive plain, stretching to beyond Taunton, whose uniformity is occasionally interrupted by small isolated hills, like islands in a lake. These are probably formed of Transition
rocks,

- rocks, although on the plain itself, where the soil is laid open, which is principally composed of limestone debris horizontal strata of the same substance were exposed to view. Approaching Taunton, the road leads over some of these hills, and here it was that I met with strata highly inclined, very similar in colour and aspect to some varieties of sandstone, but considerably more refractory under the hammer, indicating, I suspect, the commencement of the Transition series.
- Nos. 1, & 2.**
- 3.** **VESICULAR TRAP.** I found this on the road near the house of Sir THOMAS ACLAND, a few miles north of Exeter. I saw none of this *in situ*, though very commonly in the buildings in and about Exeter.
- 4.** On quitting Exeter for Moreton, the road is extremely hilly, rising and descending over abrupt knolls almost all the way; these are principally formed of a soft decomposing rock, in thin strata, breaking in rhomboidal fragments, and very similar to the slaty clay of WERNER.
- 5, 6, 7.** After passing Teign Bridge, this substance assumes a greater degree of consistence, and occurs in strata nearly vertical, some of which are coarser in the grain than others. These were extremely difficult to break, and presented a close smooth fracture, approaching to conchoidal.
- 8.** The Teign is the eastern boundary of Dartmoor, and, within a few hundred yards of it, and immediately beyond the stratified rock last mentioned, Granite occurs, containing very large crystals of felspar, which continues to within a short distance of Tavistock, situated on the Tavy, which bounds Dartmoor on the west side.
- Here, as on the banks of the Teign, the Killas rests upon the Granite. At Wheal Friendship, a mine at that time under the management of Mr JOHN TAYLOR, (to whose intelligence I am deeply indebted for a great share of the information I obtained
- obtained

obtained in the country), I selected the following specimens, as illustrative of the Cornish terms, which certainly afford the best explanation that can be given, of a language entirely peculiar.

9. *Killas*, by comparison with the Grauwacke Slate of Freyberg, I find this to be quite as similar as any two specimens from the same quarry could be expected to be.
10. *Elvan*, as pointed out at Wheal Friendship. This I took for coarse-grained Grauwacke; it was very difficult to break, and a very small proportion of it exposed to view. I could not, therefore, observe its connection with the surrounding rocks; but, from more minute examination, I suspect it may belong to a bed of Greenstone.
11. *Capel*, a veinstone or *Salband*, composed of Quartz penetrated by Chlorite.
12. A *Bunch* of ore, is here exhibited by a portion of Copperpyrites, in a vein of Quartz, which represents the lode. When found in this way in a mine, it is termed a Bunch of Metal.
13. A *heave* to the right, the *Killas* is here traversed in different directions by Quartz veins; that marked A represents a lode, intercepted and heaved to the right by B, a cross course. When the lode is cut in a very oblique direction, it is said to be *caunted*.
14. A *Horse*, when a lode is divided, and joins again, it is said to *take horse*, and the included mass in this specimen, is called the Horse of *Killas*, &c.
15. A *Squat*, when the lode suddenly enlarges, it is called a Squat; and the metal it contains a Squat of Ore.—By means of this *vocabulary*, I very soon became familiar with many of the commonest mining terms in the country.

In

- In order to form a junction between the Tavistock Canal and the Tamer, it became necessary to drive a tunnel, for a mile and a half, through a hill called Morwel Down, which promises to be a source of interest to the geologist. In forming this tunnel, several powerful veins of clay porphyry have been penetrated, the substance of which is in some places much disintegrated, in others firm and compact; veins supposed to correspond have since been observed on the surface.
- Nos. 16. to 19.
20. In the tunnel, the Clay Porphyry alternates several times with the Killas, which is here of a light-grey colour, and a soft friable texture.
- In the course of this undertaking, two workable metallic veins have been intersected, no traces of either had been found on the summit of the hill, although diligently examined.
21. Passing the Tamer, we enter Cornwall, and at Gunnislake, is a mine of Copper in Granite; and a little beyond, at Drake-walls, there is another of Tin, in Killas.
22. Tin-vein in Granite, from Carclaze, near St Austle.
23. Killas found on the road from St Austle to Carclaze; this I consider a very perfect specimen of *Grauwacke*.
24. Mass of white Quartz, of which the roads are formed.
25. Mixture of crystallised Quartz and Wolfram, covered with a coating of *Bitumen*, found in Poldice mine near Redruth, at the depth of 106 fathoms, in *Granite*.
26. Arsenical Pyrites, mixed with acicular, dark, greenish-grey crystals, supposed to be actinolite, from Blaney's Shaft, a branch of Wheal Unity.
27. Granite, Cairn Brae.
28. From a vein which traverses the north-east side of Cairn Brae: on the spot it appeared to me to be Clay-Porphyry, in hand specimens it resembles fine-grained Granite.

Vein-stone.

- Nos. 29. Vein-stone of Quartz, impregnated with red Oxide of Iron, and containing white Steatite, from Tincroft.
30. From the high-road near Tincroft. This is a very tough rock, and very fine-grained: it appears to be a variety of Greenstone similar to NO. 10.
31. Killas, marked with dark-coloured spots, from the side of the road to Cambourn, a little westward of the last.
32. Killas of a light-grey colour, from St Anns.
33. From Beacon Hill, a conglomerate formed of the debris of Granite, very similar to some varieties of Sandstone.
- 34, 35. Granite from the Land's End.
- 36, 37. Hornblend-Rock from Botallock, a curious, little, but valuable mine, on the north side of the Peninsula, near the extremity.
38. *Cockle*, massive Tourmaline, from the same place.
- 39 to 41. Specimens from the junction of the Granite and Killas, at St Michael's Mount.
- 42, 43. From the shore near Penzance: these I consider to be Killas of a very tough and compact variety, they are found very near Granite, or some similar rock, which presented something so peculiar in the aspect, that I cannot help recommending it to the attention of geologists.
- 46. Serpentine from the Lizard.
47. Hornblend-Rock, which forms the basis on which the lighthouses of the Lizard stand.
48. Clay-porphyry, near Trewithin.
49. Granite, with a vein of Tin, St Stephen's.
50. Conglomerate of Quartz and Granular Talc, from the same place.
- 51, 52. Phosphate of Lime and crystallised Talc, in Granular Talc, from Stoney Gwins.
53. Killas from a quarry between Bodmin, and the race-course of that town. This substance is very soft, but well adapted

for building, from the peculiar facility with which it is quarried; the stratification being horizontal, and the cross rents perpendicular, and so regular, the quarry presents a very symmetrical appearance.

- 54, 55. Granite, coarse-grained, near Bodmin.
 56. Fine-grained Killas, near Launcestown.
 57. About seven miles from Launcestown, on the road to Oakhampton, I found a quarry containing schistose and amorphous Killas interstratified, the last of which I believe to be as perfect Grauwacke as any in the district of Lammermuir.
 58. Greenstone, Hatherleigh.
 59. Variety of Trap from Cleave.
 60, 61. Killas from Ilfracombe, alluded to in page 119.

Specimens quoted, but not from the same country.

- 62, 63. Fine-grained Grauwacke, from Peeblesshire, in all respects similar to the Killas of Cornwall. Note, p. 120.
 64, 65. Shells in Killas, from Coniston. Note, p. 125.
 66, 67. Same from Fermoy, county of Cork, p. 125.
 68, 69. Gneiss from Coul, p. 129.
 70. Transition Limestone from Rae Quarry, containing shells, p. 113.

IV. *On the Revolutions of the Earth's Surface.* By Sir JAMES
HALL, *Bart.* Pr. R. S. ED. & F. R. S. LOND.

PART I.

[*Read March 16. 1812.*]

WE are never more disposed to give credit to a philosophical system, than when we meet with a case of its successful application, unknown to the author, or containing circumstances which he had not taken into account when he formed that system.

The facts brought forward in the following paper, which, according to my view of the subject, clearly indicate the operation of immense torrents, can be accounted for, I think, in a very satisfactory manner, by the Huttonian Theory, and consequently afford some very powerful arguments in support of it.

But such was by no means the view taken of this subject by Dr HUTTON himself, or by Mr PLAYFAIR, who, since his death,

has so ably illustrated and maintained his geological opinions. These gentlemen have expressed themselves, on all occasions, in a manner peculiarly hostile to the employment of such torrents as geological agents, believing that all the phenomena may be traced to the influence of diurnal causes only.

I have no hesitation in declaring my hearty concurrence in what I consider as the essence of the Huttonian Theory; I mean as to all that relates to the influence of internal heat in the formation of our rocks and mountains: But I could never help differing from Dr HUTTON, as to the particular mode in which he conceived our continents to have risen from the bottom of the sea, by a motion so gentle, as to leave no trace of the event, and so as to have had no share in producing the present state of the Earth's surface.

At an early period of life, while I imbibed from the delightful conversation of my worthy friend Dr HUTTON, the spirit of his geological views, I retained my attachment to opinions suggested by M. DE SAUSSURE's observations, which, at a still earlier period, I had acquired in the Alps, and which had been rivetted in my mind by the sight of the phenomena from which they have been inferred. The facts also observed in the Russian empire, and brought forward by Professor PALLAS, relative to the productions of the tropics, which are found upon the banks of the Frozen Sea, appeared to me of sufficient force to justify his belief, that, at some remote period, a torrent of water had swept across the continent of Asia. I have, therefore, been always disposed to combine the doctrines of HUTTON with those professed by the gentlemen just named, relative to marine inundations; and a number of facts which I have observed in this country, will contribute, I hope, to throw some additional light upon this difficult subject.

Before

Before mentioning my own observations, I beg leave to lay before the Society the state in which I found the question. The central ridge of the Alps, of which Mont Blanc is the principal mountain, consists, as is well known, of Granite. Nearly parallel to it, and at a distance of fifty-four miles to the north-west (by Sir GEORGE SHUCKBURGH'S measurement) lies the mountainous ridge of Jura, fifty or sixty miles in length, composed entirely of calcareous matter. Between these two ridges are interposed the valley and lake of Geneva, and other valleys and inferior hills. The native place of the granite is confined to the central ridge; but over the surface just described, innumerable detached granitic blocks are very irregularly scattered, which have originated, as we must presume, in that central ridge, and have been transported to their present position by some mechanical power.

Upon the southern side of the Baltic, a multitude of similar blocks are found, also scattered irregularly over a sandy district, of which a particular and detailed account is given in M. DE LUC'S *Geological Travels*, vol. i.

The origin and history of such blocks, which occur in various other places, have given rise to considerable discussion; and the question is inseparably connected with other points of magnitude in geology,—such as the formation of valleys and lakes, and the distribution and arrangement of various beds of clay and gravel, and of all the loose and alluvial assemblages which occur under various forms in all parts of the globe, and constitute its most valuable districts in point of fertility.

M. DE SAUSSURE ascribes the transportation of the granitic blocks on the Alps to the action of an immense torrent of water, or *Debaule*, as he terms it, which at some remote period, flowing over the summit of the Alps, had carried these blocks along with it.

Dr

Dr HUTTON and Mr PLAYFAIR, as I have already said, deny the necessity of introducing such an agent, since the circumstances, as they conceive, might have been produced by the usual action of rivers. But this simple view seems to be excluded, when we consider both the magnitude and the positions of these blocks. Their size, in some cases, amounts, as in the valley of Monetier upon Saleve, to 1200 cubic feet, and in the case of those on the Coteau de Boisy, to 2250, and even to 10,296 cubic feet, which is the measure of the block called Pierre à Martin.

To move a mass of granite of even fifty or sixty cubic feet, and to carry it a few yards, would require the utmost efforts of the Rhone or the Arve, as they flow near Geneva, in their highest floods, but that such blocks could be conveyed by one of them along its whole course, is contrary, I conceive, to all experience, and still more when we consider that these rivers are divided at their source from beneath the Glaciers into forty or fifty small streams. Yet from the Glaciers, these blocks must have come; and when we take into account the magnitude of some of the granitic masses, it is clear that the task is beyond the power of any river that flows on the surface of the earth; nay, it seems more than water, under any predicament, could accomplish, and more than could be expected from the *Debauche* itself, however extravagant its magnitude may appear: but we shall again return to the subject, and shew in what manner this difficulty may be explained.

These stones do not lie merely in the beds of rivers, but occur all over the country, and on the summits of mountains, where rivers could least be conceived to have flowed; nor are they confined to that side of the country, or to the side of the lake of Geneva which lies next the Alps; for we find them in particular on the face of Jura, which fronts the central ridge
and

and the lake, at an elevation of 2000 feet above the latter. A set of low hills also intervene, which occasionally hide that central ridge from the view; and it is principally where the snowy summits are visible from the face of Jura, by means of some depression in these intervening hills, that we find those traveled masses; as I remember well to have witnessed, at some of the places which SAUSSURE has pointed out, where, in high situations, on the face of Jura, I rode through great assemblages of granitic blocks, three or four feet in diameter.

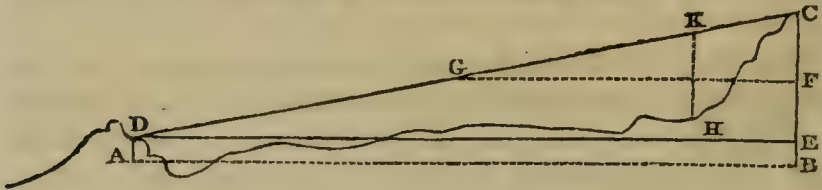
The force of this fact is admitted, but an attempt is made*, even under that admission, to refer the whole to diurnal actions, by supposing, that from the spot where these blocks lie, up to the summit of Mont Blanc, one continued solid plane has ascended, along which, on a declivity computed at one in thirty, these blocks may have been hurried by a stream of water; and that subsequently, in the course of ages, all the intervening mass had been washed away, so as to reduce the country to its present situation. But this hypothesis removes the difficulty of the intervening valleys only; for the transportation in this case would be scarcely less difficult than it would be to Geneva, as matters stand at this day;—and a circumstance occurs, founded upon the observations of Sir GEORGE SHUCKBURGH, which seems entirely to preclude this hypothesis.

According to his measurement and scale, as given in the *Philosophical Transactions*, vol. lxxvii. the height of Mont Blanc is 14,432 feet above the lake, and the distance in a straight line from Jura to Mont Blanc, I find, by his scale, to be nearly fifty-four miles, and 2000 feet is the height at which these granite blocks occur on Jura. The slope, therefore, along which these blocks must have descended, would be nearly that of one
in

* *Illustrations*, art. 345, page 385.

in twenty-three. But without inquiring whether or not this declivity would be sufficient for the purpose, it is of consequence to attend to another circumstance pointed out in these measurements. The line above which snow lies perpetually, during all summer, is there noted, and lies by the scale at 7500 feet perpendicular below the summit of Mont Blanc. It is easy, then, to calculate to what horizontal distance from the centre of the ridge, this limit of perpetual snow would extend, and we thus find it to be 32 miles *. But SAUSSURE has found the junction of the granite with the surrounding strata at the *Buet*, and by the same scale, I find the distance of this mountain from Mont Blanc is about 10 miles. It is obvious, then, that in the supposed situation of the Alps, in which the granitic

* Let AD be Jura, and BC Mont-Blanc. Let D be the place on Jura where the blocks lie; the line DC will be the outline of the supposed surface. Let AB be drawn horizontally at the level of the surface of the lake, and DE at a level 2000 feet higher, meeting BC in E. We have then $AB = DE = 54$ miles = 285120 feet; and $CE = CB - EB = 14432 - 2000 = 12432$ feet. The measure of this declivity along CD may therefore be easily obtained. Thus, $CE : ED :: 12432 \text{ feet} : 285120 :: 1 : 22.9$, nearly one in twenty-three. Mr PLAYFAIR states it at one in thirty; but he has reckoned to the summit of Jura; whereas the blocks under consideration lie at some distance below that summit.



Let CF be equal to 7500 feet, and let FG be drawn horizontally; then G will be the lowest point of perpetual snow upon the supposed surface. The horizontal distance of G from the centre will thus be obtained, $CE : CF :: ED : FG$, or $12432 : 7500 :: 54 \text{ miles} : 32\frac{1}{2} \text{ miles}$. Let H be the extreme point of the granitic mass at the *Buet*, and let HK be drawn vertically; the point K will denote the limit of the granite upon the supposed surface.

nitic mass could not be conceived to extend farther than it now does from the middle, that the whole granitic surface must not only have been buried in everlasting snow, but that the extent of this snow must have been three times that of the granite. Now there is every reason to suppose, since the temperature diminishes as the height increases, that within this upper third, the temperature has never, in the hottest day in summer, reached so high as 32° ; that is to say, that water has never there existed in a liquid form. We cannot, then, conceive any block, however small, to have been conveyed from thence by means of water, acting by the usual diurnal laws.

M. DE LUC admits the reality of M. DE SAUSSURE's debacle, and accounts for it in the same manner; but he does not ascribe to it, either in Germany or in the Alps, the transportation of the granitic blocks, the presence of which he accounts for very differently. He supposes that these blocks have nowhere migrated along the surface, but have been ejected from below, at the places where they now lie. This ejection he produces by a very extraordinary hypothesis*, founded entirely on gratuitous suppositions, and which affords no plausible solution of the difficulty, by which we might be tempted to adopt it.

M. DE LUC mentions a theory formed by Mr WREDE, a Professor of Berlin, to account for the same blocks. He embraces a belief which seems to have been in considerable favour, that the level of the Baltic Sea has been sinking for ages past, and he extends his belief of the same change taking place in the

VOL. VII. T whole

* He supposes, that below the firm crust of the earth, then lying horizontally at the bottom of the sea, a set of caverns once existed, filled with peculiar elastic fluids. That at a certain period, this crust breaking to pieces, fell with great violence into these caverns, and drove out the elastic fluids contained in them with such impetuosity, that the blocks of granite under consideration, which then constituted portions of the lower part of the crust, were forced upwards into the air, and fell back into their present position.

whole of the North Sea. His idea is, that a change has been gradually going forward in the position of the centre of gravity of the earth, which has been moving to the southward, and that an equivalent rise is experienced in the southern hemisphere. That when the North Sea stood at its highest level, these granitic blocks had been transported across the Baltic, by means of the winds, on floats of ice, and settling in their present places, had been left by the retiring waters.

M. DE LUC, in a very satisfactory manner, in my opinion, refutes one essential part of this system, by shewing, from the form of the new lands, and from other circumstances, that no change in level whatever has taken place on the surface of that sea for many ages back, and, indeed, ever since the surrounding country has possessed its present form. As to the other part of his system, which relates to the transportation of these blocks by means of ice, we shall soon have occasion to resume its consideration.

Besides the consequences resulting from these particular phenomena, there are other material branches of Dr HUTTON'S theory connected with the same views, which in my opinion require to be carefully revised. The theory which he has advanced respecting the formation of valleys, by mere diurnal actions, appears to me liable to great objections, and I cannot help agreeing with M. DE LUC, in much that he has urged against Mr PLAYFAIR on this point. I also concur with my friend Sir GEORGE MACKENZIE, in some general views, suggested to him by the aspect of certain rocks in Iceland, and in rejecting the slow operations of the atmosphere (*Travels in Iceland*, p. 39.)

The difficulty is peculiarly great where a lake occupies the bottom of the valley, and is very conspicuous in the case of the lake of Geneva lately mentioned. For, granting all that has been
been

been advanced in support of the diurnal formation of the valley in which this lake lies, granting that it has been excavated by the diurnal actions, since these granitic blocks were deposited upon Mount Jura; it still remains to be explained, how the lake of Geneva itself was formed, the depth of which amounts in some places (SAUSSURE, art. 44.) to 950 feet. Now, as has very fairly been stated, this lake is constantly receiving all the spoils of the district which lies above it, called the Val-lais, yet delivers nothing at Geneva, situated below it, but pure water. It is evident, then, that every known diurnal action tends to fill up this lake, and none to excavate it. This is therefore admitted by Mr PLAYFAIR, to be a case "in which hypothetical reasonings are warranted by the strictest rules of philosophical investigation, and where we must therefore have recourse to an agent that is invisible." *Illustrations of the Huttonian Theory*, p. 366.

This acknowledged difficulty seems calculated to entitle the following speculations to a patient hearing from this gentleman; and he has already, in another part of the same work, expressed himself with regard to them, and other kindred opinions, in a manner highly encouraging to discussion, and which affords one of the most striking examples of candour that has ever occurred.

"These arguments," Mr PLAYFAIR says, (referring to those he had just been stating) art. 367. page 412. "appear to me conclusive against the necessity of supposing the action of sudden and irregular causes on the surface of the earth. In this, however, perhaps I am deceived, neither PALLAS nor SAUSSURE, nor DOLOMIEU, nor any author who has espoused the hypothesis of such causes, has explained his notions with any precision; on the contrary, they have all spoken with such reserve and mystery, as seems to betray the weakness,

“ness, but may have concealed the strength of their cause. I have, therefore, been combating an enemy that was in some respects unknown, and I may have supposed him dislodged, only because I could not find his strong holds.”

The charge of obscurity here brought forward, is very applicable to the diluvian system of SAUSSURE and DE LUC, in what regards the origin of the torrent, or the disposal of the water after it had overwhelmed the land. The same charge seems not, however, to apply to the suggestion of Professor PALLAS, who, in an early work, entitled, *Observations sur la Formation des Montagnes*, first published in 1777, has very explicitly ascribed the inundation by which he conceives the continent of Asia to have been overwhelmed, to the action of volcanoes rising in the Indian Sea, and forming the Moluccas, the Philippines, and other islands, known or supposed to be volcanic.

It must be admitted, that such an event may occasionally have happened. But the occurrence of similar catastrophes may be inferred in a manner still more general and unequivocal, from those Plutonic* revolutions the reality of which has been established by Dr HUTTON's observations. According to his system, all our strata once lay at the bottom of the sea, and have been raised into their present situation by the subterranean and submarine exertions of heat, similar to that which appears externally in the volcanoes. And the angular fractures exhibited by these beds on many occasions, prove that this elevation was performed in such cases when the mass was in a hard state. It is obvious, then, that the same principle which I lately attempted to apply (page 87. of this volume) to the volcanic phenomena, as exhibited in the *Atrio del Cavallo*, will apply to those Plutonic revolutions; and we are authorised

* I need scarcely say, that by the term *Plutonic*, I mean to characterize that geological system in which the principal agent is heat acting under *Compression*.

sed on these principles to expect, that the liquid substances of basalt and of granite, in their progress through the rents of our strata, have been exposed to congelation, like the lava in the rents of Mount Vesuvius; that their progress has been arrested, and the protruding energy accumulated in a similar manner, till it acquired sufficient power to break through that obstacle, or through some other opposed to it; and it is manifest, however gradual and uniform the propelling force may have been previous to its accumulation, that the ultimate laceration must have been performed, by a sudden and violent motion, producing an earthquake at the surface, and thus affording a more extensive and more satisfactory solution of that tremendous Phenomenon that is furnished by the steam of Mr MITCHELL; though I am ready to admit, that in the volcanic actions, the production and condensation of steam, on many occasions, has produced very powerful effects. In the Plutonic regions, as the restraining mass was beyond all comparison stronger, and thicker and heavier, than in the most violent volcanic action, it must have exerted a power of resistance greater in the same proportion; consequently, the time of that constraint must have been of incomparably longer duration, and the violence of the shock, when the fracture did take place, though no less sudden, must have been incomparably more powerful.

Thus, as Vesuvius, in the course of the middle ages, was once at rest during several centuries, we have reason to presume that the Plutonic action, after being suspended for several thousand years*, should rush forward with a degree of violence proportioned

* This observation meets an objection urged against Dr HUTTON by M. DE BREISLAC, a gentleman to whom I feel indebted for the handsome manner in which he has mentioned the result of my experiments, and who, on most occasions, has treated our views with peculiar fairness, but who seems in this important

portioned to the time of its previous constraint, and capable of fulfilling all the conditions of SAUSSURE's *debacle*, or the wave of PALLAS; and the existence of these tremendous events is thus indicated *à priori* by the Huttonian principles.

I shall now consider, whether such traces have not been actually left by these waves, as to place the reality of their existence beyond all doubt. We have already alluded to some of these, and shall have occasion to mention some others; but in a case of this sort, no historical, nor even traditional authority need be expected; for though no limit could well be assigned to the magnitude which such a wave might actually reach, there is a decided limit to the magnitude of one that could be recorded; since, by exterminating all witnesses, every wave beyond a certain size would infallibly be the cause of its own oblivion. Those of a moderate extent are most likely to have been recorded by man, having sufficient power to produce the most awful impressions, and yet sparing a sufficient number of witnesses to transmit the event to future generations.

In our attempts to elucidate this subject, we shall refer, in the first place, to specimens of this tremendous phenomenon in still lower stages of its power, since they alone have been recorded in an authentic and intelligible form, I mean, by referring to those agitations of the sea which have accompanied most of the great Earthquakes in our own times, as we learn from the following regular statements.

In

portant article to have altogether misapprehended the system proposed. In his late work, (*Introduction à la Géologie, tr. Fr.* p. 115.) he urges as subversive of Dr HUTTON's views, some thermometrical observations in which the bottom of the sea was found to be as cold as its surface. To urge this as an objection, implies that the heat employed in this system is supposed to be for ever, and everywhere, acting upon the sand of the sea; whereas it is well known that Dr HUTTON conceived that the subterranean heat, as well as that of the volcanoes, was subject to short fits of activity, with ages of intermission.

In the *Philosophical Transactions*, vol. XLIX. p. 424. we have an account of the earthquake at Cadiz, by Mr B. BEWICKE, merchant there, 1st November 1755, in which it is said, “an hour after” (the first shock) “looking out to sea, we saw a wave coming, at eight miles distance, which was at least sixty feet higher than common. Every body began to tremble; the centinels left their posts, as well they did; it came against the west part of the town, which is very rocky; the rocks abated a great deal of its force; at last it came upon the walls, and beat in the breast-work, and carried pieces of eight or ten tons weight, forty or fifty yards from the walls,” &c.

We have another account of the same by DON ANTONIO DE ULLOA, *Phil. Trans.* vol. XLIX. p. 427. who describes the wave as having returned five times after the first*. And similar facts are stated of other scenes of this sort.

One circumstance in addition to those mentioned, which has accompanied all these great events, and which seems at first sight to contradict our explanation of them is, that in all the agitations of the water, the first event has been a retreat of the sea.

Mr

* He says, “the inhabitants had scarcely begun to recover from their first terror, when they saw themselves plunged into new alarms; at ten minutes past eleven, they saw, rolling towards the city, a tide of the sea, which passed over the parapet of sixty feet, above the ordinary level of the water; at thirty minutes past eleven came a second tide, and these two were followed by four others of the same kind, at fifty minutes past eleven; at twelve o'clock, thirty minutes; one o'clock, ten minutes; one o'clock, fifty minutes. The tides continued, with some intervals, till the evening, but lessening. They have ruined one hundred toises of the rampart, part of which, of three toises length, and of their whole thickness, were carried by the torrent fifty paces.” It is remarkable, that in these six waves, the two first intervals of time are exactly of twenty minutes each, and the three others of forty. It is probable, that they have all resulted from one great impulse.

Mr MITCHELL says, *Philosophical Transactions*, vol. LI. p. 566. 611. speaking of the earthquake at Lisbon in 1755, “ the bar (at the mouth of the Tagus) was seen dry “ from shore to shore, then suddenly the sea, like a “ mountain, came rolling in. This phenomenon accom- “ panied the same earthquake at the island of Madeira, “ where we are told, that at the city of Funchal, the “ sea, which was quite calm, was observed to retire sud- “ denly some paces, then rising with a great swell, without the “ least noise, and as suddenly advancing, entered the city*.” He further states, in the same page, that “ in the northern “ part of the island the inundation was more violent, the sea “ retiring there above one hundred paces at first, and sudden- “ ly returning, overflowed the shore, forcing open doors,” &c. Again, in page 466. he says, “ The great earthquake that de- “ stroyed Lima and Callao in 1746, seems also to have come “ from the sea; for several of the ports on the coast were “ overwhelmed by a great wave, which did not arrive till four “ or five minutes after the earthquake began, and which was “ preceded by a retreat of the waters, as well as that at Lis- “ bon.”

Looking into the original account of these calamities in DON ANTONIO DE ULLOA'S Travels into South America, French edition, vol. i. p. 467. I find that he mentions two events of this sort, which took place at Callao; one in 1687, in which the sea first retreated, and then returned with such force as to overwhelm Callao and other places. And again in 1746, when, in the course of twenty-four hours, two hundred shocks were felt: on this occasion, the sea retiring as it had done formerly, on similar occasions, returned furiously, and overwhelmed Cal-
lao

* *Philosophical Transactions*, vol. XLIX. p. 433. The retreat is here said to have begun an hour and a half after the shock.

lao so completely, that nothing remained of the whole town but a remnant of Fort Santa Cruz. Out of twenty-three vessels at anchor in the harbour, nineteen were sunk, and the four others, one of which was a frigate, were carried to a great distance on shore, and left on dry land. At Callao, out of four thousand inhabitants, only two hundred were saved.

Mr MITCHELL endeavours to account for this, by his favourite doctrine of steam, according to which he conceives, that the strata at the bottom of the sea have been raised as a roof; he then continues, (p. 613.) “ now while the roof is raising, the
 “ waters of the ocean, over it, retreat, and flow from thence
 “ every way; this, however, being brought about slowly, they
 “ will have time to retreat so gently, as to occasion no great
 “ disturbance; but as soon as some part of the roof falls in, the
 “ cold water contained in its fissures mixing with the steam,
 “ will immediately produce a vacuum, in the same manner as
 “ the water injected into the cylinder of a steam-engine, and
 “ the earth subsiding, and leaving a hollow place above, the
 “ waters will flow every way towards it, and cause a retreat of
 “ the sea on all the shores round about.” But the gradual elevation of the roof, keeping tight all the while, which is here assumed as the steam was collecting, can by no means be admitted; since, as we have shewn, a mass such as this, when forced upwards, would have yielded suddenly.

This phenomenon, however, along with all the rest mentioned, may, I conceive, be traced as consequences of the simple, but rapid, elevation of the bottom, which we have ascribed to the forcible introduction from below of stone in liquid fusion. Water being almost incompressible, or elastic in an extreme degree, it is obvious, that an elevation of that portion of the fluid which lay immediately upon the part of the bottom raised, would take place almost simultaneously. This

elevation would of course be the greatest in the middle, the quantity of matter in the vertical line being the least; but there is no doubt that an action of the same kind, diminishing in a high ratio, as the distance from the centre increased, would be extended all round; the surface of the fluid being thrown into the form of some species of conchoid, produced by the revolution round the vertical axis cb of the line dce , (Plate VI. fig. 1.) the highest point of which is at c , with a contrary flexure at f and h , and going off beyond these points, in such a manner as to have the original level surface of the water dbc for an asymptote. This conchoidal elevation, produced in the first moment, being just equal in bulk to the quantity of water displaced by the solid mass (aa) elevated at the bottom.

If this mass stopped suddenly, as would naturally happen, when, in consequence of the rupture, the protruding liquid stone has found a vent; it is evident, that every part of the water put in motion during the first moment, would tend, according to the first law of motion, still to continue moving during the next and following moments; but this tendency could not be everywhere effectual; because, if all the water that rose in the first moment were to rise also in the second, a vacuum would be the consequence. In the second moment, then, a struggle must take place between the different portions of the fluid; the middle portion, which had acquired the greatest velocity, would continue to flow upwards, and its place not being supplied from below, as, during the first moment, a suction would be exerted downwards, upon all the surrounding fluid. The consequence must be, that the rise which each particle would have performed by its separate momentum, during this second moment, would be counteracted by the pressure of the atmosphere, and entirely done away at a certain distance from the centre, as at f and h , fig. 2.; beyond that limit the water would
sink

sink below the medium level, as at *k* and *i*. An annular depression would thus be produced, surrounding the central elevation, vertical sections of which are exhibited by *dkf* and *hie*; that depression below the medium level being exactly equal in bulk to the additional elevation at the centre, occasioned by the momentum, the vertical section of which is *fghc*. The middle elevation, and the annular depression, would continue to increase during a period, and to an amount regulated by that class of laws, which determines the magnitude and duration of waves, as relative to the wind which produces them; and the water at the end of that period would be thrown into the position denoted by the dotted line in fig. 2., and by the single continued line in fig. 3., indicated in both figures by the letters *dkfghie*.

During a third period, the water flowing from without the annular depression, to fill it up, a wave of depression would be propagated outwards in all directions, and would visibly reach to places where the elevation produced by the direct impulse in the first moment, had not been at all perceptible, in consequence of their distance from the centre. It is owing to this cause, I am convinced, that the catastrophes at Lisbon, at Cadiz, at Madeira, and at Lima, in so far as the position of the surface of the sea was concerned, were all first announced by a retreat of the sea, as shewn at *m*, in fig. 4.

During another successive period, the undulatory motion continuing, the wave of elevation would advance towards the shore at *m*, and being met by the retiring water, would produce the curling form called the breaking of a wave, as shewn at *m* in figure 5. At that instant the calamitous effects described in all the great earthquakes, as resulting from the sea, have been produced.

I have made a few experiments lately with explosions of some pounds of gunpowder under water, in order to try the effect of sudden impulse. In every case, a very manifest heave of the surface was produced at the instant of the explosion; and at that same instant, a very smart percussion was felt. This was always followed in two or three seconds by a distinct and separate agitation of the water, occasioned by the rising of the permanently elastic gases produced in the explosion. The form given to the wave in fig. 1. has been sketched from the recollection of that first momentary impulse.

We have thus a theory of those waves which seem almost invariably to have accompanied the great earthquakes; supposing these to have been occasioned by submarine elevations, and to form an intermediate step between the Volcanic eruptions, and the truly Plutonic operations by which our continents have been elevated.

But, to return to the alpine scenes with which we set out: If the phenomena on the banks of the lake of Geneva, to which we have alluded, were really occasioned by a torrent of water, its magnitude must have been such as to leave few vestiges of the human race, in Europe at least, to record the circumstance; and we can only expect to find proofs of its reality in geological facts. The events of Lisbon and of Callao, though on a scale comparatively diminutive, help to lead our imagination to the conception of this colossal disaster. It may still, however, be alleged, as I have already hinted, that it would be impossible for water of any depth whatever, or moving with any velocity, to carry blocks of such magnitude to such situations; and the consideration is of so great importance, that I am induced, in attempting to unite the ideas of SAUSSURE with those of HUTTON, to retain part of the system
proposed

proposed by M. WREDE, in so far as to consider the granitic blocks as having been made to float, by means of a mass of ice attached to each.

A circumstance happened lately in this country which tends to encourage that idea. Upon the coast of the Murray Frith, a large block of stone, four or five feet in diameter, lying within high-water mark, and well known as having served to denote the boundary between two estates, was, during a stormy night in winter, transported to the distance of ninety yards, and the persons upon the spot were convinced, that this migration was performed by means of a large cake of ice, formed round the stone, and attached to it; and that the whole had been lifted and carried forward by the rising tide. The course of this stone was marked upon the sand below, by a deep and broad furrow, which remained visible for a long time afterwards, as I have been informed by several members of this Society, who saw it after an interval of more than a year.

By supposing that an agent of this sort acted in concert with our diluvian wave, the difficulty with regard to the transportation of the granitic blocks, seems to be in a great measure removed; and this explanation happens to be peculiarly applicable to the case in point; the native place of these blocks being covered profusely with ice, in the state best calculated for fulfilling the office here assigned to it. M. DE SAUSSURE has given the following clear account of the formation of the Glaciers. In any region whose temperature is such that the heat of summer is not sufficiently powerful to melt all the snow of winter, the remaining snow, which, by partial thawing, and subsequent freezing, is turned into ice, must accumulate year after year to an unbounded degree, so that the mass must become at last so lofty, as to be unable to bear its own weight, and must produce a motion downwards, which will be greatly assisted.

assisted by fractures, and by the sliding of the ice upon the ground. In this manner the glaciers are produced, which consist of an assemblage of great blocks of ice, each as big as a house, which, sliding and rolling upon each other during summer, make their way from the high frozen valleys where they were formed, through narrow glens, into the rich and warm vales below, in which they remain a considerable time, presenting a curious spectacle among the meadows and woods, some of which they overtop. These masses, however, melt at last, in this new situation, and leave vast assemblages of stones, which had been attached to them, forming a ridge called the *Marene* of the Glacier.

It is obvious, then, that a wave washing over these high alpine valleys in summer, would float and carry off all the ice in the glaciers, and accumulated in the higher valleys, and, along with the ice, all the blocks of stone imbedded in it, or attached to it in any way. The stream, with this load, would find its way through every opening, and would in a particular manner flow through those depressions, which at this day, as we have said, afford a view of the snowy summit of Mont Blanc, from certain places on the face of Jura where these blocks abound.

The enormous masses already mentioned, which are found near Geneva, and at the Coteau de Boisy, may now be accounted for ; and the same system will apply also to the blocks upon the Baltic, which may have been brought to their present place, not by a permanent and steady position of the ocean, varying by slow degrees, as has been alleged by M. WREDE, but by a sudden diluvian wave washing over some district, situated either at a sufficiently high level, or near enough to the pole to be the seat of glaciers. I am not at present acquainted with any facts by which the native place of these
blocks

blocks can be traced; but I trust we shall not long remain in that state of uncertainty, since there are means by which that point will be found of very easy decision, as I shall endeavour to shew in a subsequent part of this paper, by examples in the neighbourhood of Edinburgh.

It is well known, that granite is found native in Sweden; so these blocks may have been carried across the Baltic, as those of Mont Blanc have crossed the valley of Geneva. It is possible also, that they may have been transported by the help of ice from the Alps across Germany, by the very same torrent we have been considering, and which had left a portion of its load behind it on Mount Jura.

This view would afford a natural account of the production of Holland, and of a great part of that quarter of Europe, which consists entirely of sand, and whose magnitude appears to me very far to surpass any deposition that could reasonably be ascribed to the present rivers. All this sand may be conceived to have been hurried along by that mighty stream, and deposited when the torrent began to spread, and lose its force by diffusion. It is likely, too, that an immense quantity of this sand would be carried far into the ocean, and its deposition being there modified in various ways, by local tides and currents, might assume the character of horizontal strata, so as to lay the foundation for future productions of freestone or of killas. We might thus, by the help of this diluvian agent, complete the great circle of events, so elegantly pointed out by Dr HUTTON, but which the diurnal agents seem quite insufficient to fulfil.

As the sand was depositing upon the low countries, the blocks of granite, with their accompanying ice, from whatever quarter they originated, would still keep floating, and thus account for a striking fact stated by M. DE LUC. He observes, that

that the granitic blocks lying in the district between Berlin and the Baltic Sea, occur frequently, and almost constantly, in very numerous assemblages, upon the summits of the sandy hills with which that country is interspersed, whilst none are to be met with in the intervening valleys. That they also abound on the islands of the Baltic; and these blocks shew themselves upon the beach only in those places where the sea reaches the base of some of the hills on which they lie.

The present theory affords an easy solution of this fact. In the descending stream, these hills, constituting shallow sandbanks, would afford the first resting place for the floating blocks of ice, which, grounding upon them, would accumulate to very numerous assemblages, and there deposit their granitic charge, while all the other similar blocks flowed onwards, in the deep water between*.

We are thus enabled to give a tolerable account of the granite blocks; but the formation of valleys, and the excavation of lakes, in particular of the lake of Geneva, remain to be explained.

* This aid, by which our diluvian wave is so much assisted, can be of no such service to M. DE SAUSSURE's hypothesis, as he has originally framed it. He conceives (art. 210, p. 141. vol. i.) that, at a period when the mountains were covered with water, the crust was broken which defended certain caverns then void, and the waters rushing into them with violence, left the mountains in their present state, and, by their retreat, produced the diluvian torrent. It is plain that such a torrent could derive no assistance from the blocks of ice. For when this water, in its previously stagnated state, touched the solid earth, if any blocks of ice were there, they would adhere to that solid mass, and would not be carried off by the retiring waters; and where this water lay deep upon the solid mass, its ice, if it had any, must have been floating at the surface, and could not be attached to blocks of stone, lying of course at the bottom.

It is obvious, from the mode in which this topic is introduced by M. DE SAUSSURE, (which he does, as he expressly says, to meet the impatience of his reader,) that he is not satisfied with it himself, nor can it bear the slightest examination.

plained. I trust, however, that the same principles will extricate us from this difficulty also.

If my view is correct, as to the violent manner in which our continents have risen from the bottom of the sea, to their utmost elevation in the atmosphere, it is quite obvious, that the cold and hard external crust, while it communicated such shocks to the ocean, must itself have undergone the greatest agitation, and must have been rent and broken in every conceivable mode. The stone, in liquid fusion, introduced into the rents, would assist the elevation, in so far as it tended to facilitate the shifts, by enabling one mass to slide on the other; but forcing its way upwards, this liquid would at last reach a temperature, in which, as we have said, it would congeal; its further progress in that direction, with respect to the neighbouring substance, being thus effectually stopt, and the propelling force from below continuing to act, the local elevation would be converted into a more general one, either where the strata were in a flexible state, by means of an horizontal thrust, producing an elevation along with the convolution of the strata, (in a manner lately pointed out in this Society), or, where the neighbouring substances were inflexible, by a penetration of the liquid through the mass, giving rise also to a vertical heave of the whole. In either case, a number of rents would be formed, in the hard outer crust, which would widen upwards as the heave advanced*, thus forming the rudiments of valleys

Vol. VII.

X.

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* This elevation may be illustrated, by the familiar example of what happens in the act of digging a piece of firm soil. The gardener first thrusts his spade into the unbroken ground, then leaning on the handle, gradually forces the flat iron upwards; by this means, a heave of the soil takes place; the middle part being raised, while the sides remain more or less attached to the firm soil. A separation between the two is the consequence; and a formation of rents open upwards, which gradually become wider and wider as the spade rises.

of every degree of wideness, from the narrowest ravine to the most open vale.

All this progress, or at least great part of it, being performed by successive starts, each of which, while under water, producing a tremendous wave; the mass, in rising, would be repeatedly exposed to these diluvian torrents, some formed by itself, and some by neighbouring elevations, so as at last, after long exposure to agents, partly diluvian and partly diurnal, it would arrive at its present situation and condition.

In consequence of the diversity of these elevations in place and in magnitude, acting upon substances of every sort, an endless diversity of effect would be the result. In some cases a rent, in consequence of the locality of the heave, would be rendered both large and deep in the middle, while it remained nearly closed at both ends. Water, therefore, could not flow through it without stagnating by the way, and thus a lake would be formed, the depth of which might be very great in the middle, though its extremities were shallow. This applies to the formation of the lake of Geneva, either before or since the passage of the granitic blocks. It is applicable also to all lakes which occur in alpine or rocky districts. It will appear in what follows, that an account no less satisfactory may be given on other principles of those which belong to alluvial countries.

An example of the relative changes among rocks, produced by motions of this sort, occurs in the mass on the coast of Berwickshire, with which we have been so much occupied in a former meeting, as affording a display of the convolutions of the strata. Upon this coast, the killas and sandstone meet on the East near Eyemouth, and on the West in the parish of Cockburnspath, at the Siccar Point, where the junction is beautifuly

ly displayed. In the neighbourhood of both junctions, the sandstone strata, laid open by the shore, present to view a striking picture of former revolutions, as I have endeavoured to represent by two sketches taken upon the spot. One is at the Hal-lahole near the Press, between Berwick and Eyemouth (Plate VII.) on the east; and the other at the Coveshore (Plate VIII) in the parish of Cockburnspath on the west.

In both, the sandstone, remote from the junction, is seen in an horizontal position, which seems to be its general character, and which upon the West it maintains to a great extent; but in the neighbourhood of the junction, the beds become absolutely perpendicular, in a rock which, in both cases, rises to the height of thirty or forty feet. This change of position takes place differently in the two cases. That upon the East is sudden and immediate, the whole change being performed in a single cliff, at the bottom of which the beds are horizontal, and at its summit vertical, nay, leaning over upon themselves; on the other it is gradual, the strata being seen in succession to pass through every intermediate declivity, the change occupying more than a mile of coast.

This local elevation seems unequivocally to denote a great perpendicular shift. Either the sandstone mass has sunk and left the killas behind it, or the killas has risen from below, leaving the sandstone, and dragging its contiguous beds partly up along with it. As we know that the whole once lay deep below the sea, and that it is now raised, it seems more simple to ascribe these changes to one action, and to believe that the last supposition is the true one.

A fact which I witnessed in Calabria, not long after it occurred, belongs to the same class, and may bear a similar interpretation. What is called the Plain of Calabria is an alluvial
X 2. district,

district, situated along the western coast, behind which there rises a ridge of primitive mountains. When the terrible earthquake of 1783 took place, a very extraordinary circumstance happened, the remains of which I went to see in 1785, and found it corresponding to DOLOMIEU's description. A naked scar of red earth, facing the plain, made its appearance upon the primitive mass, all along the line of junction between the two; this scar being ten or fifteen feet high, and appearing almost universally in a tract of many miles. DOLOMIEU has given an ingenious theory of this phenomenon, which appeared to me satisfactory at the time. He supposes, that in consequence of the agitation of the alluvial mass by the earthquake, it had undergone a subsidence, or *tassement* (as it is expressed in French), similar to what happens when a bushel of wheat is struck by the hand; and that in consequence of the subsidence of the loose country, the firm country had been left behind. I am now, however, inclined to suggest a different explanation of the same fact, and to connect it, by analogy, with some of those lately mentioned, by conceiving that the middle, or rocky mass, may have been raised, leaving the alluvial mass behind it, and that its motion has been the proximate cause of the earthquake. It may further be observed, in favour of this view, that if the alluvial mass did subside in the manner pointed out by DOLOMIEU, the districts in the neighbourhood of the sea must have been submerged. It would have been easy, if such a conjecture had occurred on the spot, to bring it to the test of observation; for the opposite side of the coast of the peninsula must have furnished some facts either in confirmation or refutation of the idea. I mention it here, as a hint to future travellers, in tracing the result of earthquakes.

I cannot too strongly recommend to geological travellers, the examination of those scenes where earthquakes, and their attendant

tendant waves, have exerted their influence, and which have probably left behind them very striking monuments of their power. I conceive that in this way, the doctrines already advanced in this paper, and which I am now about more fully to illustrate, will be submitted to the test of direct observation.

The fact which I have met with most strictly in point, occurs in HUMBOLDT'S Account of Mexico, *Eng. Trans.* London, 1811, vol. ii. p. 212. At San Pedro de Jerullo, "in the month of June 1759, a subterraneous noise was heard. Hollow noises of a most alarming nature (*bramidos*) were accompanied by frequent earthquakes, which succeeded one another from fifty to sixty days, to the great consternation of the inhabitants of the Hacienda. From the beginning of September, every thing seemed to announce the complete re-establishment of tranquillity, when, in the night between the 28th and 29th, the horrible subterraneous noise recommenced. The affrighted inhabitants fled to the mountains of Aquasarco. A tract of ground, from three to four square miles in extent, which goes by the name of the Malpays, rose up in the shape of a bladder. The bounds of this convulsion are still distinguishable in the fractured strata. The Malpays near its edges is only twelve metres (thirty-nine feet) above the old level of the plain, called Playas de Jerullo; but the convexity of the ground thus thrown up, increases progressively towards the centre, to an elevation of one hundred and sixty metres (five hundred and twenty-four feet)."

In this most striking and interesting scene, we have an actual specimen of those violent and sudden operations by which our continents have been raised to their present position, according

ording to my view of Dr HUTTON'S Theory ; and had this event come to his knowledge, which happened in his day, it might have induced him to admit the probability of those sudden elevations, indicated by so many facts.

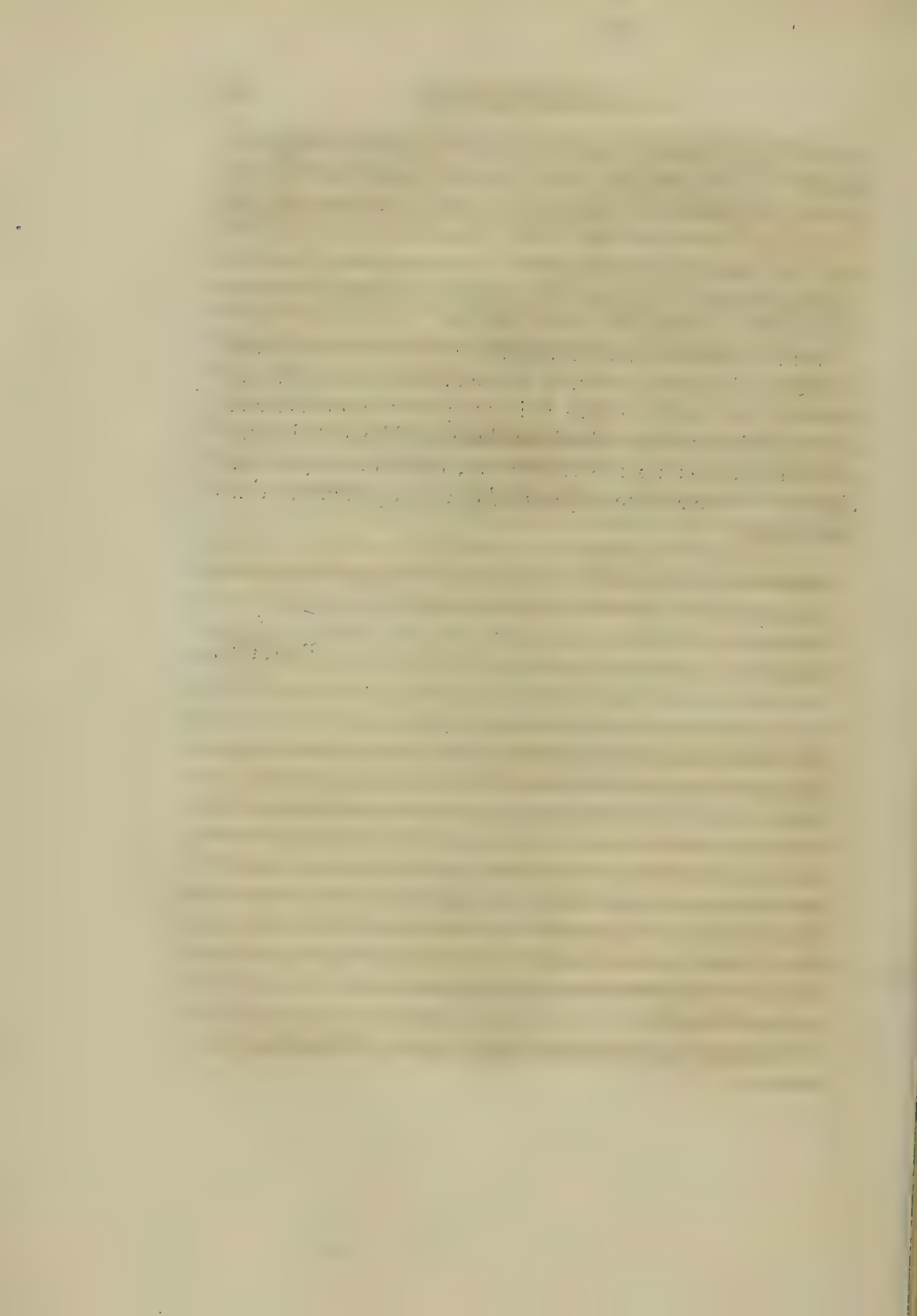
The process of elevation, whether performed gently or rapidly, is free from a difficulty to which the systems of both SAUSSURE and WERNER are exposed : both of these geologists conceive that our continents and rocky districts were once covered with water, which has since flowed away, these rocks maintaining their original position ; now, to lay a rock bare in this manner, we must dispose not only of the water which covered the immediate mass of rock, but also of that body of the same fluid which occupied an equal level all over the globe. This difficulty was strongly felt by Professor PALLAS, who says, (*Observations sur la Formation des Montagnes*, p. 79.), that were the summits of the mountains supposed to have been covered, the mass of water required to equal and surmount them round all the globe could not be disposed of within the earth, even were its inside made up of caverns : On that account, he denies that the summit of the hills has been covered. He burdens himself, however, with a very considerable share of the same difficulty, by supposing that the sea had stood at such a level as to submerge hills of 100 fathoms high.

According to our theory, there is no such embarrassment. We suppose these low hills, as well as the high ones, to have been raised from the bottom of the sea, which need not be considered as ever having stood above its present level. And I think myself authorised by the facts stated in the course of this paper, in deviating so far from the Huttonian hypothesis, as to believe that the elevation of the land was performed

formed by successive starts, similar to volcanic eruptions, though far more rare and more powerful; and that the percussions impressed by these starts upon the waters of the ocean, were such as to form waves, sometimes of a moderate force, as those at Lisbon or Callao; sometimes of overwhelming magnitude, and capable of producing the effects described in the Alps, in Germany, and in Russia.

As the inferences derived from these distant facts are called in question by some gentlemen of the highest authority in this Society, I am happy to have it in my power to produce a set of observations made in this immediate neighbourhood, which seem in a manner no less satisfactory, to lead to similar conclusions. These are the subject of the second part of this communication.

PART II.



V. *On the Revolutions of the Earth's Surface.* By SIR JAMES
HALL, *Bart.* Pr. R. S. ED. & F. R. S. LOND.

PART II.

BEING AN ACCOUNT OF THE DILUVIAN FACTS IN THE NEIGHBOUR-
HOOD OF EDINBURGH.

[*Read June 8. 1812.*]

IF such tremendous agents did in reality exert their influence in the Alps, it is not conceivable that other countries, situated lower, and composed of materials much more frail, could have been spared. We hear in fact of similar traces, more or less distinct, in all quarters of the globe. From the bones of animals, natives of the hottest climates, which are found over a frozen region of vast extent in the Russian empire, PALLAS* concludes, that an enormous torrent had transported their carcasses across the ridge of Tartary.

VOL. VII.

Y

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* Observations sur la Formation des Montagnes, p. 71. Nov. Com. Petr. tom. xvii. p. 576.

On my return from the Alps, looking for traces of the same agents in this country, I found them in abundance, particularly in the neighbourhood of Edinburgh; and the circumstances of this sort which I have met with, both on a large scale and in detail, seem to afford more precise information as to these events than has hitherto been furnished by the alpine phenomena.

In order to acquire principles upon which these observations may be made to advantage, and by which the truth or falsehood of the systems to which they lead may be brought to a certain test, it is of importance to examine the results of similar actions, in such analogous cases as lie in all respects within reach of observation. If a torrent, like that which is supposed to have inundated the Alps, had flowed over this country, it must have left behind it traces of various sorts, resembling in some degree those that occur in the course of any common river which has recently overflowed its banks. Thus in both cases, sand and mud, and loose stones, must have been transported and deposited; and fixed objects must have been overwhelmed and abraded, by the action of the water and of these moving bodies: The relation of the stream to these objects, either met or transported, is very different, it is true, in the two cases; but a sufficient agreement exists between them to guide us on the present occasion.

It is an undoubted truth, that where an obstacle occurs in the course of a fluid, which is in the act of transporting and depositing heavy substances, the deposition in the neighbourhood of that obstacle undergoes considerable modification. The theory of these modifications would be difficult to determine; but their effects may easily be traced, by observation in the bed of any stream after a flood, or in the drifting of snow, fallen during a high wind, or which is in the act of falling.

Where

Where the obstacle is high and narrow ; where its height, for instance, is at least equal to its breadth, we perceive that a stagnation of the fluid takes place on the side towards which the stream is flowing, and a deposition is formed of the transported substances, constituting a tail or prolongation, which extends in the direction of the stream, by a gradual descent to the distance frequently of eight or ten times the height of the obstacle. At the same time, an acceleration is occasioned at certain places in the neighbourhood, by which the general deposition, which was going on at the time, and which would have been universal had no interruption occurred, is there prevented. This acceleration takes place along that face of the obstacle which fronts the stream, along both its sides, and along those of the tail, forming by the stagnation just mentioned: the consequence is the production of a hollow or depression in all those places, below the level of the general deposition in the neighbourhood. The case is different where the obstacle is of great breadth compared with its height. When its breadth, for example, is five or six times greater than its height, the effect is often reversed, and an excavation is occasioned on the side towards which the stream is flowing.

The action of a current of water upon sand, or of wind upon snow, previously deposited, produces effects similar to those just pointed out. Thus, a firm body, occurring in the midst of such an assemblage, mitigates or prevents the action behind the obstacle, that is, on the side towards which the current is flowing, and, at the same time, augments the corrosive energy in front of it, and along the two sides. Effects are thus produced similar to those in the case last mentioned ; and it may be difficult in many cases to determine whether a particular assemblage has been produced, by a modification of the first deposition, or by a second action upon an assemblage previously

viously formed. The action of a current, with the assistance of the solid masses of every size transported by it, upon the substance of any solid rock opposed to it, is subject to certain laws, the principles of which must, in many respects, be common to it in the two cases just mentioned, as we shall have occasion to point out more fully in the course of this paper.

In attempting to apply these principles to the great scale of geology, and to vindicate my opinions on this curious subject, I shall appeal to a series of facts which are very accessible to this assembly, the greatest part of them lying within two or three miles of this city. In that view, I have given, along with this paper, a plan, on actual survey, taken on this account, of a small district in the neighbourhood of Edinburgh, comprehending the Corstorphine Hill and its immediate neighbourhood. I thus hope to indicate the place of each specimen alluded to, in such a manner, that, provided the rock remains in existence, it may be in the power of an observer to discover it at any future period, however much it may have been concealed by the accidents to which such specimens are perpetually exposed.

The country in the neighbourhood of Edinburgh is what all are agreed to call Secondary, consisting of beds of sandstone, and occasionally of limestone and coal, interstratified with thick assemblages of shale, in loose and frail strata. This mass is traversed with the utmost irregularity, by dikes or veins of whinstone, which occur also in vast interjected masses, sometimes lying in great amorphous blocks, and sometimes in thick beds, parallel to the strata. The strata, themselves, as might be expected, are thrown, by means of its intrusion, into much irregularity, and though nearly parallel to each other in any particular spot, exhibit the utmost variety, when different places are compared together, as to their dip and direction. This
contrast

contrast is conspicuous in Salisbury Craig on the east, and in Corstorphine Hill on the west of Edinburgh. Each consists of a thick mass of whinstone, parallel to the strata beneath it, which, in the first mentioned hill, dip rapidly to the east, and in the second to the west.

The surface of this district, together with the alluvial part of its mass, bears every mark of the effects which a wave of sufficient magnitude to overwhelm it, might be expected to occasion upon so multifarious an assemblage.

Raised from below by the violent and abrupt means already alluded to, in my last communication to the Society, this district would present to a stream overwhelming it at any subsequent period, numberless points of attack. Many of the rocks being rent in various ways, the hardest parts being in a shivered state, would easily be carried forward. The soft beds of shale or slate-clay being laid open to the attacks of the current, would be deeply abraded by its action, and thus masses, both stratified and unstratified, that were originally unbroken, would be undermined, and, yielding to their own weight, would add to the quantity of moving matter, and extend the field of attack upon the weaker parts. The water would thus be loaded with a multitude of blocks of every size, shape, and quality, and with a quantity of clay, which being soon reduced to mud, through which these stones were irregularly and confusedly scattered, would flow at the bottom of the water, and along with it, and would be deposited, according to the laws already pointed out, when the stream approached to a state of rest. Such seems to have been the origin of that body of compact blue clay which forms a material part of our low districts, bearing every indication of having flowed as a mass into its present situation; for it is totally devoid of stratification, though frequently of great thickness.

ness. This mass shews itself in several places, in the bed of the water of Leith, where the banks have been laid open by natural or artificial means. It was well displayed formerly, and may still be seen above the Bells' Mills Quarry, and is now exposed to view upon the right hand, after crossing the Dean Bridge, on the old road to Queensferry. It here presents to view a face of about twenty or thirty feet deep, though it often extends to forty or fifty feet. We find it also upon the shore to the west of Leith, as laid open by the sea; and I am informed by the person who conducted the work, that at the Fort in that neighbourhood, in a search for water, it has been penetrated to the depth of eighty feet from the surface, being fifty below high-water mark. It is obvious, that the power of such mud, when flowing as a stream, in transporting heavy bodies, and in abrading assemblages previously formed, must bear some relation to the resistance which it would oppose to any object forced through it, and of course, that its power in these respects must have been much superior to that of pure water. I conceive also, that a deep stream must exert a greater power of transportation and corrosion than one which is shallow, flowing with the same velocity.

The existence of assemblages of this sort, affords, by its simple testimony, a powerful argument in favour of a stream having overflowed this country, superior in magnitude to any known river; and the facts seem to meet the challenge held out by Mr PLAYFAIR in the following passage, *Illustrations*, art. 366. "Lastly," he says, "if there were anywhere a hill, " or any large mass composed of broken and shapeless stones, " thrown together like rubbish, and neither worked into gravel, nor disposed with any regularity, we must ascribe it to " some other cause than the ordinary detritus and wasting of " the land. This, however, has never yet occurred, and it
" seems

“ seems best to wait till the phenomenon is observed before we seek for the explanation of it.”

Now it appears to me, that these vast assemblages, in which blocks of every size, and shape, and quality, some sharp, some round, are confusedly scattered through clay, are inexplicable by any diurnal cause, and do call for some particular solution.

Such parts of the torrent as encountered less of the strata of shale and clay, would hurry along with them the comminuted sandstone, and deposit it in the form of sand and of gravel. Vast accumulations have thus been formed in all our lower districts, the external figure of which, and of the clay, has acquired, as we shall soon have occasion to point out, a character peculiar to itself, and having externally a smoothness and regularity, which forms a striking contrast with the abrupt and most irregular dislocation which very commonly occurs in the solid mass within.

In the midst of this general wreck of all the frail parts, the strongest masses, principally those which, like Arthur's Seat, have been powerfully pervaded with whinstone, would resist and defy all the impetuosity of the stream. The principles which we have endeavoured to lay down, as to the influence of firm obstacles on depositions and abrasions, would thus be brought into action.

The rock upon which the Castle of Edinburgh stands, together with the site of the Old Town, exhibit the most perfect example that could be conceived of the application of these principles. The rock itself, about two hundred feet in height, above its base, and bare on three-fourths of its circumference, consists of one of the most complete and uniform masses of whinstone that is known in this country. Its form is rudely cylindrical, and from it the ridge upon which the Old Town stands,

stands, composed partly of deposites, and partly of protected strata, extends, gently sloping, for about a mile to the eastward, from the Castle to the Abbey of Holyroodhouse, where the tail terminates. Round the western, southern, and northern sides, a hollow valley occurs, which, towards the north, is still a marsh, and was once a lake, being known by the name of the North Loch.

Corstorphine Hill, which, as seen from Edinburgh, occupies the horizon to the north-west, affords, in one respect, an example of the other case just mentioned. It consists of a ridge of about a mile and a half in extent, rising in the middle, declining gently at both ends, and pointing from north to south, with a declination of about 20° to the east. It presents a smooth face of whinstone to the west, towards which the mass dips in parallelism with the strata beneath it. Upon its eastern side a hollow valley occurs, in which the old castle of Craigmuck stands, and from its southern extremity a tail extends to the eastward, lying between Ravelstone and Murrayfield, upon the southern face of which Murrayfield stands.

Thus, the Castle of Edinburgh, gives an example of the effect of a narrow obstacle; and this hill, of a broad one, in so far as it has a valley on the side towards which the stream was flowing.

We have endeavoured, in the last communication to this Society, to account for the formation of such lakes as occur in alpine and rocky districts. The circumstances just pointed out, explain the formation of those which belong to districts formed of frail and moveable substances. At Lochend a striking example occurs, of a lake produced upon the upper side of an obstacle, in consequence of local acceleration.

Immediately on the east of Corstorphine Hill, a set of firm rocks, or little hills of sandstone occur, rising up from this hollow,

low, or standing upon its eastern side. Of these, Ravelstone, Craigleith, and Blackcraig, are the principal, well known as excellent quarries. From each of them a tail or prolongation extends to the eastward, formed chiefly of the blue clay already mentioned, together with beds of sand and gravel. These decline very gently, and maintain, to a considerable distance, the individual character given to each by the firm mass producing it. These ridges, however, are occasionally interrupted by the interference of the same principles; as we see well illustrated near the rock of Craigleith Quarry, by which the tail extending from the Maiden Craig, (another sandstone mass to the westward of it,) is abruptly cut off.

From Corstorphine Hill to the eastward, the country embracing all the space between Edinburgh and the sea presents one continued series of ridges, upon one of which the New Town of Edinburgh stands. It is an important circumstance, that these ridges maintain a very correct parallelism with each other, with the tail of the Castle rock, and of the Calton Hill, and with the alluvial prolongations that extend to the eastward from all the eminences of this neighbourhood. And a series of parallel ridges occur also on the south side of Edinburgh, extending from all the rocky eminences, as may be well seen on the road leading to Dalkeith, which passes over several of these; one of the most remarkable of which is, that on which the village of Libberton stands.

Such an arrangement cannot have been the work of the diurnal waters produced by our common rains; for the course of such waters, flowing by the action of gravity, and guided by the general slope of the country, which declines towards the Frith of Forth, ought to have produced depositions nearly at right angles to those under consideration. It is in vain that a vast duration is ascribed to the influ-

ence of an agent, unless it can be shewn, that its action has a tendency to produce the alleged result. If it has a tendency to produce a different result, that difference would be augmented in proportion to the duration of the action. Now, the diurnal operations are everywhere found in the act of corroding and altering the forms here alluded to *; but they are nowhere seen to produce them. This class of facts, on the other hand, all conspire in giving probability to the hypothesis of a diluvian wave, which affords an easy explanation of all the large features of this country.

An important principle of the theory of running streams must here also be considered, namely, that the shape assumed by such a stream flowing through sand or other loose matter, bears a distinct relation to the magnitude of the stream; the radius of curvature of its bendings being in proportion to that magnitude. Thus, all the water collected from this neighbourhood, is capable of producing no more than a paltry brook, as appears from the Water of Leith, which we see meandering between two of the ridges just mentioned. This meandering course, suits the diminutive size of the brook; whereas these ridges being straight, or, mathematically speaking, having a curvature whose radius is of infinite length, we are led, by a very obvious analogy, and in concurrence with what has been observd in other parts of the globe, to believe that a cause very different from any now in activity, and far more powerful, has exerted its influence upon this spot; that a stream
has

* As is very apparent upon some of the ridges last mentioned, as we see on the road to Dalkeith, just as we leave Edinburgh. Some ravines there present themselves in front, and on the right hand, descending the northern side of the ridge, and denoting a genuine diurnal action, in obedience to the mere action of gravity.

has flowed over it, capable of overwhelming and disregarding objects by which the Nile or the Ganges would have been turned out of their course.

But the testimonies in favour of this hypothesis are not derived from these large features alone; and it is not conceivable, that such agents could have been at work, without leaving behind them indications of their influence still more unequivocal. These occur in the very places indicated by the theory, and exhibit remarkable instances of abrasion. In order to investigate them with success, we must have recourse once more to the effects produced by one of our common streams.

Where a firm rock of any kind has been exposed to the action of a rapid river, its surface acquires in consequence of that abrasion a peculiar character, which every one recognizes at a glance, but which it is difficult to describe in words. The most obvious and universal effect of such an action, is the rounding of all the original angles of the rock; not only the prominent, but also the entering angles. For where an original hollow has occurred, coinciding at all with the course of the stream, the water has undergone an acceleration along that hollow, and has excavated for itself a waving groove more or less longitudinal. The whole has thus acquired a peculiar character, by an assemblage of flowing lines, which recalls the water-worn state of the rock. Another set of forms also present themselves in all such cases, which could not easily have been foreseen, and whose existence we learn only from observation of the fact: we observe, that the surface is in many parts excavated by shallow depressions of various sizes, which I shall distinguish by the name of *scoopings*, as resembling the effect which would be produced upon a soft body by the oblique blow of a spoon or scoop. I

conceive that they have in fact been produced by the action of eddies of inferior force to the main stream, but acting in company with it in different, and sometimes opposite directions. These various corrosions going on together, have each produced its peculiar effect; and most of them being concave, their meeting has given rise to the set of waving angular ridges which constitute the most unequivocal feature of a water-worn rock. These angular forms differ completely from those which occur in the broken surface of a rock. These last are acute, rectilinear and abrupt; while those others are continuous, flowing, and having their angles very obtuse; so obtuse in some cases, as not to be visible, unless the light strikes upon the rock in a peculiar direction*.

In a small but rapid brook near my house in the country, these forms occur on the surface of a smooth bed of sandstone over which it flows. I observed lately, when the brook was low and clear, that, to a certain distance below each of these obtuse-angled ridges, the rock was covered with green moss, while above the angle it was bare. The mode in which these forms are produced and maintained, seems thus to be pointed out: the main stream being possessed of just power enough to keep the rock clear of moss, and the eddy being too weak for this purpose. In a flood, I presume that the eddy acquires such power, that the whole rock is cleared.

In

* I have endeavoured, as yet without success, to make or procure an adequate representation of these forms; but I do not despair of overcoming this difficulty, and of being able, on some future occasion, to lay something of the kind before this Society that may not be unworthy of their notice. In this undertaking, I hope for the assistance of a very masterly artist, who is interested in the investigation, and who has already rendered me great services in this and other pursuits.

In whatever manner we account for the production of these forms, it is certain, that they present themselves on the surface of water-worn rocks. I have also observed them upon the surface of a mass of snow which had been acted upon, and partly removed by a strong wind. As the abrasion occasioned by a fluid in motion seems alone to possess that power, we may reasonably conclude, then, that a fluid has acted where such forms occur.

Now, it is a fact of which I shall presently lay various examples before this assembly, that the very rocks over which, according to the theory advanced in this paper, torrents of water have flowed, loaded with sand, and gravel, and large stones, and accompanied with streams of mud, are found to exhibit at their surface all the characters of abrasion lately mentioned;—the rotundity and flowing character;—the excavation of hollows into the form of waving grooves;—the concave scoopings, and the obtuse-angled, and waving ridges. Circumstances, then, seem to justify the conclusion, that, in fact, mighty torrents have traversed these districts.

In addition to the facts mentioned, and in company with them, another set also occur in these scenes, which powerfully corroborate the same conclusion. As stones of considerable bulk are often carried down by torrents, it is reasonable to expect, that upon rocks along which they have been hurried, and on which they could not fail to act as grinders, traces should shew themselves of that passage by scratches and abrasions of various sorts; and I have little doubt, that such will be found, when the effects of great floods in rapid rivers are properly examined; especially where a stream of mud has accompanied the torrent. Not having had occasion, however, to visit any scene of this sort, since the importance of the observation occurred to me, I have as yet, only met in rivers
with

with cases in which the surface of the rock has been dressed to smoothness, and in which the abrasion shews itself in the general rotundity of the grooves and scoopings, and in the obtuse angled and waving ridges just described*.

But what I have hitherto looked for in vain in common rivers, occurs universally in the diluvian scenes, where there is reason, from other circumstances, to believe, that a powerful abrasion has taken place, and where the surface has been protected from the injuries of the weather. Where it has been exposed to that injury, we generally find, that the large features of dressing, the grooves and scoopings, and obtuse-angled ridges, only remain. But where a mass of this kind, either by accident or design, has been followed under ground to where its surface has been protected by a covering of clay, an interesting and striking scene presents itself; the surface is found to resemble that of a wet road, along which a number of heavy and irregular bodies have been recently dragged; indicating that every block that passed, and every one of its corners, had left its trace behind it; and these are rendered very distinctly visible, when the surface is drenched with water.

In many cases these furrows or scratches have been so deep as to resist all the effects of the weather, and shew themselves in rocks that have been always exposed, sometimes
many

* The only case which has as yet occurred to me, bearing any relation to the action here alluded to, arose from a cause seemingly quite insignificant. In a neighbouring county, a country house, situated upon the slope of a hill, was assailed by a sudden torrent of water, produced by the bursting of a thunder-storm on the hill above. The impetuosity of the stream was such, that it forced its way through the under storey of the house, carrying along with it quantities of sand and gravel, and stones of considerable bulk. Happening to be upon the spot a few weeks after the accident took place, I observed, that every stone, as it passed through the house, had left a rut or scratch behind it, upon the flags over which it passed.

many yards in length. Occasionally, single scratches, and parallel sets of them, deviate by five or six degrees from the general direction; but the important circumstance is, that such deviation is rare, the very great majority of both sets agreeing in parallelism with each other, and with the general direction, not only of the scoops and grooves of the rock upon which they occur, but also of the ridges and large features of the district. A rock covered with these furrows, has externally an appearance greatly resembling what is called Slickenside, with this difference, that in the slickenside, we can always discover some proof that one portion of the main rock has performed a small slide upon the other; whereas, in this case, every thing shews, that the rock under consideration has stood firm, and has been abraded by a number of bodies in motion. The circumstance just mentioned, of occasional deviations from parallelism, seems also to distinguish this form from the slickenside; in which last, I believe, the lines are invariably parallel*.

The direction of the stream in the neighbourhood of Edinburgh, as indicated by the medium result of a number of observations, appears to have been from 10° S. of W. to 10° N. of E., by true bearings taken with a needle, and allowing $27\frac{1}{2}$ degrees west of north as the variation; and I have met with

* They are also distinguished by this, that in the case of Slickenside, the rock has always upon it a crust of calcareous spar or zeolite, which seems to have occupied a vein. Both sets seem to have been the result of mechanical action. The slickenside has been produced, I conceive, in the Plutonic regions, at a time when the mass in general has been so far cooled, as to be solid, though sufficient heat has remained to keep the fusible matter, as carbonate of lime or zeolite, which filled the vein, in a state at least of semifusion; so that in the sliding of one mass on the other, it has acted like grease in the wheel of a carriage: this theory accounts for the parallelism of the lines of slickenside.

with no case deviating more than 10° or 12° from that average on either side*.

Specimens.

I shall now describe the situation of fifteen specimens around Corstorphine Hill, denoted in Plate IX. by the number of each inclosed in a little circle.

No 1. The first is upon the rock at Craigleith, part of which is now worked as a quarry. It is a very complete and firm sandstone, and, as I have already mentioned, stands up, in the course of our supposed stream, to the height of forty or fifty feet, having a long projecting tail to the eastward, similar, on a small scale, to that of Edinburgh. It has likewise, on its western side, an excavation similar to that which forms the North Loch, but on a proportionally small scale.

This rock had lain bare, and open to the day on its westerly front, and it everywhere presents to view, where not concealed by the rubbish of the quarry, that rotundity of form which might be expected in such a situation, though, by its exposure

* I am aware, that the usual mode is to describe the direction of a current at sea, by stating the point towards which it flows. But my object, on this occasion, being to trace the origin of these currents, I have been induced to follow an opposite mode, and in general to denote the course by the point from whence the stream has been supposed to flow. In the following descriptions, the principal object of which is to mark the place of each specimen, I have frequently omitted the direction, in order to avoid interruption. But these directions have been carefully observed, and may be seen by turning to the page at the end of this paper, where they are all placed in one column, each opposite to the name of the spot, and to its number.

posure to the air, it has here lost every other symptom of diluvian action. Another face of the same rock, sloping to the south, which occupies the left hand mass of rock on the entry to the quarry from the south, which has lately been freed of its alluvial covering, exhibits the same rotundity of form, with the addition of the more minute circumstances of dressing, in high perfection. The space here laid bare is about ten paces by three. The upper portion is nearly flat; a great part of the surface inclines rapidly to the south; indeed, in some places, it is almost perpendicular. The perpendicular face, as well as the rest, is covered with furrows and dressings, indicating that the grinders had been pressed against the rock by the impetuosity of the stream, independently of their gravity; for almost the whole of these dressings are horizontal, or nearly so. There are some, however, which cut the rest at an angle of five or six degrees, or even ten degrees. The average direction is west, five degrees south*.

No. 2. The second specimen is of small size, but is well characterised, differing in no essential point from the last. It appears on a space of only two or three square yards, close to the road side, at the remains of an old quarry, called the Maiden Craig, about two hundred yards west of Craigleith, and opposite to the lodge of Ravelstone House. This rock, which is of sandstone like the last, has also a tail, interrupted, as mentioned above, very distinctly by the diluvian excavation on the west of Craigleith.

* Since this paper went to press, the valuable specimen here mentioned has been almost entirely concealed by the mettle of a road thrown upon it. The rock, however, remains entire, and may be cleared by any future observer.

No. 3. The third is within the court of the old farm yard of Ravelstone. The greatest part of this court consists of a rock entirely bare, and nearly horizontal; the whole surface of which exhibits the dressings in the most distinct manner, being entirely composed of furrows, some of which are of a large size.

No. 4. The fourth specimen occurs near the foot of Corstorphine Hill, upon an old quarry, called the Well Craig, within two or three hundred paces, bearing east 25° south of the old castle of Craigcrook. It presents the dressing very distinctly, and, like the two first specimens, upon a rock sloping rapidly to the south.

These specimens are all upon sandstone. The remaining cases belong to whinstone, which, as has been said, occupies the upper part of Corstorphine Hill, sloping to the west. The ridge of the hill lies north and south, with an inclination of about twenty degrees west of north. It therefore meets our supposed westerly stream nearly at right angles. The ridge, as seen from Edinburgh, is very much serrated, being cut across by small ravines; and upon the north, after the general mass has sunk below the neighbouring country, the whinstone makes its appearance in various elevated masses in Barnton Park, lying in the prolongation to the northward of the longitudinal line; the most northerly of these elevations, which comes within forty or fifty yards of the old house of Barnton, rising not more than six or eight feet above the turf. These rocks, all of which, like the hill itself, present a sloping face to the west, exhibit, upon that face, all the large diluvian features already described: the rotundity, together with the grooves and scoopings; but none of the scratches, or other small features, are to be seen. The character, however, of the general operation is so well shewn, that the bearings of the stream

stream are distinctly marked, and agree with those already observed.

The ridge of the hill is about a mile and a half in length, and rises in the middle to a height, according to the map, of four hundred and seventy feet from the sea, perhaps three hundred from its base. Along the summit, which is everywhere planted, the rock is seen bare very frequently, in the midst of the young woods, and often shews itself also in the adjoining fields. Among these rocks, which I have examined again and again with care, so many examples occur, similar to those just described in Barnton Park, that the hill itself may be looked upon as one specimen illustrative of this important truth, in so far as the great features are concerned. In the following ten specimens, chosen on that same ground, the minuter indications of abrasion are also visible.

No. 5. In a place called Craighouse, upon the south side of the Queensferry Road, opposite to Barnton Park, and between it and the main hill, a mass of whinstone occurs, making part of the same longitudinal line. It was formerly worked as a quarry, for the purpose of paving and road-making, and is now clothed with young wood. In its highest point it exhibits all the large features of diluvian action, but none of the small ones. These, however, it appears, have been defaced by the action of the weather; for, on removing the young wood and soil from a surface of about twenty yards by thirty of the lower part immediately adjoining, I have discovered them in full perfection. Not only the grooves and scoopings make their appearance, but the surface is seen, when drenched with water, to be entirely covered with longitudinal dressings, as already described, in speaking of the specimens on sandstone. Where a rent has occurred at all in the direction of the current, it has guided the formation of a groove,

the zig-zag form of the rent being softened off into a waving hollow of uniform breadth, highly characteristic of the violent action of a stream vested with the power of grinding. It is remarkable, too, in more than one of these grooves, that at their Eastern extremity, after maintaining an equal wideness throughout, they suddenly spread to right and left, and lose their form. These grooves are three or four feet in length, by four or five inches in breadth. When a rent has occurred at right angles to the current, the rock has been ground across it, without visible modification of the action.

No. 6. From Craighouse Quarry (following the road which leads to Corstorphine along the western face of the hill, and passes on the east side of Clermiston) we arrive at a ruined cottage, on the left hand, at the mouth of a little ravine which here crosses the main ridge, and is called the Glen of North Clermiston. Close to the road, immediately upon the north side of the ruined cottage, and within the wall which encloses the wood, the whinstone almost bare, presents to view a continued series of parallel grooves, one of which is twenty paces in length, and others more. The hollows of these grooves are filled with moss and earth, which, being removed, and the rock drenched with water, a set of furrows shew themselves perfectly well characterised, having the same bearings with those of the other specimens, so as to lie quite at right angles with the longitudinal direction of the hill. This situation must be at least one hundred feet perpendicular above Craighouse.

No. 7. About two hundred yards farther to the south, below the road, stands a group of cottages, comprehended under the same name of North Clermiston. Opposite to these, and above the road, is a barren space which has been the seat of former quarries. Here several rocks are visibly dressed with the large diluvian features; and at one spot, just twenty-six
paces

paces north-east from the nearest house, a fine set of furrows occur, having the direction west 10° south. Continuing along the same road, after passing through a young wood, we arrive at the farm-house of Mid Clermiston, lying below the road upon the westward. Two sets of enclosures occupy the space above the road, between it and the young wood, which covers the summit of the ridge; the most northerly of these, called the North Hill Park, contains two very interesting specimens, both situated upon the upper side of the field, and close to the wood.

No. 8. Near the middle of the upper side. Here a space of several yards square has here been laid bare by my former operations, and furrows and diluvian dressings are most distinctly visible, indicating a direction from west 10° south. One small set, consisting of four or five parallel scratches, produced probably by one stone, cut the rest at an angle of five or six degrees, indicating west 5° south.

No. 9. In the same field, and also close to the wood, about thirty or forty paces towards the south, another specimen of the same kind occurs very well characterised.

No. 10. Immediately adjoining to this last-mentioned field, on the south of it, lies another irregular field, called the North Mid-hill-park. From the middle and upper part of this field, a path leads up to the summit of the hill, in a direction nearly due east. This path crosses an open and nearly bare space, in the midst of the wood, at the distance of about fifty paces from the ruins of the signal-house on the summit. This space is of about half an acre in extent, and so bare, that no trees have grown upon it. The rock has here stood up in parallel tables, as is not unusual with whinstone, running nearly east and west; these have been completely rounded, as I conceive, by the current, and shew the
large

large features of dressing very distinctly; the scoopings, in particular, are very well defined; the small features have been mostly defaced by the action of the air, but the removal of the turf has brought some of them very well into view. These agree with the grooves in the general indication of west 10° south. In several places where a rent has occurred in the direction of the stream, it has been excavated into the form of a waving groove, as already seen in No. 5.

No. 11. At the summit, a signal post had once been placed, and the house used for that purpose, remains in a state of ruin*. The rock of the summit (eight feet south-east from the south-east corner of the house) is dressed with some diluvian furrows, denoting west 10° south, as I found by clearing away some of the turf.

The ridge of the hill stretching to the south, shews everywhere the same general character, by the rounding of angles, denoting the action from the west.

No. 12. In a field called the South Mid-hill-park, in the farm of Mid Clermiston, in the south-east corner of the field, is a fine specimen, indicating west 5° south.

No. 13. The rock on which the summer-house of Ravelstone stands (the Stone View, as it is called by the country people) though completely exposed to the weather, shews some decided furrows. It forms the summit of the southern part of the ridge, and is only a few feet lower than the highest point. From it the scenes No. 1. No. 3. and No. 4. are still distinctly visible.

No. 14. In a pasture-field facing to the south, called the Sheep Park, belonging to Corstorphine Hill house, and immediately

* Since this paper was read, the signal-house has been removed, and its foundation only is now visible.

diately above it, is a good specimen, shewing the small features of the dressing; the direction of which agrees with that of the rest on this hill. This specimen lies contiguous to the fence, on the south side of the field, at the distance of forty-nine paces west from a wicket, close to some cottages.

No. 15. Close to the bottom of the hill, on the south-east, there is a specimen, in fine condition, near to the avenue leading to Belmont. As we go up this avenue, in a direction nearly due north, we leave two fields upon the right hand. The specimen here alluded to lies in the second or uppermost of these fields; being the same in which a whinstone quarry is now worked. The specimen lies near to the south-west corner of that field, being forty paces distant from the wall on the west, and forty also from that on the south. It presents one of the most perfect and complete examples I have met with, of a groove, with a set of scoopings corresponding alternately to each other in opposite sides of it, and some of the obtuse angled ridges in the highest perfection. The whinstone is also very distinctly dressed with the small features, which lie horizontally, and follow some wavings of the groove: their general direction is like that of all the rest, with slight variations on each side, of ten degrees south of north; at the same time, it is worthy of notice, that the surface of the ground declines rapidly from north to south.

I have thus pointed out fifteen specimens, within a circle two miles in diameter, surrounding Corstorphine Hill, each exhibiting both the large and the small features which indicate the action of water flowing with violence along the surface,

surface, and carrying large blocks of stone along with it. The number of examples concurring in the same indications of direction, might easily have been greatly augmented, as any person will see who examines the summit and western face of this hill ; but I have judged that number to be already sufficiently great. I have taken care to denote each spot alluded to, in a manner so particular, that it might easily be found at this moment : I say at this moment, because, as is well known to every geologist, nothing is more precarious than the existence of such specimens, they being liable perpetually to be overgrown with vegetation, or covered with rubbish, or perhaps completely annihilated by the progress of the same operations which brought them into view. These specimens are by no means all equally interesting. Some of them are brought forward as simple indications of direction, and owe their value to the force which the argument derives from the concurrence of such a number of results. Those which are deserving of notice on their own account, are Nos. 5, 8, 10, & 15.

By considering together four or five of these examples, which lie at a small horizontal distance from each other, we shall be led to a conclusion which could not be deduced from any one of them singly. I have said, that close to the summit of the hill upon the west, an extensive space (No. 10.) of rock occurs nearly bare, displaying a succession of parallel grooves, pointing in the direction of the supposed stream, and upon which some of the minute dressings also are apparent. On examining this spot, two members of this Society, whose ideas on this subject are hostile to mine, immediately remarked, that the appearance reminded them of what they had seen in Arran, in the bed of the Garvalt, where the granite has been dressed by that torrent ; and they alleged as a consequence, that the phenomenon was reduced to a common event.

I beg leave, in the first place, to avail myself of this testimony, in proof of the fact, that a stream, at least equal in force to the Garvalt, has, at one period, flowed over the rock at the summit of Corstorphine Hill.

It now remains a question of importance to say, from what source such a stream could have flowed, and how so much water could be collected. The spot under consideration, is not above twenty feet perpendicular below the extreme summit, so that the heaviest thunder shower, could scarcely produce there a sensible run of water; and if such a run were produced, it would have nothing to carry, the whole consisting of one hard and firm rock. The dressing is of large extent in itself, as the bare rock shews, and it is also connected, more or less intimately, with the dressed spots on the western face, Nos. 6, 7, 8, 9, 12, & 14., so that a stream of considerable breadth as well as power would be required to fulfil the conditions of the case. The difficulty is rendered more pressing still by two cases already mentioned; one at No. 11., and the other at No. 13., in both of which cases the rock is visibly dressed, in a spot which constitutes the local summit; the last of these, No 13. being the actual summit of this hill, and the highest point within the distance of many miles; and the other being separated from any higher ground by ravines, so that, in the present state of things, no water could flow towards either of them.

The only explanation that could be given of these facts, by means of the diurnal agents alone, must be obtained by going back (as has been done in a theory discussed in an early part of this paper), to some very remote period, when this hill was much higher, and more extensive than at present, and when a stream equal to the Garvalt could be collected above those spots, and could run over them, so as to produce the dressings under consideration; and, by supposing, that in sub-

sequent intervening ages, as in the hypothesis alluded to, all this additional mass of elevation and extent had been removed by the perpetual corrosion of diurnal actions.

This hypothesis might be maintained as within the limits of mere possibility, if all these dressings lay on one side of the hill, as this imaginary additional height might be carried to the opposite side. But as they occur equally on both sides of it, either upon the hill itself, or very near to its base, that hypothesis seems to be done away, as applicable to a part only of the phenomena*.

To

* It has also been alleged, admitting these dressings to have been the work of flowing water, that the action may have taken place at the bottom of the sea, and that the rock has since been elevated into its present position. Subscribing, as I do most heartily, to the doctrine of elevation, as advanced by Dr HUTTON, I am far from denying, that the very case here supposed, may frequently have happened; I even conceive that it must have occurred occasionally. I should not, therefore, object to this explanation of a dressed rock, that stood single in an extensive district. But that is by no means the case in the present instance: the phenomenon is far from single; it is surrounded, on the contrary, by facts, and classes of facts, calling for an explanation. This is afforded, if I do not deceive myself, by the hypothesis advanced in this paper. But the explanation just alluded to, would account for the dressed rock alone, and would be altogether inapplicable to the other classes of phenomena; by such a partial solution, we should therefore lose all the advantage derived in the theory proposed, from the mutual light which the large features and the small ones have thrown upon each other.

This explanation, too, would require no less an effort of imagination than ours; in fact, the two suppositions do not differ as to the magnitude of the exertion employed, but only as to the place of its original action, which we have conceived to lie somewhere to the westward. An elevation such as would carry this district from the bottom of the ocean into its present position, could not fail, (as we have endeavoured to prove in the first part of this paper), to be performed by starts, and consequently to produce waves. If the whole elevation took place at once, it would be of magnitude amply sufficient to fulfil, in some other quarter, all the conditions of our hypothesis.

It

To account for these dressings, therefore, we must call in the assistance of some extraordinary agent; and this assistance is at hand in the stream flowing from the west, the existence of which we have been led to imagine by the parallel ridges and tails which occur in this district. For the form, situation, and direction of these dressings are such, as a belief in the reality of that stream would lead us to look for. The mere arrangement of ridges stretching east and west, leaves it in doubt from which of these two opposite points the stream flowed; but the circumstance of the long depositions or tails parallel to these ridges and dressings, which occur upon the eastern side of obstacles, such as the rock of Edinburgh Castle, entirely removes that ambiguity, and proves that the stream must have flowed from the westward.

The *grinders* must then have been forced to move upwards along the westerly face of Corstorphine Hill, and in that mode

B b 2 *continued from page 194* their

It might also be alleged, that the elevation was performed only in part, when the dressings took place; and that a further rise had carried the mass, on a subsequent occasion, to where it now stands; but nothing here seems to justify such an intermediate supposition. All the diluvian facts in this neighbourhood, that have come under my observation, concur in denoting *one* inundation overwhelming the solid mass of this district, which had been elevated into its present position by some still more ancient revolution of the same sort; this inundation being the last catastrophe to which it has been exposed.

Having endeavoured to illustrate the appearance of the dressed surfaces, by referring to abraded rocks in the beds of rivers, I find that some gentlemen, who had heard but a partial statement of my views, conceived it to be part of my supposition, that these dressings, like those in a river, were produced by water acting for a long time. But this is by no means my view; my theoretical notions limit the action upon the hill under examination, to the passage of a single wave, embracing a period of time that could only be expressed in minutes; but during that short time, I conceive the water to have been urged forward with such force, and to have carried with it so many powerful agents, that it has produced effects equal to the work of ages under other circumstances.

their action upon the rock would be most powerful, provided they were driven forward by sufficient force; the horizontal motion thus urging each stone against the object to be ground, (by an action similar to that which impels forward the chisel of a mason, dressing the surface of a stone, which acts in the direction of the axis of the chisel, forming an angle with the surface upon which it acts), whereas, if the declivity lay the opposite way, that same horizontal force would tend immediately to draw away the grinder, and prevent its action. We observe, accordingly, that though furrows or dressings do occur at a little distance to the eastward of the base, upon elevations presenting a western aspect, or lying flat, none such are any where to be found on a surface of rock inclining to the east.

It is further remarkable, how little the direction of this action has been disturbed by local circumstances. Thus, the furrows in No. 6. follow the medium course right up the hill; though close to the ravine on the south, and though, upon the other hand, at the distance of about two or three hundred yards from the abrupt termination of the hill itself. This seems to prove, not only that the stream flowed over Corstorphine Hill, but that it flowed over it with a great depth; for had the stream done no more than just cover the summit, its course must have locally obeyed the inclination of these surfaces. To pass along and disregard them entirely, seems to denote a great superiority of depth; and in calling it double, we probably do not exceed what is necessary. We have seen enough, however, to justify my assertion, that the stream must have been incomparably superior in magnitude to the Nile or the Ganges, since even the Corstorphine Hill, occurring in the course of these mighty streams, must have completely deranged their course.

This

This hill is stated to be four hundred and seventy feet above the sea. If we consider our stream as double the depth, or one thousand feet, we shall probably not go beyond the truth. This height is nearly sixteen times the altitude of the wave at Cadiz, which was sixty feet. The phenomena of the Alps, though very imperfectly known, indicate a magnitude double of this, by the mere position of the blocks on Jura, two thousand feet above the level of the Lake of Geneva. While I thus, however, ascribe great magnitude to the diluvian torrent, I am upon my guard against the excess of such impressions, and the means have already occurred, as I shall presently state, by which limits are assigned even to this colossal agent.

The geological truth supported by this concurrence of facts, both vast and minute, is confirmed by innumerable circumstances, which present themselves to view on all hands in this neighbourhood.

No. 16. At a place called Dickson's Craig, upon the road to Queensferry, about two or three hundred paces west of Barnbogle Gate, is an example of whinstone rock dressed like those mentioned, having a direction of three degrees south of west.

No. 17. Another also occurs at the sandstone quarry of Redhall, at the distance of about two miles south of Corstorphine Hill, and occupying the side of the valley directly opposite to it. This quarry is opened in one of the diluvian ridges, and the rock is covered by a bed of about twenty feet thick of the blue clay, having various large blocks of whinstone suspended in it, one of which is not less than forty cubic feet. The dressings point eight degrees south of west.

No. 18. On the hill of Ravelrig, in a young wood, near the eighth milestone from Edinburgh, on the road to Lanark, is a fine

fine specimen of dressed rock, which had been laid open by the forming of the road, and is still visible ; the direction is fifteen degrees south of west.

North Berwick Law in East Lothian, has a tail extending towards the east ; it has likewise a specimen of whinstone rock with the dressing upon it.

No. 19. Another specimen occurs on a hill, which makes one of a group of rocky eminences, to the southward of North Berwick Law, upon one of which the ruined tower of Fenton stands. The particular spot lies three or four hundred yards to the northward of that tower, and close to the eastward of the little village of Kingston. The rock presents to view furrows and scratches similar to those above described on Corstorphine Hill, with the additional circumstance, that the action of the stream has here undergone a visible modification, by the prominent form of some parts of the rock, in consequence of which the dressings have, in some places, been turned, to the amount of five or six degrees, out of the general direction ; which, however, they resume gradually, in the course of a few yards. A curvature is thus produced, highly characteristic of the action of a torrent from the west. The scoopings, too, meeting the general direction in obtuse angles, are indicated in the most striking manner. The general direction, independently of these local disturbances, agrees with that in the neighbourhood of Edinburgh, being from fifteen degrees south of West.

The same law prevails in other parts of East Lothian, as appears by examination of the ridges at Spott and Pinkerton, which are very conspicuous objects from the great road between Edinburgh and Dunbar.

Observing the direction so constantly maintained in all this district, which, at the same time, is that of the valley in which
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the estuary lies, I began to suspect that the production of these effects may have been influenced by that valley; for though the torrent had sufficient power to disregard Corstorphine Hill, it may have yielded in some degree to greater eminences. If this law were established, we should be enabled better to understand these operations, by having their stupendous and unbounded magnitude reduced within tangible limits. I became, therefore, anxious to examine the facts which lay beyond the influence of this valley, where the indication of actions of a similar nature might be found. I recollected having seen some ridges of a diluvian character in the country immediately behind the promontory of Fast Castle in Berwickshire; and as this constitutes one of the southern flanks of the mouth of the estuary, I expected that some novelty in point of direction might here occur, and I was not disappointed; for I here met with a set of very distinct diluvian ridges, called Lowry's Knolls, the direction of which lies, by true bearings, west 35° north; so that they form an angle of no less than forty-five degrees with the general direction of the estuary, such at least as it occurs in East and Mid Lothian. As this new direction is assumed by the stream immediately on finding itself at liberty, we must suppose that the water, in this last-mentioned course, follows either the main direction, or approaches nearer to it than it did in the estuary. More observations are now much to be desired. With a view to them, and in consequence of a recollection which led me to suppose, that the Castle of Stirling was possessed of a diluvian character, I have (since this paper was read) made a little excursion to that place.

I discovered, however, that the elongation from Stirling Castle is entirely composed of solid whinstone, consequently
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that its form has not been produced by a diluvian action. On the other hand, I found myself surrounded by a set of ridges of alluvial matter possessing a true diluvian character, and several rocks presented themselves, having the larger features. What gave a peculiar interest to these facts was, that these features agreed with the ridges, in indicating a direction between west and north, or east and south; that is to say, quite different from that near Edinburgh, but nearly agreeing with what I had observed in the lower extremity of the Frith of Forth. After considerable search, I discovered a rock of sandstone in Torwood, about six miles on this side of Stirling, possessing the diluvian dressing in the highest perfection. This rock is visible upon the south side of the road, just one hundred paces west of Torwood turnpike-gate; and the dressing is most conspicuous close to this, in the neighbouring wood, on the surface of a rock, some square yards of which I laid bare. The direction of this dressing confirms what in a great measure I had inferred from the large features, and from the ridges. Its direction indicates no less than fifty degrees north of west, or south of east, and thus differs very widely from that near Edinburgh, going still farther to the northward than what I had observed at the mouth of the Frith of Forth. It has a considerable agreement, however, with this last, and seems to favour the idea just suggested, that the direction in the neighbourhood of Edinburgh may have been occasioned by the local influence of the estuary, since the direction of the stream before entering it, and after quitting it, is nearly from north-west to south-east. This influence upon the direction of the stream, and on the deposition of loose matters, seems to bear an analogy to that which is exerted, on a small scale, in the abrasion of solid rocks, as seen on the ridge of Corstorphine Hill and at Kingston.

I have witnessed another set of facts, of which I can, as yet, produce but one example. The district along which the road passes from Edinburgh to Dumfries, in the neighbourhood of Noblehouse, exhibits a series of low hills, possessing the characteristic forms of craig and tail, which belong to those in the neighbourhood of Edinburgh, but such as to indicate the action of a stream flowing from the south-west. This is a high district compared to that of Edinburgh, and the waters may here be supposed in some measure to flow towards the estuary of the Frith of Forth.

These changes shew the importance of more extensive observations. At the same time, the concurrence of various indications, in pointing out one common direction at each place, seems to denote the influence of some general cause, and authorises a hope that the facts, when properly collected, will enable us to trace the general direction, and perhaps the origin, of this important agent.

Character of the Western District.

Another district of Scotland, which circumstances have led me to traverse in various directions, presents to view a state of things as different from those which have lately been described as could well be conceived. I mean, the south-west of Scotland, comprehending the stewartry of Kirkcudbright, the shires of Galloway and Dumfries, with part of Ayrshire, on one hand, and part of Cumberland on the other.

In that district, the straight and parallel ridges, so universal in this neighbourhood, and the forms of craig and tail, such as that of Edinburgh Castle, nowhere occur. The same assemblage of substances assume a different form, and present to view a perpetual succession of knolls, equally round at both extremities. This arrangement is conspicuous in the neighbourhood of Gilsland, which is surrounded with knolls of this description. It was there that a theory occurred to me, to account for the contrast of the opposite side of our island, which I have never since had occasion to alter, having found that it gained ground by every new observation. I have, in particular, observed these forms to prevail exclusively in the valleys of Carlinwork, of Dumfries, and of Kirkcudbright. I imagined, that the diluvian wave had flowed at some remote period from a westerly or north-westerly direction, and had broke over our island; that its magnitude, though perhaps far short of the alpine stream, had been such, that a great body of its water crossing the ridge of country which separates the two coasts, overwhelmed the district in the neighbourhood of this city, discharging itself into the German Ocean. But that another portion of the wave, being retained by that ridge of country, and being left to follow the impulse of gravitation, when the superior water flowed on, had returned again upon the countries which it had just crossed, like the back-draught of a common wave. By watching the operations of a high sea, breaking upon a rapidly shelving beach, covered with coarse gravel, we may form some judgment as to the influence of such a back-draught; the sound produced by the blocks of stone grinding upon each other, as the wave retires, conveys a strong idea of the corrosion which must take place in such circumstances. Our Eastern coast would thus be exposed to a single diluvian action, whereas that on the West would undergo two in succession,
and

and nearly in opposite directions; and the analogy of the events at Lisbon and at Lima, leads us to suppose that several similar actions may have taken place in succession. At Lisbon, five waves in succession followed the first. In the present case, the influence of all but the first, would probably be confined to the western side; or if any of the rest possessed elevation sufficient to surmount the ridge, still the portion of its water which passed over, and that which returned, would each follow the same course with the corresponding portions of the first wave. And there is every reason to believe, that the action would be repeated often upon the side facing the inundation.

The facts seem to correspond with this theory. We have endeavoured to account for the peculiar form of craig and tail, and for the parallel ridges, so frequent on the eastern coast, by the influence of a single powerful stream, the effects of which have since undergone no modification, but from the feeble exertions of diurnal causes; whereas, on the other side of the island, the same set of forms, which we may conceive to have been generated by the first direct impulse, being assailed in new directions, by other forces, less powerful, but frequently repeated, we have reason to expect that these assemblages, composed of loose, and newly deposited materials, would undergo great changes, and that the ridges would be broken down into successions of knolls; that the crags would be filled up, so as to equalise the declivity on all hands; in short, that such a scene as the west coast of Scotland presents to view, would be the result. The circumstances of detail concur with the observations founded on the large features; at least they by no means contradict them. In the west, many striking instances occur, of rocks dressed with grooves and furrows, as already described in the east: but there is this material difference between the

two sets,—that whereas in the east, these furrows have preserved the same direction, those on the west follow directions so irregular, as to baffle all arrangement, or, if any such can be traced, it is found to be entirely local, and to depend upon the course of the valleys in the immediate neighbourhood. In some places in the west, as near Moffat, I have met with vestiges of ridges; but, on close inspection, I found them rounded off at both ends. Even these, however, occur but very rarely, and are to be expected, under the hypothesis here laid down, according to which, it is reasonable to suppose, that the effects of the first impulse should not be always completely done away by those which followed. Also the direction of hills in the west, may occasionally have been such as to produce conspiring effects with those of the first action.

In the neighbourhood of the Bay of Kirkcudbright, I have seen two specimens of rock, dressed with furrows, very well characterised, and both indicating a direction from the north; one near the porter's lodge connected with the House of Balmae; the other upon a rock within high water-mark, upon the south side of St Mary's Isle. But the neighbouring country has no corresponding character, all the loose assemblages being arranged in knolls.

On the island in Loch Doon, upon the confines of Ayrshire, and the stewartry of Kirkcudbright, the dressing of the rock is peculiarly interesting. The island is of granite, near its junction with the strata; and the granite contains many sharp angular fragments of killas. Where the rock has undergone the diluvian dressing, the contrast of the two substances is beautifully displayed, the granite being of a light colour, and the killas dark-blue.

By the action of the air, the granite has been decomposed at the surface, and has been corroded nearly to the depth of an
inch;

inch ; but the killas has resisted that action completely, and its fragments stand above the level of the corroded granite, presenting to view the diluvian dressing unchanged. A curious consequence has resulted from this circumstance : A fine white clay, produced by the decomposition, was mistaken some years ago for marl, and in order to obtain it, a tunnel was cut through the rock, over which the water in its exit to the lake had flowed. The level of the water having in this manner been lowered several feet, a portion of the rock of the island is brought into view, which had previously lain under water, and which, in consequence of the exclusion of air, had not been decomposed, nor had suffered any change, since it underwent the diluvian dressing. The consequence is, that in the upper part of the island, we see the granite corroded, and the killas entire, whereas, near the water's edge, the surface of the rock presents its diluvian dressing throughout ; and the two substances are only distinguishable by their colour. We have thus an interesting display of the influence of time ; and the circumstance suggests to me a mode, by which the antiquity of these events may perhaps, in a certain degree, be ascertained, and of which some future observer may avail himself. Had the tunnel been cut many centuries ago, at a known period, and had the granite exposed by it to decomposition, undergone a measurable corrosion, we should have been furnished, by instituting a comparison between the two corrosions, with a chronometer, or a rational basis for calculating the antiquity of the diluvian event. The same view may be taken of those blocks of granite in the valley of Monti, upon Saleve, mentioned by SAUSSURE, and alluded to in a former part of this paper.

In the neighbourhood of Loch Doon, a set of very large, loose, and rounded blocks of stone occur, which resemble those

to

to which I have so often alluded in the valley of Geneva: the largest of these is known by the name of the Carnwhaplestone. The appearance of the country induces me to imagine, that they had been lifted out of the loch by some concentrated force of the returning stream.

A number of large blocks of granite also occur on the hill, at the Pass of Stanmore, in Cumberland, which I was at pains to trace to their source, and found it to be a rock called the Westledale Crag, which is a granite of the same quality with them.

A single block of granite, of four or five feet in diameter, occurs in the street of Darlington.

The transportation of such blocks does not seem to have happened very frequently in Great Britain, at least in those parts of it with which I am acquainted; and where they occur, in the neighbourhood of Edinburgh, they are generally embedded in the blue diluvian clay. That water, however, in the circumstances which we have supposed, does possess the power of transporting very large blocks, we learn by a direct proof, in what was done at Cadiz by the wave which accompanied the earthquake in 1755. It broke down a large piece of the rampart, and carried solid masses of masonry, of eight or ten tons weight, to the distance of forty or fifty yards, as we learn by the account of Mr B. BEWICK, (*Philosophical Transactions*, vol. XLIX.)

THE observations I have made on this diluvian subject, has been confined to a small range of country, comprehended between our two seas, because it is only of that small range that I can speak with any tolerable certainty; but I have reason to believe, that other members of this Society, possessed of every requisite advantage, are ready to take up this curious subject, and to pursue it with vigour. On that account, I have the less scruple in bringing forward views, that may to many appear extravagant, since there is reason to expect, that whatever errors I may have committed will soon be rectified.

I trust also, that the facts brought forward in this paper, are susceptible of an extensive application, and that they furnish the means of ascertaining the direction of diluvian inundations across the great continents. By a comparison of directions, these tremendous agents may be traced to their source. The native place of the granite blocks, mentioned in the 1st part of this paper as occurring on the shores of the Baltic, and also of those on Mount Jura, may thus be discovered. If both sets came from Mont Blanc, we may expect to find between it and the Baltic Sea a system of crag and tail like that in the neighbourhood of Edinburgh. If the Prussian blocks came from another quarter, the circumstance will soon speak for itself, and the question will probably be decided by the first traveller acquainted with these ideas; who passes; ever so rapidly, through those countries.

In the same manner, we may expect to see light thrown on the history and circumstances of that tremendous event already

dy alluded to, as indicated by the observations of PALLAS; by which it appears that the skeletons, and even carcasses of great animals and fish, natives of the torrid zone, are found embedded in the soil along the banks of the great Russian rivers, and buried in the earth, in the frozen regions along the shores of the North Seas, where thaw never penetrates more than a yard below the surface.

The series of inquiries, the result of which I have laid before the Society in the course of this season, was first suggested to me by the vertical rents, filled with congealed lava, which are visible on the craggy face of Somma, the ancient part of Mount Vesuvius. They exhibit a clear view of those restraints under which the eruptive efforts of volcanoes are held in quiet times, and which are occasionally surmounted; and this circumstance, transferred by analogy to the plutonic regions, has afforded, by means of the forcible intrusion of liquid granite, a satisfactory account of the convoluted structure of the strata of killas, when in a semifluid and flexible state; the convolutions thus formed, furnishing an easy explanation of vertical stratification.

The same restraint, occasionally overcome, has explained the protrusion of whinstone among the secondary strata, sometimes in vast beds parallel to them, and sometimes in huge amorphous masses. The restraint exerted by these inflexible obstacles, being surmountable only by acts of violence, I have concluded that the elevations, both volcanic and plutonic, must have been accompanied by a succession of powerful starts; and great part of these operations taking place under the sea, that enormous waves must have been thus produced. In this manner, a satisfactory account has been furnished of earthquakes, and of their attendant waves, as well as of the overwhelming inundations alluded to in this paper, the reality
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of which is evinced, as has been shewn, by the concurrence of a three-fold set of facts, surrounding us in this neighbourhood.

1. The distribution of loose matters in tails and ridges, on this side of the Island, and in knolls on the West. 2. The grooves and scoopings, and obtuse-angled ridges, occurring on the surface of rocks of every description; and, lastly, the scratches and other minute features of abrasion, which are found to accompany the large features, where the rock has been protected from injury. All these events have been shewn to arise, as natural consequences, out of the Huttonian Theory.

As another consequence of these events, I have also brought forward the formation of valleys of all sorts. I have imputed the detachment of mountains, and the formation of islands, as well as of promontories and arms of the sea, to circumstances which could not fail to take place in the forcible elevation of stiff and frangible masses, performed by a succession of local efforts from beneath, urged by irresistible power*.

I have furnished a theory of the formation of lakes, which does not seem hitherto to have been accomplished in a satisfactory manner, by any other hypothesis. One class, belonging

VOL. VII.

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* Since the first part of this paper was printed, a very familiar circumstance has presented itself, which affords a clear illustration of this great natural operation. When a mason is preparing common lime for building, he lays the burnt limestone in a heap, and throws water upon it; after this, some time elapses before any action is visible, before the lime, as it is said, begins to fall; in this interval the sand, which is to be mixed with the lime, is thrown upon the heap, so as to cover it completely, and the surface is made smooth and uniform by repeated blows of the flat of a spade. Soon after this, a heave takes place from within; the lime swelling, raises up the sand, and divides it by numberless rents, which becoming wider and wider upwards, as the heave advances, a representation in miniature is produced, of an alpine district.

ing to solid rocks, I have derived from rents formed by inequalities of elevation, and the opening of rents upwards. Another class, formed in alluvial districts, has been traced to modifications performed by various obstacles upon the depositions made by the great inundations alluded to.

In thus controverting some of the collateral opinions of Dr HUTTON and Mr PLAYFAIR, I venture to hope that my arguments, which have been founded on their principles, and which have led me to acquiesce in their most general and important conclusions, may tend less to weaken, than to confirm, the result of their immortal labours.

SPECIMENS.

SPECIMENS.

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| No. 1. | Craigeith Quarry, from | - | W. 5° S. to E. 5° N. |
| 2. | Maiden Craig, | - | W. 5° S. |
| 3. | Ravelstone old farm-yard, | - | W. due. |
| 4. | Well Craig, near Craigcrook, | - | W. 20° S. |
| 5. | Craighouse Quarry, | - | W. 5° S. |
| 6. | North of ruin at Dean of North
Clermiston, | - | W. 10° S. |
| 7. | North-east of cottages there, | - | W. 10° S. |
| 8. | Middle of the North Hill Park, | - | W. 10° S. |
| 9. | South side of ditto, | - | W. 10° S. |
| 10. | Bare space west of summit, | - | W. 10° S. |
| 11. | Summit of the hill, | - | W. 10° S. |
| 12. | South-east corner of South Mid-
Hill Park, | - | W. 15° S. |
| 13. | Summer-house on second sum-
mit, belonging to Ravelstone, | - | W. 15° S. |
| 14. | Sheep Park of Corstorphine
Hill House, | - | W. 8° S. |
| 15. | Below Murrayfield Quarry, east
of Belmont, | - | W. 15° S. |
| 16. | Dickson's Craig, Barnbugle, | - | W. 3° S. |
| 17. | Redhall, | - | W. 8° S. |
| 18. | Ravelrig, | - | W. 15° S. |
| 19. | Kingston, near North Berwick, | - | W. 15° S. |

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Fig 1.

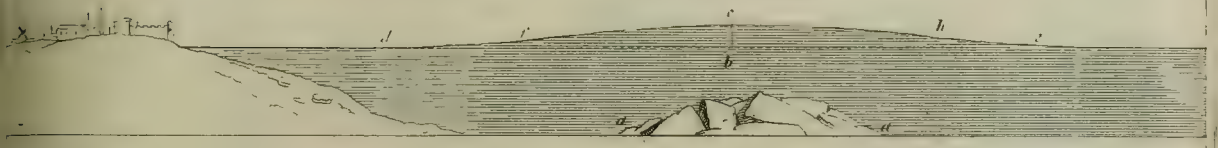


Fig 2.



Fig 3.

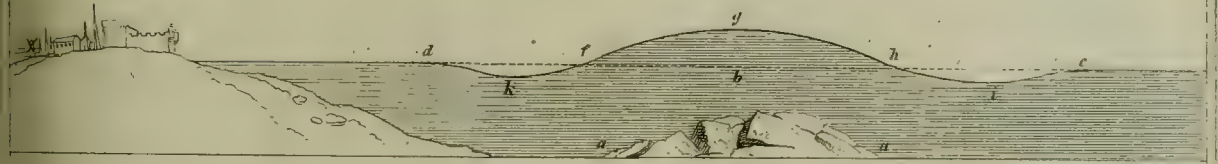


Fig 4.

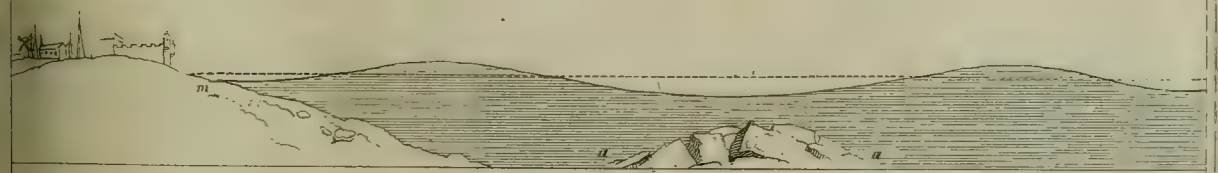
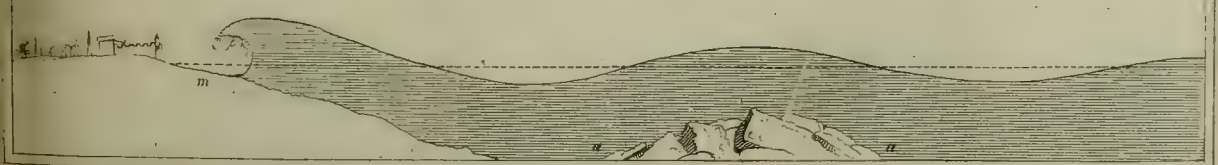
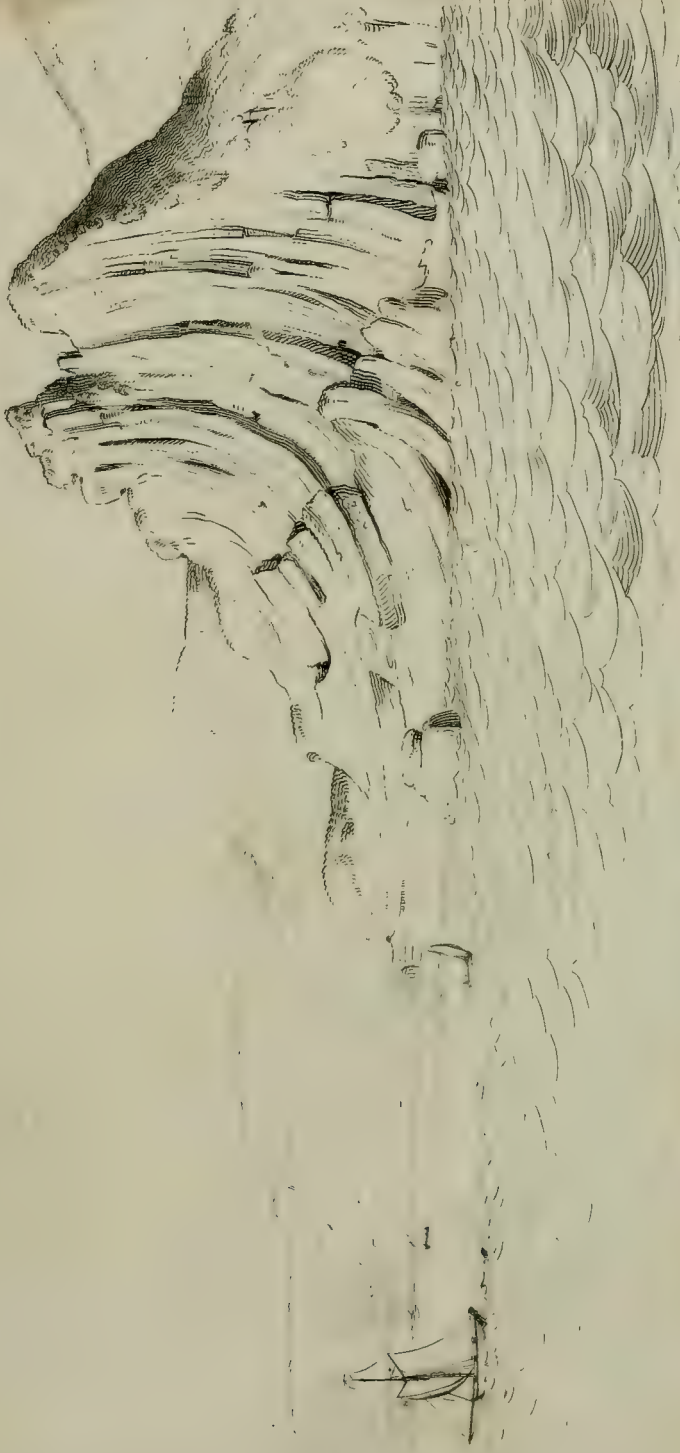


Fig 5.







Engraved by W.A. D. Fraser, Edin.

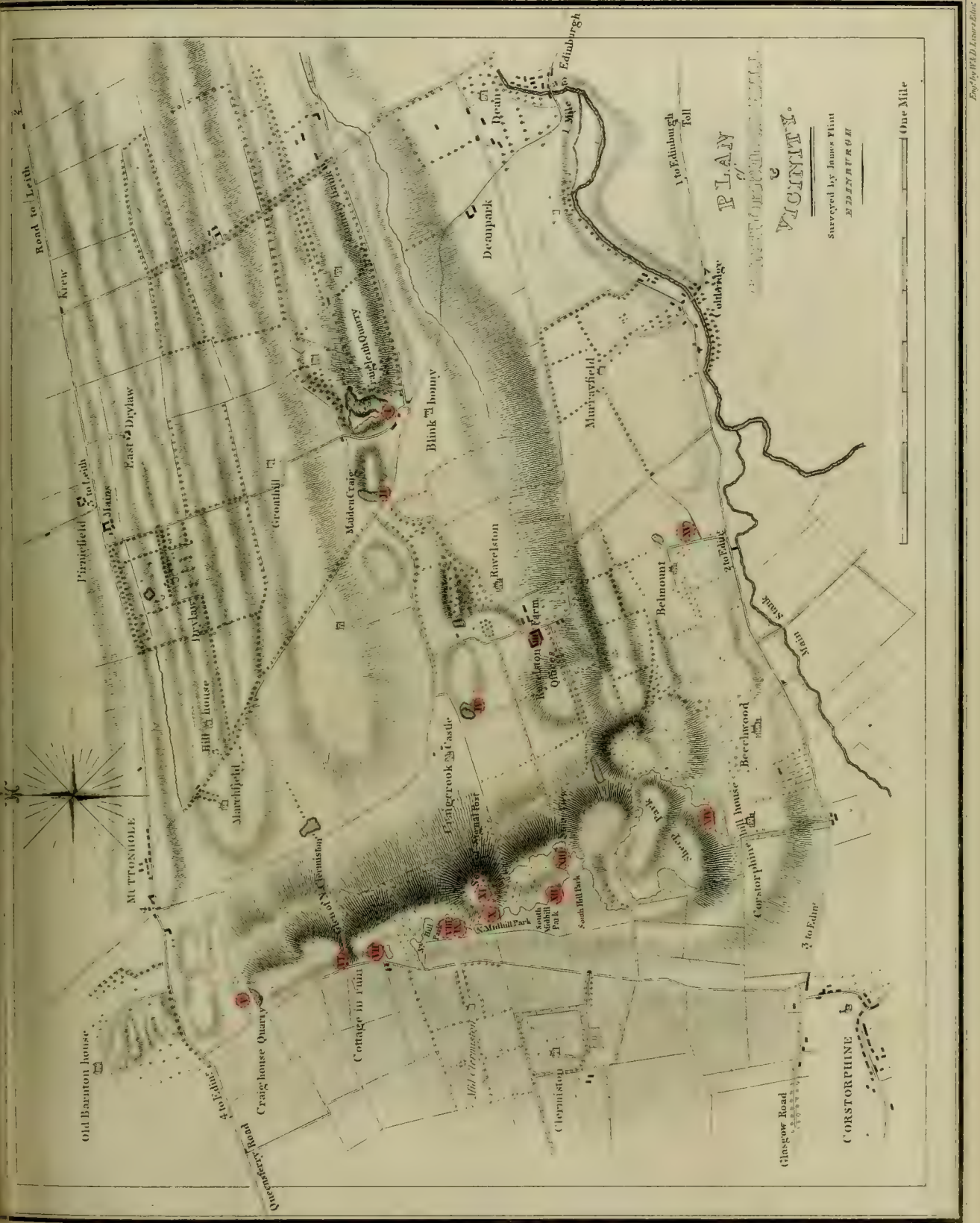




Engr. by H. P. Lister. Edin.

COVE SHORE.





PLAN
OF THE DISTRICTS OF
BROOMFIELD &
CORSTORPHINE
IN THE VICINITY OF
EDINBURGH.

Surveyed by James Flint
1825

One Mile





VI. *An Account of some Geological Facts observed in the Faroe Islands.* By SIR GEORGE STEUART MACKENZIE, *Bart.*
Pr. Ph. Cl. R. S. Edin.

[Read Nov. 2. 1812.]

THE singular appearances which were presented to my view by the Trap Rocks of Iceland, and the interest which they excited, made me resolve, as soon as I had given an account of that country to the public, to visit the Islands of Faroe. This expedition was undertaken, for the purpose of ascertaining, whether, in a Trap Country, where no traces of *external* volcanoes existed, any thing similar to the peculiar features of the rocks of Iceland was to be found. In the latter country were discovered a series of rocks, lying above the beds of Trap, which bore the most striking marks of igneous origin; in some instances having a perfect resemblance to ordinary Trap, and in others to the common Lavas of the country. The beds of Trap, and those above them, being separated by mechanical depositions of Tuffa (Trap-Tuff), led me to the conclusion, that the whole of the beds had been formed at the bottom of the sea, by successive eruptions of a submarine volcano.

cano. The results which I suppose may have attended a submarine eruption at a great depth, will be quoted in the sequel.

The following observations of LANDT, respecting the formation of these islands, contributed essentially to confirm the desire I felt to explore the country. It must be believed, that this clergyman had possessed some previous knowledge of the igneous theories. "It would form," says he, "a curious object of research to inquire, in what manner the Faroe hills have been formed, and how they attained their present elevation; whether above or under water? Whether they owe their height to volcanic explosions, which threw one stratum above another, or whether these strata were deposited upon each other under the water, and were afterwards raised to their present situation by a volcanic eruption, or some elastic force produced by subterranean inflammation; or whether these hills have been formerly covered by the sea, which has since retired back, in consequence of some convulsion of nature*?"

Again; "But in examining the bottoms of the hills along the sea-coast, one will often discover indubitable marks of volcanic eruptions, or of some other convulsion of nature, which has acted a distinguished part in the formation of the singular phenomena which here present themselves to the eye of the curious observer. It is seen in many places close to the water's edge, that the matter of which the rock is formed has been in a state of fusion, and has become hard in its course. Sometimes this hardened matter is smooth on the surface, but has the appearance of the ice on a stream or rivulet, where the water rises above the first crust, and forms several strata, one above the other; but sometimes this hardened matter is rough,
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* LANDT, *Translation*, Lond. 1810, p. 5.

and full of holes and knobs, such as we may suppose would be seen in metal first fused, and then cooled in water*.'

Besides the desire natural to all travellers, to have a friend to partake their labours and enjoyments, the particular object I had in view, rendered it a matter of importance, that I should have a companion, of whose competency to judge upon the spot, of any new fact that might occur, there could be no doubt. On proposing to him to take a share in the expedition, my friend Mr THOMAS ALLAN, whose great experience in geological examination, and his intimate acquaintance with individual minerals, eminently qualify him for research, readily agreed to accompany me.

The islands of Faroe, or North Faroe as they are commonly called, are nearly midway between Shetland and Iceland; and lie between the parallels of latitude $61^{\circ} 20'$ and $62^{\circ} 25'$, extending north and south about seventy-five miles. The meridian of the seventh degree of west longitude divides the group into two nearly equal parts; the extreme points of land on the east and west sides including $1^{\circ} 25'$, or about forty miles. Besides a great many detached rocks, the islands are eighteen in number; the largest, viz. Stromoe and Osteroe, being in the centre. The extent of the former is twenty-eight miles long, and, on an average, six miles broad; that of the latter, about the same breadth, and twenty-two miles long.

The general aspect of the country is mountainous and precipitous; and, while the lofty rocks which frown over the ocean, inspire distrust in those who approach them for the first time, many excellent harbours are to be found, where ships of any size may ride securely.

The principal place is Thorshavn, situate on the east side of Stromoe. It is a small town consisting of wooden buildings,

* LANDT, p. 11.

ings, huddled together on a narrow tongue of land jutting into a bay, and forming two very commodious harbours. The country in the vicinity, though by no means flat, comprehends the greatest tract of comparatively low land to be met with in any of the islands; but it is exceedingly bleak and bare.

There are, in a few places, valleys of very small extent; the separation of the mountains being for the most part narrow glens, and, in many instances, merely the breadth of a small stream.

The soil, in almost every part of the islands, is chiefly peat. No trees are to be seen, excepting one or two stunted mountain-ashes in the governor's garden. This deficiency of trees may be attributed, perhaps, more to the wetness of the soil than to the climate. The whole country abounds in springs; no part of the soil being naturally dry, while, at the same time, it is very shallow.

There are two lakes of considerable size in the island of Vaagoe. One, called Sorvaags Vatn, is about three miles long, and about half a mile broad. The waters of this lake approach very near the coast, which is lofty and precipitous; and, after a short course, fall into the sea, forming a magnificent cascade about eighty feet high. The other lake is on the north-west side of the island, and is about a mile long, and a quarter of a mile broad. It is not named on the chart, nor by LANDT.

In Stromoe there are several small lakes, the most considerable being that near the village of Leinum.

There are but few in Osteroe; the only one of note is that called Tofte Vatn, at the south-east end.

The streams are in general merely brooks; that which issues from the lake of Leinum being the only one which can be considered as a small river. The variety of cascades which these streams form, is endless: and some of them, when
swollen

swollen by rain, are exceedingly picturesque. Many of them fall from such a vast height, that they are completely dispersed into fine spray long before they reach half-way down the precipice.

The mountains present a variety of forms, but tend chiefly to assume that of a cone at their summits, which are often very sharp and rugged. The Northern Islands, Kalsoe, Kunoe, Bordoe, and Videroe, consist of long sharp ridges, the summits being broken into many fantastic shapes. The highest land is in Stromoe and Osteroe, which are separated by a narrow channel, which in one place is scarcely a quarter of a mile broad. The state of the weather prevented our ascending the highest mountain, called Skellingfell, in Stromoe: but we succeeded in reaching the summit of one of the highest mountains in Osteroe, called Slatturtind, near the village of Eyde. By barometrical observation, this appeared to be 2825 feet above the sea. Skellingfell* cannot be less than 3000 feet high, and is probably somewhat more.

The western coast of Stromoe presents an extent of twelve miles of the most sublime rock-scenery that can be conceived. Every part of the Faroe group has its romantic beauties; and there is scarcely a promontory which does not exhibit a scene calculated to excite the most lively admiration. The general elevation of the precipices on the west side of Stromoe, varies from 1000 to 2000 feet. There is a cliff called Kodlen, forming the north-west promontory of Osteroe, which did not strike us so much by its elevation, (for it sunk far beneath the neighbouring rocks of Stromoe), as by the circumstance of its being exactly perpendicular. Mr ALLAN measured its height by

Vol. VII.

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means

* The name of this mountain, and some other names, are variously spelled; but the mode adopted in this memoir conveys the pronunciation.

means of a line, and it was thus ascertained to be 1134 feet high.

Greatly as our admiration was excited by the stupendous exterior of these islands, their internal structure did not fail most powerfully to arrest our attention.

The Faroe Islands are composed almost entirely of Trap, the most common characters of which are amygdaloidal and porphyritic. Beds of coal occur in Suderoe, and, it is said, also in Myggenæs, neither of which islands we could visit, on account of the unfavourable state of the weather, at the time we were about to make an attempt to land upon them. The beds of Trap are inclined at a small angle, about 4° or 5° , and dip towards the south-east*. Their thickness varies; in some, it is but a few feet; and LANDT states that of some columnar beds, which we did not reach, to be from 100 to 300.

The first striking resemblance between the rocks of Faroe and those of Iceland, we observed in the separation of many of the beds of Trap by thin layers of Tuffa, resembling red sandstone. In both countries, this tuffa occurs of a greyish and of a yellowish colour; and sometimes assumes the small columnar form, and then it has a tendency to the texture of Wacke. In Faroe it occurs also of a green colour †.

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* We had a view of Myggenæs, near enough to distinguish that it was composed of beds, which rose at an angle considerably greater. The coal is probably in the same position as that found in the Isle of Skye, near Talisker, where it occurs between beds of trap.

† I have preferred the term *Tuffa* to that of *Trap-tuff*, because I wish to employ a generic term, and one that has no allusion to theory. *Trap-tuff* is, no doubt, used as generic by the Wernerians; but it is in reality a specific term. It is a question whether the specific terms should be derived from

The first example we had an opportunity of examining, of that species of tuffa, immense beds of which form so remarkable a feature of Iceland, was observed forming part of a very curious rock on the west coast of the island of Vaagoe, called Tindhholm. We saw the same sort of tuffa in different places; particularly at the bottom of the promontory called Niepen, in Stromoe. It may also be seen on the beach, near the village, in Naalsoe.

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from the basis or the included masses. As it is not in any case easy, I may say possible, to determine to what specific substance we ought to refer the basis, which varies in several respects, it may perhaps be best, when there is any uncertainty, to describe both it and the included masses; for prolixity is, in every description, preferable to want of perspicuity. *Trap-tuff* implies, either that the basis is derived from Trap, or that the included masses are Trap, or that both have the same origin. Now, as the rock in question often contains sandstone, and sometimes wood; and as in Iceland and other volcanic countries, it contains lava and other volcanic substances; the term *Trap-tuff* conveys none of these important particulars. Correctness seems, therefore, to demand some reform. I would propose that the basis should be understood generally, as Professor JAMESON has described it, to be "rather a loose, spongy, "clayey basis," and that we should relinquish the term *trap*, and use, as a general term, the word *tuff*, or the original and more harmonious Italian *Tuffa*, and describe the included masses. Thus, in Iceland, I met with tuffa, the component parts of which were so minute, that it resembled red sandstone; tuffa including rounded masses of trap, from a size very minute, to many feet in thickness; tuffa including masses of trap, lava, slags, and mineralized wood. Here the description appears to be absolutely necessary to prevent misapprehension; and instead of being at all offensive, it affords perfect satisfaction: whereas, were I to use for them all merely *Trap-tuff*, many important particulars would be omitted.

Objections also arise to the term *Calc-tuff*, which may be understood to mean a calcareous basis, cementing together substances, the nature of which we are left to imagine. The included masses being sometimes entirely calcareous, and sometimes heterogeneous, the term, by itself, conveys nothing satisfactory. For fragments connected by a solid basis, the old term *breccia* seems to be unobjectionable, when qualified, as circumstances may require, by a description of the basis and the included masses.

The Society will no doubt recollect the fact which I described, of the under-surfaces of many of the beds of Trap in Iceland, bearing unequivocal marks of their having been in a state of fusion, of which there are various specimens in our cabinet. On finding the red tuffa separating the beds in Faroe, I expected to meet with some indications of heat in that country also; and I was not disappointed, though their occurrence was not so frequent in Faroe as in Iceland. Near Kirkeboe, we observed a considerable degree of roughness on the bottom of a bed of rock, which resembled some that occurred in Akkrefell in Iceland so much, that the specimens differ only in a shade of colour. At Kirkeboe, such marks of fusion were in many places quite apparent; but I did not succeed in my attempts to procure a mass of a size sufficient to exhibit, in a satisfactory manner, the general feature which is so remarkable. The most perfect I could procure on a small scale, of that slagginess which was so conspicuous in Iceland, was found on the hill of Leinum in Stromoe. I had to regret that heavy rain, and the risk incurred in wandering among high precipices, while surrounded by thick fog, prevented my exploring this mountain so completely as I wished.

But whatever degree of doubt might have been left on my mind by the appearances just described, it was entirely dissipated by a new and most important fact, in relation to heat having operated in the formation of trap, which first presented itself to our notice in the island of Naalsoe, where almost every circumstance of importance in the geology of Faroe is to be seen, and where the greatest variety of the individual minerals which occur in trap are to be found. This island lies opposite to Thorshavn, being separated from Stromoe by a channel about five miles broad. It consists of one mountain, rising to an elevation of









1500 feet; and receives its name, which signifies *Needle Island*, from a perforation in a rock at the south end, thought to resemble the eye of a needle.

The surfaces of many lavas which I passed over in Iceland, were not unlike coils of rope, or crumpled cloth; an appearance which we should expect to be assumed by any viscid matter in motion. On our first visit to the Island of Naaloe, we observed the surface of a bed of amygdaloid, which had been exposed to a considerable extent by the removal of the bed above, exhibiting an exact picture of the lavas I had seen in Iceland. At first sight, this discovery forced instantaneous conviction on the minds of those who were with me, none of whom had ever seen lava, that heat must have caused the appearance before us. We brought away a number of specimens, which are now before the Society, and which speak a language not to be misunderstood. (See Plate I.)

We afterwards discovered varied examples of this crumpled surface in different parts of the country. In the vicinity of Eyde in Osteroe, there were many instances, in which the matter appeared, as if, in a viscid state of fusion, it had flowed and spread itself out. Of this a specimen is before the Society, sufficient to explain the fact, though it does not afford so good a display as we could have wished: our attempts to raise large masses entire having failed. (See Plate II.) We found that one bed was exactly moulded on another, and had every appearance of the lower one having become hard before the next had flowed over it. This was observed at various elevations. Near Waii in Bordoe, we saw several examples; some at the height of 1000 feet above the sea. It is to what has been described, no doubt, that LANDT alludes in the passage quoted in page second.

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It has been observed to me, that what has been remarked on the *upper* surfaces of the beds of Faroe, cannot be reconciled with the ideas I had formed respecting the manner in which the slagginess of the *under* surfaces had been produced. Though all I wish to contend for, in either case, is, that heat had operated, I request the indulgence of the Society, while I shortly endeavour to show, how a lava, flowing at the bottom of the sea, might also assume the marks of fusion on the upper surface.

The effect of water on the surface of metals in a state of fusion, is well known to be the production of wrinkles. This is the case also when any viscid substance, whether fluid by fusion or otherwise, becomes gradually solid while in motion. In the case of water applied to the surface of a hot body, the phenomena are familiar to most persons. When, for instance, brass is in fusion, a few drops of water poured upon it remain suspended almost without motion; but as the heat diminishes, the water approaches nearer and nearer to the hot surface, and at length coming into contact with it, violent ebullition and quick evaporation take place. In the case of a lava erupted at the bottom of a great depth of sea, a stratum of steam must be produced, continuing, while the heat exceeds a certain degree, to keep the water and the hot mass separate. In the operation of boiling the mercury in a barometer tube, an event analogous to this takes place, and we see the column of mercury raised up by the vapour occupying the lower end. As the heat of a stream of lava flowing under the sea is reduced, and the lava itself becomes viscid, its motion, combined with the action of the water coming into contact with it, will infallibly produce a surface full of wrinkles; and therefore we should expect to find the upper surfaces

surfaces of submarine lavas uneven and rough, as in the examples before the Society.

In considering this subject, it ought to be observed, that steam is not like permanently elastic fluids, which, when they escape, overcome any pressure of water, and rise quickly to the surface. In the case of water above a greatly heated surface, there is a constant production, as well as a constant condensation of steam; and the water itself may, to a considerable extent, become greatly heated. But steam produced under the lava, by its flowing over a wet, and, if tuffa be present, over a spongy surface, would act in a very different manner, as I have shown in my account of the Mineralogy of Iceland. It will have a tendency upwards, and will act upon the hot mass, rendering it more or less vesicular, according to its degree of fluidity*.

Thus

* “ When the lava is very hot, and consequently very liquid, the steam will have less difficulty in penetrating it than when it is viscid. We may conceive cases in which the lava burst forth in such a high state of liquidity, as to permit the whole of the moisture to pass through it in the form of steam; in such a state of viscosity, as to admit of its escaping very slowly, so that the lava may become solid, and, by confining the steam, more or less vesicular †; and, lastly, so tough, that the exertions of the elastic vapour shall be confined entirely to the lower surface of the lava. In the first case, a compact mass of stone would be formed, having no appearance of the action of heat; in the second, on account of the pressure of the superincumbent water being sufficient to prevent the escape of carbonic acid and other volatile ingredients, a vesicular and amygdaloidal mass would be produced; and from the last would result a mass entirely compact, excepting at the under surface.”—*Travels in Iceland*, chap. ix.

To this passage of the text the following note is subjoined:

“ In such a case, it is possible that the steam, when condensed, would, in some instances, remain confined in the stone in the form of water; and thus
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Thus it appears, that the marks of fusion, exhibited by the upper, as well as the under surfaces of the beds, are equally reconcileable to the supposition of their having flowed in a state of fusion at the bottom of the sea.

Another point of resemblance between Iceland and Faroe remains to be noticed. LANDT, in his description of Faroe, (which, as far as I had the opportunity of judging on the spot, is a very faithful one, though his translator does not always do him justice), mentions great fissures which can often be traced from one island to another. We found that these fissures had been originally entirely filled with veins of basalt; and in those only did that rock occur to our observation. Being columnar, and the columns being perpendicular to the walls, the basalt is easily broken down; and water-courses being formed, the veins are soon worn away. On examining the sides of the veins, we found, though not in every instance, a vitreous coating, similar to what was observed on those of Iceland.

Thus, the perfect resemblance between the trap rocks of Faroe, and those of Iceland, seems to be completely established. The beds are separated by the same substance, viz. layers and beds of tuffa. They present indications of fusion; and Faroe has afforded some of a new and very decided character. I have heard that similar appearances exist in Norway; and it is not improbable, if geologists take the trouble of looking for them, that such will be found in every country wholly composed of trap; and that the igneous origin of that
genus

the fact of water being found in the vesicles of basalt and other rocks, may be accounted for. It must be observed that the steam, in such circumstances, must have been very much condensed, so much, indeed, as to be almost in the state of water greatly heated; much more so than in the familiar experiment made with Papin's digester."

genus of rock will be established beyond a doubt. The discovery of submarine lavas, the idea of which first struck the celebrated DOLOMIEU, serves as an additional illustration of the magnitude and various exertion of that power, which, when subjected to the control first conceived by the ingenious HUTTON, explains almost every phenomenon of the mineral regions.

While the great point, the action of heat in the formation of trap rocks, seems to be demonstrated, forming a theory of the manner in which heat has operated in particular cases, is, in a general view, perhaps not absolutely necessary in the present state of geology. Yet it is satisfactory to the mind when any explanation, consistent with the laws of nature, is suggested. The theory of submarine volcanoes is not meant to be extended to the appearance of beds of trap interspersed between strata of sandstone and other rocks. These have been thrust among the strata from below, and have probably been the cause of the strata being elevated above the sea, in which they were formed. The succession of beds of trap, one above another, is the fact which the theory of submarine volcanoes is intended to explain; and though it may not be thought to afford grounds for conviction, yet, in a case where the agent employed is of unlimited power, the imagination is led, by this theory, into a scene on which it can dwell with some degree of satisfaction. Some affect to view geological theory in the light of idle speculation; while others, of whom I am one, consider it the main spring of research, and the direct road to discovery. There are three descriptions of geological facts of importance to theory; those which support it, those which are difficult to account for, and those which can be explained by the operation of agents opposite in their nature, as fire and water. In proportion as the first accumulate, the second present fewer

obstacles ; and the last, which may be called neutral, are associated with the first. Difficulties remain to be overcome both in the Huttonian, and in the Wernerian system ; and unless the partizans of each were zealous in applying their doctrines to the facts which they discover, the face of nature would cease to be referred to for those steps by which alone we can hope to arrive at a perfect system : and our progress would be so slow and heavy, that prejudice might take possession of our minds, so as to shackle, if not to exclude, the free exercise of reason.

Rocks such as those of which the Faroe islands are formed, are very liable to destruction by the operations of the atmosphere. The great abundance of springs, which is characteristic of a trap country, aid the action of frost. Accordingly, the whole of Faroe exhibits extensive marks of the constant and destructive operation of these agents. The sea, too, rapidly undermines the precipices, large masses of which are daily buried in it. As all the narrow channels which separate the islands, lie in the same direction, and as veins of basalt are sometimes seen as if branching from them through the adjacent islands, I am induced to suppose, that the separation of the islands has originated in the destruction of large veins, subsequent to the land being elevated above the sea, and caused in the same manner as that of other veins or dikes, now going on. The support of the walls being removed, decomposition, and the effects of moisture and frost, would operate in gradually destroying the beds, and at last the sea, breaking through (as it is constantly doing in various parts of the islands), these channels might in this manner have been formed. In the same way, the position, in some instances, of those huge masses which now stand separated from the coast may be accounted for. Indeed, in several places, we saw the separation actually proceeding, by the removal

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val of veins, particularly near the village of Tiornevig in Stromoe.

An account of some other geological facts, of the varieties of the trap, and of the various minerals contained in them, I leave to my friend Mr ALLAN, whose great accuracy in such descriptions is well known to the Society.



FEROE ISLANDS

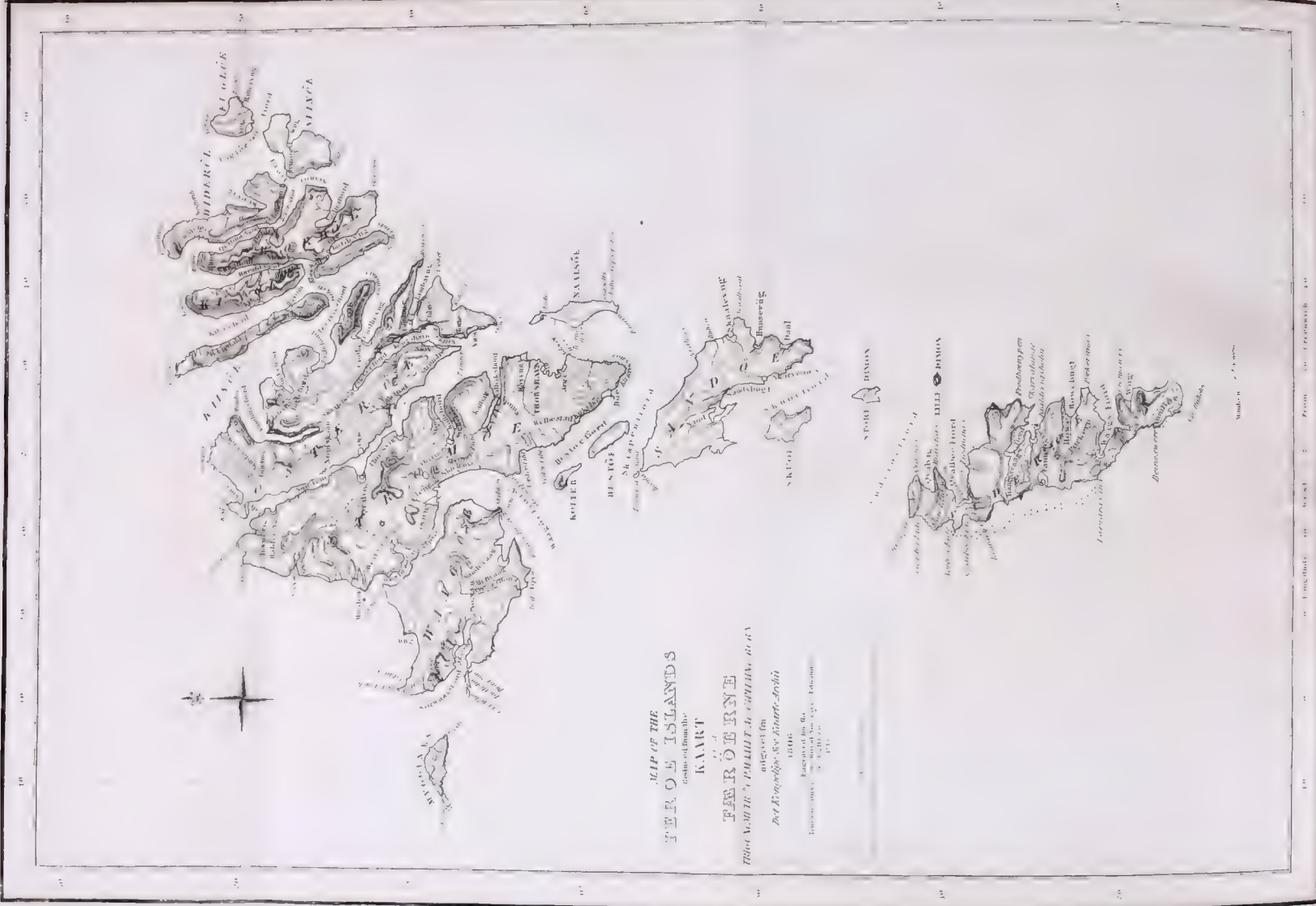
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MAP OF THE
FEROË ISLANDS
 (continued from page 10)

FEROË ISLANDS

FROM A MAP BY PAUL H. A. SCHUBERT, 1874
 (continued from page 10)

By the Hon. Secy. of State, 1874

Engraved by G. W. Colver, 1874

Published by the U.S. Government, 1874

Scale, 1:100,000

VII. *An Account of the Mineralogy of the Faroe Islands.*

By THOMAS ALLAN, ESQ. F. R. S. EDIN.

[Read 1st February 1813.]

SIR GEORGE MACKENZIE having already given a paper, entitled, "An Account of some Geological Facts observed in the Faroe Islands," it may be necessary to explain why our communications were not combined in one. This was simply owing to our objects being in some measure dissimilar. He was anxious to compare the facts presented in a country decidedly volcanic, with those in a trap district, where no traces of a volcano were to be discovered; consequently, his observations were confined to particular facts: whereas my object is to describe, without relation to theory, whatever appeared to me interesting in a geological point of view.

I do not propose to embarrass myself with an attempt to reduce the various phenomena I remarked, to any existing theory, farther than what appears warranted by the new and additional light we derived from some of the appearances in Faroe. I shall content myself with aiding the labours of future travellers,

travellers, by enabling them to profit by our experience, and by removing the difficulty we encountered in procuring information, regarding the objects calculated to gratify curiosity, or assist us in our investigations, which neither of the accounts of Faroe published are capable of doing*.

In the following pages, I shall take notice of the objects most deserving of attention, and particularly note the localities where minerals are to be found; and conclude with a few observations on the geology of the islands.

To this I shall not attempt to add a description of the inhabitants, their mode of life, or their means of subsistence, although all are peculiar, and all interesting. It would be temerity to attempt it on an acquaintance of five weeks, although that time was sufficient to satisfy us, that they are a people in all respects honest, industrious, and hardworking, who earn their scanty livelihood with more labour than perhaps any other set of human beings, while nothing but patience and contentment appears to prevail among them.

In our voyage to Faroe, we were extremely fortunate; although the weather was boisterous, the wind was fair and steady, so that we cast anchor at Thorshavn exactly in sixty hours after passing the Isle of May,—a run of about five hundred miles. When we first made the land, the atmosphere was so thick, that it was some time before we discovered the island we were approaching to be the Lesser Dimon, situated between Suderoe and Sandoe. The form of this rock is similar to that of Ailsa; it gave us a glimpse of the trap country we were going to ransack, and as we approached our anchorage, other magnificent cliffs successively displayed themselves, affording some idea of the grand scenery we were about to visit.

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* The only publications on the Faroe Islands are those of DEBES and LANDT. The former appeared in 1670, the latter in 1800.

Having established our head quarters in the house of the Governor, Major LOEBNER, who entertained us with the greatest hospitality, we engaged a servant and a guide, who both of them occasionally acted as interpreters; and the weather being unfavourable for commencing our journey, we delayed a few days, in hopes of its improvement. We had selected June, as a season most likely to afford us comfortable weather; but during the whole month, we had not two days together that it did not either rain or snow. August and September, we found, were considered the most favourable periods. During this delay, we had time to inform ourselves of the best mode of travelling through the country.

In Faroe, the term *road* means little more than direction, as not even a path is to be discovered in some of the principal routes, which are merely the most passable cuts across the hills: occasionally a priest may save himself bodily labour, by traversing these wastes on horseback; but a stout man will generally accomplish the same distance in less time on foot.

Where a country is so entirely intersected by the sea, inland communication is but seldom resorted to, nor would it at all answer the purpose, where such constant intercourse is required; the inhabitants of the most distant parts being obliged to repair almost weekly to Thorshavn, to draw their little portions of grain from the Government store; where the stock is always so stinted, never more than a fortnight's allowance is delivered at one time. Besides, all the habitations are situated on the coast. Boats, therefore, afford the best mode of travelling, and the only means of conveying goods. Even to a stranger, there is no inducement to traverse the interior, which presents nothing but dreary desolation. All the striking scenery is on the coasts, and there only are minerals to be procured; for although LANDT mentions, that the best zeolites,

lites he got, were found among the debris upon the sides of the hills, those we picked up in such situations, were uniformly deprived of lustre, and otherwise much damaged.

The boats are principally used for fishing; they are all constructed in the country, of plank brought from Norway. They are built in the canoe shape, and though not very commodious, are so admirably fitted to the seas of Faroe, that I do not recollect, during the whole of our excursions, to have been in the least incommoded by the rising of the spray. The continual practice of the natives, and their constant habits of attending to the tide, not only for the purposes of their avocations, but also to mark time, when the sun is obscured in clouds, renders them extremely expert in the management of their craft; yet they never venture their boats to sea without having them well manned,—a precaution extremely necessary, where, by the rapidity of the currents, and the sudden gusts of wind, the waves are thrown into the most violent agitation almost instantaneously.

The Faroe Islands being so celebrated as the source of the finest zeolites and calcedonies, which decorate the cabinets of Europe, I there expected to find a perfect magazine of every thing magnificent of that nature; and never doubted that we should meet with people in plenty, who, if they had not objects of this description to dispose of, would at least be able to conduct us to the places where they were to be procured. I was therefore surprised, that not one person in Thorshavn could give us any satisfactory information on the subject. Such is the indifference those beautiful productions of nature, so justly prized abroad, meet with in their own country. Our guide, HANS, who had also attended Sir JOHN STANLEY in the same capacity, told us, that that gentleman had supplied himself with zeolites from a cave in Nalsole, about twenty-three years before; but that he had
not

not been in it since. Knowing what a hopeless measure it is to explore a country in quest of its mineral productions, without being previously directed to the particular spots that afford them, this information occasioned considerable chagrin; and as it appeared we had only our own exertions to depend upon, we commenced operations with all possible speed.

That part of Stromoe which surrounds Thorshavn is tamer than the generality of the country, and, so far as we could discover, presents nothing of interest to the mineralogist. We therefore immediately proceeded to explore Nalsoe, a long narrow island which lies within five miles; and here we found a constant source of amusement*.

Almost the very first object that attracted our attention on landing, was the very remarkable appearances of fusion, on the surface of a bed of amygdaloid, mentioned by Sir GEORGE MACKENZIE. These occurred on a point as nearly due east as possible from Thorshavn. Two miles south of this is the cave in which Sir JOHN STANLEY had found so many brilliant specimens of zeolite. It is described by LANDT as a very remarkable cavern, not, however, for its productions, of which he takes no notice, but as leading to a perforation said to pass so nearly through the island, that the noise of the waves on the opposite side may be heard at the extremity of it. This aperture I could not find; it may, however, be covered with debris, the place being so much altered since HANS our guide was there before, that he scarcely knew it to be the same.

It is situated considerably above the level of the sea, in a soft amygdaloid, and presents an opening of about two hundred feet in length, while its inmost recess does not exceed

VOL. VII.

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eighty,

* In the annexed map of the Faroe Islands, reduced from that of Captain BORN, I have traced our different routes.

eighty; in front it is nearly closed up with debris, which slope gradually into it, so that it is in many places quite low. Here, both in the solid rock, and among the fallen fragments, we found specimens of zeolite in abundance, of the species Stilbite, Apophyllite, and Chabasie.

The first of these was the most abundant, and occurred very beautifully crystallized, in irregular crevices, disposed in groupes, on globular mesotype, mixed with minute crystals of chabasie, and of the following forms:

1. Flat rectangular prism; the broad surfaces of which have a lamellated and pearly aspect, while the narrow ones are longitudinally streaked, with smooth shining terminations, set at right angles to the rest, forming a parallelepiped.
2. The same; having each of the solid angles replaced by two planes, set obliquely on the narrow sides of the prism, presenting the form *dodécacèdre*, fig. 178. of HAUY.
3. When the replacement of the solid angles is not complete, part of the terminal plane of the first variety remains. Form *epointé*, fig. 179. of HAUY.

Besides these, we found it in radiated and lamellar masses, and in aggregations assuming the sheaf shape.

The Apophyllite is a rarer mineral; we found it only among some of the fallen masses of rock, disposed on a ground similar to the last, and mixed with crystals of stilbite. The forms it assumes are very simple, and as follows:

1. Equilateral rectangular prisms; terminated at each end by a plane set at right angles. Here the sides of the prism

prism are all streaked, and the terminations pearly; these crystals appear to have but one cleavage, which is at right angles to the axis, and extremely distinct.

2. The same; slightly truncated on all the solid angles.
3. The same; with the truncation on the angles somewhat deeper; a small four-sided facet remaining on the summit, forming a truncated pyramid.

In other specimens I have found the apex complete, producing a very distinct sharp-pointed pyramid*.

We here likewise found radiated and amorphous mesotype; also in most delicate minute acicular fibres.

Chabasie occurred here only in very small crystals; but towards the southern extremity of Nalsole, we found it in very large crystals, some of them at least an inch in diameter; they were imbedded in a very tough rock, and, being naturally brittle, we were unable to detach them.

From the southern point of Nalsole, a portion of the rock was removed some years ago to Kongsberg in Norway, for the purpose of extracting the native copper dispersed through it; this, however was not found to answer, from the small quantity it produced. Another spot was also pointed out to us as affording copper, on the east side of the island, a little north of a small detached rock called *Kabelen*, and not very distant from where we first saw it.

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* This variety has been referred by HAUVY to the species Mesotype. From the great respect I entertain for the opinions of that distinguished mineralogist, it is with reluctance I venture to place it under a different name. It is not, however, without due consideration that I have done so: my reasons I shall detail more fully, when speaking of the Crystallised Mesotype.

I here had the good fortune to procure some very good specimens of the metal, in rather larger bulk than usual, very beautifully crystallized, with some of the sprays forming the nuclei of radiated mesotype; which, when considered in a geological point of view, is a circumstance highly deserving attention. Here the native copper is found in amygdaloid, at no great distance from the spot where we saw a bed of the same material covered with the most decided marks of fusion. It does not traverse it in veins, but is disseminated in minute particles, and sometimes presents crystallizations, equal in beauty to any from the veins of Cornwall, although imbedded in the solid substance of the rock, through which it branches with the utmost elegance and freedom.

The places I have mentioned as the localities of these minerals, with one or two others, rather of less note, were the only spots we landed at. There were many others, which appeared sufficiently inviting; but from the roughness of the waves, and the quantity of sunken rocks, we could not with safety approach them.

Having delayed some time, vainly waiting for good weather, we at last proceeded to the western coast of Stromoe, and took up our lodgings at Quivig, in the house of the priest, Mr HOLM. During our voyage to this place, the mist lay so thick upon the islands, that we seldom could see the summit of the cliffs, near which we were obliged to steer, in order to keep our course.

Our host at Quivig, was the only person whom we met with in Faroe, in the habit of collecting its mineral productions; and although he did not appear to have made a very good selection for himself, yet he conducted us to different cavities on the shore, from which we obtained some of the finest zeolites that ever were brought from these islands. Some of the
cavities

cavities were three feet in diameter, and entirely lined with large crystals of stilbite; but the most interesting, were between Quivig and Westmanhavn. In one, we observed the mesotype, in long acicular crystals, but so extremely slender, that it was quite impossible to detach them entire from the rock. In another, we found the same substance, but in a more tangible form. This cavity was about two feet wide, eighteen inches deep, and nine high, in a perpendicular rock, about ten feet from the base, which was washed by the tide. By means of a ladder, we succeeded in reaching this repository, and found the whole interior coated with aggregated groups of stilbite, having only the crystallised acuminations visible, of an opaque yellowish-white colour, and varying from an inch to an inch and a half in thickness. Upon this ground were disposed numerous groups of mesotype, radiating from a centre, and shooting from the surface, an inch to an inch and a half in length, in clear, transparent, well-defined prisms, of a rectangular form, terminated with a flat four-sided pyramid, the variety *pyramidée* of HAUY, and varying in size from a hair to a line in thickness. It was a mortifying circumstance to be obliged to destroy any part of this very magnificent specimen, in detaching it from the rock; a circumstance, however, totally unavoidable. I succeeded in obtaining several very good specimens, and by gluing them carefully to the bottom of a box, was very fortunate in preserving them in all their natural beauty.

This is the Mesotype of HAUY, and the Nadelstein of WERNER; it varies entirely from the Apophyllite formerly noticed; in place of a distinct foliated cleavage, cutting the axis at right angles, with a pearly lustre on its terminations; it presents a vitreous fracture, without the appearance of any regular cleavage, and an uniform lustre in all directions. Fragments of Mesotype dissolve in nitric acid, and form a clear transparent

rent jelly; while the Apophyllite separates into minute particles, of a semitransparent white *. It besides varies in refraction, and is in all respects similar to the Mesotype from Puy de Dome, a magnificent specimen of which I had lately the honour to receive from the hands of Monsieur HAUY.

On the east side of the Bay of Westmanhavn, we found other cavities containing Mesotype. We there also discovered a vein of calcareous spar, about two feet in width, from which we detached several specimens; it was entirely composed of a congeries of crystals, among which were imbedded, rounded masses of amygdaloid. The forms here presented by the carbonate of lime are quite new to the mineralogist, and are particularly described in the late work of the Count de BOURNON, who has added to his already numerous collection of the crystallizations of that substance, no less than eleven varieties. To his interesting work I beg leave to refer for their detailed description †.

Having been compelled to abandon our intention of ascending Skeelingfeld, from the state of the weather, we were likewise advised not to attempt the open sea, by which we proposed to proceed on our journey to Osteroe. We therefore passed over to Wagoë, and paid a visit to the island of Tintholm.

The peculiarities of Tintholm are not mentioned in the translation of LANDT, although fully described in the original.

* The Apophyllite of Utö in Sweden, as well as of Disco in Greenland, after separating in the acid, swell out in a very remarkable manner, occupying a space very much larger than the original; but do not combine into a gelatinous mass.

† *Catalogue de la Collection Mineralogique du Comte de BOURNON*, London, 1813.

nal. This little island forms a continuation with the west side of the Bay of Sorvaag, but is not seen from the village of that name. We there obtained a boat, to proceed towards it, and in going down the bay, which is very narrow, we came suddenly in view of it, with all the singular and grotesque rocks by which it is surrounded. One of these is perforated by the sea, forming a very fine natural arch; while another, like an enormous column, placed crooked on its base, stands as if prepared to fall into the abyss beneath.

The summit of Tintholm may be from five to six hundred feet above the level of the sea; it is divided into a number of pinnacles, so extremely slender to appearance, that it becomes a matter of surprise how they resist the fury of the storm. This peak presents on the south side a bare perpendicular cliff, and appears to be principally composed of amygdaloid. The only observation we made here, was with regard to the extreme similarity of the trap-tuff to that which Sir GEORGE MACKENZIE met with at Akkerfeld, and in other parts of Iceland. It contained no minerals of any note.

Here, for once, the weather favoured us; and although the cliffs of Wagoë were not more elevated than others we had previously seen, they were disincumbered of clouds, and when viewed from the farther extremity of Tintholm, formed a most sublime prospect. At the time, we could not sufficiently estimate their altitude; but the diversity of form into which their summits were broken, and the fine contrast afforded by the intervening rocks, with the beautiful effects of light, altogether rendered our visit to this place extremely gratifying, particularly, as we were not at all prepared to meet with any thing of the kind; and we left it with a feeling, that overlooking Tintholm, would have been the omission of an important object of curiosity, in a tour to Faroe.

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Next morning Wagoë was obscured in heavy clouds of thick mist. When the tide served, we left Midvaag, on our return to Thorshavn; but had no sooner fairly quitted the shores of this island, when, emerging as it were from darkness, we had a view of Skeeling, and the other mountains of Stromoe, with the islands of Hestoe, Kolter, Sandoe and Suderoe, none of which had been visible as we passed them before.

It very frequently happens, that one island is enveloped in fog, while the next is quite clear of it. The clouds take their partial stations with inconceivable tenacity; and, according to the direction of the wind, it was usually known what sort of atmosphere was enjoyed in the remote quarters of the group.

From Thorshavn we proceeded on a second tour, to Eide, a small village situated at the farther extremity of Osteroe. The channel which divides the two principal islands is very narrow in some places, and cannot be passed at all times of tide, even in the small craft of the country, being traversed by a reef of rocks near the middle. The hills are generally tame on both sides of this passage, excepting at Zellatrae, where there is a magnificent bed of columnar greenstone.

At Eide we remained some time; but our proceedings were still sadly impeded by the weather; we, however, had an opportunity of ascending Slattertint, a neighbouring mountain, apparently not much lower than Skeeling.

According to Captain BORN, the last-mentioned is only 2400 feet high. From the very excellent chart constructed by that gentleman, after an accurate survey of the islands, in which even the shading of the mountains is executed with great correctness, we ought to state with diffidence, that our measurement of Slattertint, by means of a barometer, indicated an elevation of 2825 feet; from its summit we could discern,
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that Skeelingfeld was considerably higher, and, according to our estimation, not less than 3000 feet above the level of the sea. We had no opportunity of repeating our observation; but as we took all possible pains to guard against error, we have room to suspect that Captain BORN must have fallen into some mistake; although, from the opportunities of observing, which his long residence in the country must have afforded him, we cannot, without much hesitation, come to such a conclusion.

In this excursion we had reason to congratulate ourselves on our good fortune. The atmosphere was clear of clouds, consequently almost every island in the group was within sight, which, from the wonderful variety in the form of the hills and islands, affords a very grand and striking prospect.

The peculiar delight one feels in the enjoyment of a boundless view from an elevated situation, was, on this occasion, considerably enhanced by the unobscured horizon, and peculiar brightness of the day; since our arrival in the country, we had not till now enjoyed a single hour, that would have permitted us to see half the extent. Though bleak and barren, and in many places covered with eternal snow, the novelty of shape, which varied in every hill,—the deep indentations of the sea,—the contrast of the irregular outline with the even and unvarying direction of the rocky beds, and the placid stillness of the surrounding ocean, all contributed to repay us most amply for the trouble of our ascent.

Like many of the Faroe hills, the summit of Slattertint is flat, and presents an irregularly oval plane, of sixty yards by thirty-six in its greatest dimensions. This surface is covered with thick moss, under which the soil was completely frozen. We observed nothing remarkable in the composition of the beds, and no minerals of any consequence.

The village of Eide is situated on the acclivity of one of the headlands which presents a perpendicular front to the ocean. Some of these we had previously been much struck with; that of Nypennæs puzzled us extremely. LANDT represents it to be 1200 feet high, which, from the magnitude of every thing around, the little distance we thought we were from it, and without any object by which we could form an estimation, we could not bring ourselves to believe. I was therefore determined to ascertain, by the simple mechanical means of a line and plummet, the height of the Kodlen, as the headland near Eide is called, and succeeded, by letting down a fishing-line, with a stone at the end of it, from the top of this formidable precipice, from which I ascertained its height to be 1134 feet *. This, I believe, is a very near approximation to the truth: the elasticity of the cord was the only material source of error; but I endeavoured to counteract this defect, by stretching the cord sufficiently when it was measured off.

We were thus furnished with the means of forming a proper estimation of these tremendous cliffs, and without it we should in all probability have left the islands under a very different impression regarding them than we did. The Kod-

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* It was not till after the third attempt that I was satisfied with this experiment. In the two first I met with unlooked for difficulty, by the hitching and entanglement of my cord, upon the projecting points of the rock, and also by the severity of the blast, which, striking on so large a surface, blew upwards with tremendous fury, even when there was otherwise apparently but little wind. I consequently bethought myself of coiling a given quantity of cord, upon a round stone, and forming a compact mass, of sufficient weight to overcome the violence of the wind opposed to it at the summit, which decreased proportionably to the opposition it met with in descending; while the shape, and unfolding of the rope, rendered it less liable to attach itself to the points of the rock. By means of this device, I succeeded to my satisfaction.

len is by no means remarkable for its elevation; the Myling, in Stromoe, is at least a third higher, presenting an unbroken mural front, with scarcely a point for a bird to perch upon from the summit to the base.

On the west side of the promontory, opposite Thiornivig, and also on the east side, upon the shore, we observed numerous indications of fusion upon different surfaces of the rocks; and it was from this vicinity that we procured the principal part of the specimens, bearing testimony of this fact, that we brought home with us. It was among some large fragments, also on the east side, that we discovered specimens of apophyllite altogether rare for the magnitude and beauty of crystallization.

They here occur in rectangular prisms, often in perfect cubes, sometimes longer, sometimes shorter; occasionally, the cubes are truncated upon the solid angles, either slightly, or so deeply as to meet, forming the regular *cubo-octohedron*. The crystals, like those formerly mentioned, which we found in Nalsole, are streaked on the sides longitudinally, and have a pearly aspect on the terminations; they are disposed on a ground of mamillated mesotype, and in some instances grouped like fluor-spar; the cubes being irregularly implanted on each other. They are perfectly transparent and colourless, except when affected by the weather, which renders them dull and opaque. The largest I met with measures seven-tenths of an inch, by five-tenths. I was assured by M. VARINA, a Spanish mineralogist, and a distinguished pupil of WERNER, that it is to this variety he gives the name of Cubizite, although it is under that appellation the Analcime and Chabasie of HAUY are described by BROCHANT and others. It may be, therefore, that the Cubizite of WERNER, is in fact the Apophyllite of

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HAUY, and that WERNER's Nadelstein should be confined to HAUY's Mesotype.

The only other circumstance of interest which I met with near Eide, was a remarkable instance of the abrasion of the surface, where the rock appears to have been worn down by the friction of heavy bodies. Of this I shall take notice in the sequel.

Again, we were obliged to abandon our station, without completing our intentions, in consequence of the badness of the weather; and being deterred from venturing to the northern islands in this direction, we returned to Thorshavn, resolving to attempt it by the south side; and after some delay for a favourable day, we reached Waaij in Bordoc, with tolerable prospects of success. Here we had just landed, and prepared ourselves for our afternoon's excursion among the neighbouring hills, when it began to rain heavily, and continued pouring for fifty hours, accompanied with sleet; on the third evening a shower of snow commenced, and next morning the mountains were clothed in white almost to the water's edge. It was now the 23d day of June, and yet this inclemency created no surprise among the natives, who assured us, that at Christmas the weather was generally better. The recent snow did not lie long, and, anxious to do something before we left the place, we ascended a hill which lies on the west side of the village, while it yet snowed, and were repaid for our trouble, by observing marks of fusion, similar to those of Eide, but at a height of at least 1200 feet. We had hitherto found them only close upon the shore.

Despairing of being able to proceed farther, and as our provisions were not calculated for so long a delay, we were compelled to return to Thorshavn, and busied ourselves in directing preparations for our final departure. At this time the weather suddenly

suddenly assumed a more settled appearance than it had hitherto done; and having still a few days at our disposal, we resolved to make a last effort, to visit the objects we had hitherto been prevented from seeing. We had now to retrace a great part of our former track; but the difference of weather rendered it entirely new to us. Formerly, as one cloud followed another, we caught an occasional glimpse of the coasts along which we were steering; now we had a delightful view of all the islands and groups of mountains, perfectly unimpeded; and their rough surfaces afforded as magnificent effects of light as can well be imagined. One of the most extraordinary of these, is in the island of Wagoe. On the north side of the entrance to the Bay of Midvaag, there is a fragment of a rock standing out from the rest, to which, from its acute pyramidal form, they have given the name of the *Trolkende Fingeren*, or Witch's Finger. As we approached it from the extremity of Strømoe, the light struck upon it, and the rocks in its vicinity, exactly so as to produce the appearance of a Gothic cathedral; an appearance not requiring fancy to help it out, but actually demanding attention to overcome the illusion. We had passed close under this rock twice before, but had not till now seen it.

We again passed our former residence at Quivig, and reached Westmanhavn soon after noon. Next morning we were ready to start on our long meditated expedition at an early hour. The weather was fine, and the water in the bay as smooth as any lake; still the people wished to dissuade us from venturing upon the open sea, asserting that we should find it very rough without. We now began to be less moved by their representations than formerly, having observed their extreme caution, or rather timidity, on all occasions. We determined to proceed, and although the sea was certainly more agitated than

than we expected, all inconvenience it occasioned soon absorbed in the stupendous grandeur of the scenery that opened upon us. From the northern coast of Wagoe, all the way to Eide, we had a continued series of magnificent cliffs, towering in many places, like minerets, to the height of 2000 feet. Here also we saw a series of rocks, which, from the accounts contained in LANDT, had excited a great degree of curiosity: although they do not by any means accord with his description, yet they present some of the wildest and most magnificent appearances possible. Here a part of the coast is separated from the rest, and appears to have formed a barrier or wall in front, the ruins of which now only remain. In one place, where the largest portion stands highly elevated from the water, an immense perforation, like a grand portal, presents itself, through which boats of any size may row in safety. The rest of this reef, as it may very properly be considered, presents a string of fantastically shaped rocks, many of them acute pyramids, denominated *Drengs* in the language of the country, in a line at unequal distances. In a little narrow bay near this, of about three or four hundred yards in depth, there is another wall, somewhat similar, dividing the bay in two; the extremity of which ranges with the entrance of it. The end towards the sea is highest; and from this it declines to the other, being ornamented all along with the same kind of pinnacles we formerly admired at Tintholm. In it we also found a natural arch, through which we passed in the boat. These walls, I first suspected, might have been the remains of enormous dykes, similar to some which exist on the coast of Antrim; but on examination, I found no traces to support that conjecture, nor could I discover any probable cause for their separation from the adjoining country.

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We arrived at Eide in the afternoon; and from thence proceeded, early next morning, towards the northern islands, directing our course to Wideroe. From the general appearance of the country, and the perfect sameness we every where met with, in its geological characters, we found there was little inducement to prolong our stay in this quarter, and therefore resolved, if circumstances permitted, to return the same day to Thorshaven.

The morning was beautiful; and as we skirted along, we had a delightful view of the coast, which presented many of the singularly shaped rocks, of which we had seen so many the day before. Being obliged to keep out to sea, to get into the current, we had an opportunity of comparing the relative heights of the Myling and Kodlen, and to form a very accurate judgment of the former, by knowing the altitude of the latter. We now passed along the islands of Kalsoe and Kunoe, which are little more than lengthened ridges of barren rocks, presenting the most frightful aspect of sterility. We arrived at Wideroe a little before mid-day; it is situated on a peninsula, the isthmus of which is of considerable extent: on the left, the rocks rise, in wild and rugged peaks, to a very great height; and having viewed the bold and rocky shores of Fugloe and Swinoe, from the opposite side of the island, we again embarked.

It was our intention, at this place, to have changed our boats; but the people being all engaged abroad, plucking their sheep*, our crew from Eide offered to take us forward, apparently delighted with the opportunity it gave them of obliging us. We had already come twenty-three miles, and were dis-

* The savage custom of tearing the wool from the backs of their sheep, still prevails in these islands.

tant thirty-three from Thorshavn, where our people were still twenty-three miles from Eide; and as they never remain from home over night, they had to complete a journey of seventy-nine miles without rest, and almost in the constant exercise of the oar. From their knowledge of the tides, they are enabled to take every advantage of the current; but in a voyage of this extent, it was impossible to have it always favourable; we were therefore more than once stationary for upwards of half an hour; the utmost exertion of the crew being barely sufficient to prevent the boat being carried back with the stream, in turning some of the headlands we had to pass. About eight in the evening we reached Thorshavn, where, after resting an hour, our crew left us to return home, and arrived there at two in the morning, having performed their voyage in nineteen hours, including stoppages, an exertion which may appear incredible but to those who saw it.

By this rapid and successful voyage, we retraced in three days nearly all our former excursions, and, besides, accomplished our visit to the northern islands. We were now fully satisfied, we had nothing new to expect by extending the period of our stay, and nothing now remained worth examination, except the island of Suderoe, which, lying in the course of our return, we expected to stop at after our final departure from Thorshavn; but in this we were unfortunately disappointed.

We were likewise satisfied with the supply of minerals we had obtained. We had made a splendid collection of zeolites, of every variety, except analcime, of which I was rather surprised not to have seen a single specimen in the country, except one solitary crystal, which was found among the debris at Waaij. Since our return, however, I have procured a very magnificent specimen of this substance from Kollefiord in Stromoe;

Stromoe, not above ten or twelve miles distant from Thors-havn ; a discovery somewhat mortifying, considering we could have visited it with great facility.

Of Calcedony we had likewise procured several very capital specimens, all brought by the people to barter against tobacco ; nor could we learn where they were to be found *in situ*, being constantly told, that they were got loose upon the summits of the mountains, and by the priest at Quivig, that the place he found them in, was at that time covered with snow. Two days before our departure, an accidental circumstance led to the accomplishment of what I so much desired : a country man offered us some masses, having the appearance of being recently detached, from some small barnacles which adhered to them being still alive. This led us to inquire where they were got ; and being informed at Lambevig, within twelve or fifteen miles distant, we determined to explore it. The nearest approach to this place is by Skaalefiord, on the west side of Osteroe. Here we landed at Glibre, and walked over to Lambe, not three quarters of a mile distant, and having there procured a boat, were set on shore among the rocks, a little to the right of the harbour, where we soon found some immense masses of calcedony.

Our apparatus was now considerably impaired ; besides, the rock was so situated, that nothing but mining tools could effectually act upon it. These we had now neither time nor opportunity to apply, although we would certainly have done so, had we come here earlier ; as it was, we procured some excellent and interesting pieces of calcedony, as well as some capital specimens of chabasié. But the circumstance which afforded me most satisfaction on this expedition, was to find the cavernous calcedony *in situ*. I here saw plates of it of extraordinary dimensions, one being four feet long by two in breadth,

which appeared to be parallel, with several others which I saw in the same place, and all of them horizontal.

On the shore of Skaalefiord, we also found several masses of calcedony, of the same description, likewise cavernous; and I have no doubt, that by a little attention, abundance may be found in the same vicinity.

This excursion was particularly gratifying to me, as it afforded an opportunity of observing some peculiarities in the relations of a fossil, very much referred to by the supporters of the two great geological questions, each as affording decisive proofs in their own favour. Viewing it as belonging to that subject, I shall connect my observations on calcedony, with those I have to make on the geology of the country.

The difficulty we met with, in discovering the localities of minerals, fully confirmed the fears I anticipated on our first arrival. Excepting Mr HOLM the clergyman of Quivig, we did not find a person in the country, in the practice of collecting minerals. But notwithstanding the apparent indifference, with which these beautiful productions are treated by the natives, yet they soon found out the objects we were in pursuit of, and frequently afforded considerable assistance in finding them; as all, however, depends on the care with which minerals are taken from the soil, and as these poor people are destitute of implements for this purpose, it is impossible they can obtain them in their most interesting state, even if they were inclined to bestow their time and labour in collecting them, which the difficulty they experience in obtaining the more necessary articles of food and raiment most peremptorily forbids.

This excursion to Lambe was the last. The day following we embarked, expecting the vessel would come to anchor off Suderoe, as was originally intended. But as we approached that

that island, the wind blew so fair, and at the same time so strong, that we thought it more advisable to bear away, and had the singular good luck to reach Leith Roads in less time than we took going out, having performed our expedition exactly in six weeks.



GEOLOGY.

IN the preceding part of this paper, I have purposely refrained from touching on geology, in order that I might bring together the few remarks that occurred on that subject. No country perhaps presents such perfect sameness in its geological relations; from end to end the islands are composed of trap rocks; consequently, it is only to the peculiarities this interesting series here affords, that my observations can extend.

SIR GEORGE MACKENZIE has already described part of the curious phenomena I now allude to, which he thinks corroborate the doctrines suggested by some of the facts he met with in Iceland, and has, in a great measure, exhausted a subject so singularly meagre. A few observations, however, still remain to be made, without entering into any theoretical discussions, which, though amusing, in the present state of knowledge, are not capable of leading us to any satisfactory conclusions, with respect to the general formation of the globe. All I hope to accomplish is, to give a faithful description of what I have seen; and to mark the impression made upon my mind, by

certain features which distinguish the Faroe Islands from all other countries I am acquainted with.

The Faroe islands occupy a geographical extent of about sixty miles by forty; their greatest stretch being north and south. Throughout the whole, there is not one atom of stratified rock to be met with, if I may be allowed to judge from the islands we visited, together with what we learned of those we did not touch at. I do not consider, that even the coal of Suderoe presents an exception to this general conclusion. We have much reason to regret, that circumstances prevented us from exploring this island; as, from some remarks, both of DEBES and LANDT, as well as from some observations of Captain BORN, which occur in a periodical publication of Copenhagen, devoted to natural history, Suderoe contains peculiarities in the arrangement of some of the trap rocks, with which we found nothing analogous in any of the other islands. According to LANDT, the coal was particularly examined by a Mr HENCKEL in 1777, in obedience to the orders of the directory of mines; and by his report, the veins extend in length 12,000 feet, and, at an average, 4000 in breadth. He then goes on to calculate the quantity of fuel that an area of such dimensions would produce. From this it must be supposed, that he has estimated in the above sum, the whole of the coal to be met with in Suderoe; consequently, as there are several places at which it is found, none of the seams can be of large extent. With respect to the quality of the coal, he asserts, that it was found to be superior to that of Airshire; but this cannot be the case, if it be all like the specimens we procured, which are entirely similar to that occurring imbedded in the basalt, in the vicinity of Ballintoy, on the coast of Antrim. It presents the same ligneous texture; it burns with difficulty;

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it emits the same intolerable smell ; and produces a very large proportion of ash. At Ballintoy it occurs only in small beds, sometimes in very minute patches, and is used only in burning lime.

I think it is fair, therefore, to conclude, that this coal presents none of the characters of regular stratification ; and it besides, appears to be totally unaccompanied with sandstone, as I find it immediately connected with trap in some of the specimens I have got. So far as we saw, sandstone does not occur in the country. LANDT mentions generally, that it is to be found in the dales of Nordstromoe. We nowhere, however, saw any vestiges of it ; and from the very low ebb of his geological acquirements, nothing on that subject, unless corroborated by other testimony, can be attended to, as we may judge by the following passages, quoted from Captain BORN, as authority. “ If (says he) a bluish-grey fine-grained sort of stone, which contains grains of quartz and calcedony, be not *granite or limestone*, neither the one or other is to be found in “ the islands.” Page 140.

The varieties of trap we met with, were Greenstone, Porphyritic Greenstone, Amygdaloid, Trap-tuff, and Porphyry-slate. Basalt we found only in a few dykes ; we nowhere saw it in beds, or presenting the beautiful articulated columns of Staffa and the Giant’s Causeway. The beds composed of these different kinds of trap, have one uniform dip and direction ; stretching from S. S. E. to N. N. W., and so very slightly inclined, that it required a considerable portion of the rock to be seen at once, to render the inclination perceptible. When surveyed from the north or south, the beds consequently appear horizontal ; while on the east and west sides an inclination is observed, dipping almost imperceptibly towards the south.

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This arrangement of the mass of beds, prevails with great uniformity throughout; we observed them to deviate only in one instance from it, at the western extremity of Myggenaes, where they rise abruptly to an angle exceeding 45° . There was no particular uniformity that we perceived, in the succession of the different kinds of trap, although such might perhaps be discovered on more minute investigation. The trap-tuff was more abundant than any of the other varieties, and was often the only material we could distinguish, in cliffs of tremendous elevation, in which we sometimes found it difficult to trace the separation of the different beds. When greenstone or amygdaloid are interposed, the separations are better defined, and are often divided, by a layer of brick-coloured fine-grained tuff, which passes into the substance of the adjoining bed, and is analogous to the red material which forms so striking a feature in the basaltic promontories of the Giant's Causeway. The grandest display of greenstone which we met with was at Zellatrae in Osteroe, where the bed, like that of Fairhead, is three hundred feet thick, and split in a similar manner, into prismatic concretions, which, at a distance, give it a regular columnar appearance. On the top of the hill, at Leynum, I found the greenstone in the rude columnar form, which it assumes on the south-west side of Arthur's Seat; and in Naloe, as well as in some other of the islands, we noticed beds of a description quite new to me. Being considerably elevated in the cliff, we could not ascertain the dimensions with accuracy. I think, however, that, in Naloe, may be from forty to fifty feet thick; it is composed of vertical columns, enormously thick. In some the diameters were little short of their length; and although they stand straight upon the base, they are all bent at the sides, so as to present an irregularly waved line between each. The most singular circumstance

circumstance respecting them is, that they are all coated with an arrangement of small prisms, set at right angles to their axis; so that when some of these rude masses are broken in their longitudinal direction, a circumstance which occurred not unfrequently, a double set of small prisms appeared, to separate the columns, not unlike little dykes; and when an horizontal direction chanced to be displayed, a radiating arrangement of prisms was observed around the edge. The substance which generally occurs, of a brick-red colour, and forms partial separations between some of the varieties of trap, seldom extends to any great length; in thickness it is very irregular; the greatest depth we met of it, was in the vicinity of Kirkeboe in Stromoe, where it may be from eight to ten feet. It is not invariably of a red colour: under the greenstone at Leynum, it occurs of a dark-green. This material, which in some cases may very readily be mistaken for sandstone, particularly as it frequently presents a slaty texture, after being acted on by the weather, appears to me to form a portion of the overlying trap, although it no doubt presents a very different character. In other beds of greenstone, nearer home, alterations equally remarkable may be pointed out; and as we frequently observed in it cavities lined with zeolite, similar to those which were dispersed through the other parts of the same bed, it cannot possibly be considered of a separate formation.

Notwithstanding the uniformity that prevails in the general disposition of the trap in Faroe, there are some beds which depart entirely from it. The most remarkable of these, we met with in the vicinity of Norderdahl in Stromoe. A little north of this place, there is a conical hill, presenting a projecting rounded front towards the shore; and being denuded from top to bottom, the structure is distinctly seen, and the horizontal beds traced with the utmost precision. Resting on these, and
covering

covering the summits and sides of this cone, excepting on the south, a bed of greenstone stretches, which is rendered particularly conspicuous by the vertical rents of its prismatic concretions. On the west side, after descending more than half-way from the top of the cone mentioned, it continues towards, and again rises to the top of Dalsnyphen; from whence passing under other beds, it may be traced in an undulated line, almost without interruption, to Skellingfeld, where it terminates in a wedge, about half-way from the base of that hill. A little north of this, and nearly in a line, the same kind of greenstone recommences, and extends to beyond Leynum. Skeeling presents on the west side a bare section, like the conical hill of Norderdahl, whence the beds may be counted in one continuous slope, from the base to the summit, rising to a height of about three thousand feet; and here, although the powerful mass of greenstone I have just noticed, loses itself in a wedge, there is not the slightest derangement created among the other beds of trap of which Skeelingfeld is composed.

The position of this enormous mass of greenstone is worthy of attention, and is difficult to reconcile with any of the doctrines I know of. Its occurrence in Skeeling, &c. as an adventitious bed, like that of Salisbury Craig, is nothing uncommon; but when it extends from under the incumbent trap, stretching itself over a most irregular surface, and finally covering, as like a cap, a very acutely conical hill, it assumes a new and totally different character.

It is in the beds of amygdaloid that the beautiful zeolites and calcedonies are found. I have already noted the different places where they occur, and have also given sufficient details respecting the varieties of zeolite. But as the peculiarities of Calcedony have often been urged in support of doctrines very
opposite

opposite in geology, a description of them belongs more properly to this part of my subject.

From the proofs we met with in Faroe, of the igneous origin of trap rocks, I think it almost unnecessary to enter into any discussion respecting the formation of Calcedony, as no doubt can very reasonably be entertained of its co-existing with the matrix in which it is inclosed. The laws by which it has been regulated, in assuming the various appearances it now presents, are, however, beyond our reach; nothing I met with-tended in any degree to elucidate the subject.

Calcedony occurs in the rock in the most irregular masses, generally rounded, and sometimes shooting into forms like bunches of grapes. It is either solid, or in hollow cavities, and varies in size from the head of a pin to a foot or upwards in diameter. When solid, the masses are marked with parallel lines, straight or concentric, sometimes with both; the latter forming a border or coating round the straight lines. The solid pieces are frequently penetrated by minute stalactites of the same substance, branching through the mass, in a form perfectly vegetable; and as these always contain a portion of green earth, it occasions an appearance extremely similar to moss; which no doubt has given rise to the assertion of LANDT, that moss and straw actually occur in them. I have sometimes found, on breaking the solid masses, which contained these slender fibres, the latter would separate from their bed, and leave an impression of their form,—a tolerable proof that they must have been in a solid state previous to their being enveloped in the larger mass. I have obtained very few stalactites, whose centres are not occupied by this green substance; it seldom exceeds in thickness the twentieth part of an inch, and varies in colour, from a very

Vol. VII. K k dark

dark green, with a tinge of blue, to a pale green, and even a light straw-yellow. It is always opaque. It is not easy to conceive, that this substance could have existed without something to support its very delicate fibres; but yet this appears to be quite evident, from the examination of specimens, and consequently adds one more difficulty to the solution of the formation of Calcedony.

When calcedony occurs in hollow cavities, they are simply mamillated, ornamented with stalactites, or lined with crystals. Externally, they are of a very fantastic shape; but within, the arrangement is more symmetrical. The upper part forms a kind of dome, with a smooth mamillated surface, or appended all over with stalactites, which generally hang perpendicularly, but sometimes ramify in every direction; while the lower part of the cavity is filled up, so as to present an horizontal surface or platform, affording a pleasing contrast with the irregularity opposed to it.

This platform is evidently the result of circumstances subsequent to the formation of the stalactites, as they not only occasionally penetrate it, but sometimes the horizontal part seems to have risen to the top, so as to have filled the cavity, and envelope the stalactites entirely. This part of the mass also varies from the rest, by presenting different shades of colour, forming the onyx, or *Band Calcedony* of the lapidaries; it is often accompanied with semi-opal, white, yellow, and green in colour, and also with the opaque white substance, known by the name of *Cacholong*. It likewise often happens, that the upper and stalactitic part of the cavity is covered over with a second deposition, forming a distinct coat, which sometimes differs in colour. Besides the circular arrangement which this produces in the section of the stalactites, when they are cut across
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and polished, a radiation of fibres may be observed diverging from the slender green thread which occupies the centre.

It does not appear as if the form of the stalactites were owing to the pendulous position in which they are found, as they sometimes occur attached by the side, and terminated both above and below. With respect to size, they vary, from the thickness of a hair to two inches in diameter; but I have not found any above four inches in length.

The cavities are often entirely lined with crystallized quartz, which always forms a distinct coat, and is sometimes interposed between the two coats of calcedony. To illustrate these combinations, I selected a series of specimens, all of them portions of hollow masses. In the first, the quartz is transparent and crystallized, in the usual form. In the second, the crystals are coated with calcedony, but so slightly, that the shape of the quartz is preserved entire. In the third, the edges of the crystals are rounded off. In the fourth, the apices are partly converted into mamillons. And, in the fifth, the crystallization is entirely replaced by a mamillated surface; the quartz being visible only in the section of the mass. Nearly the whole of these changes may be traced in one specimen belonging to Sir GEORGE MACKENZIE. At one part, the crystallization is nearly complete, while, at the other extremity, it is perfectly mamillated. I never observed quartz crystals among stalactites, or stalactites among crystals. The quartz and calcedony always occur in distinct coats, one or other occupying the whole surface; and, except very rarely, with perfect uniformity.

Quartz and calcedony is a combination well known to mineralogists; but before visiting Faroe, I was not aware, that the latter substance occurred, also intimately connected with zeolite. Some Danish mineralogists have considered this substance as the connecting link between calcedony and opal; a

conclusion I did not find to be justified by the observations which occurred to us; opal and the mixture of zeolite and calcedony having uniformly very different aspects.

The first specimen of this description which I procured, is a very interesting one. The outer surface, or that which lay next the matrix, is formed of radiated zeolite, which has also been projected into the cavity, in the form of stalactites. The whole of this is covered over with calcedony, which is so intimately blended with the acicular fibres of the zeolite, that it is nearly impossible to trace the demarcation between the substances. And, again, upon this calcedony, is imposed a coating of most slender stalactitic fibres of the same material. In the centre of the zeolitic stalactites, there is a point, like that occupied by green earth, in those of calcedony, from which the zeolite diverges. Here the zeolite is infinitely harder than is usual with the varieties of that substance,—an effect no doubt produced by its intimate connection with the calcedony.

In cavities of this description, we sometimes found quartz and sometimes zeolite; but the latter does not, like the former, uniformly occupy the entire surface, being occasionally dispersed in solitary crystals, among the stalactites of calcedony, and sometimes, though rarely, accompanied with carbonate of lime.

The only variety of crystallized zeolite which occurred in these cavities, was Apophyllite, presenting two or three very beautiful varieties of crystallization:

1. In rectangular prisms, terminated at each end by faces set at right angles to the axis, and deeply truncated on all the solid angles, so as to form a truncated pyramid,
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the sides of which would measure about 67° . This is the *Mesotype epointée*, fig. 175. of HAUY.

2. The same crystal, with the apices of the pyramids perfect; may be termed the Pyramidal Apophyllite.
3. When the same crystal is so short in the prism, that the truncating faces touch, and so broad on the terminal faces, as to reach the sides of the prism, a cube truncated on all the solid angles is produced.
4. In rectangular prisms, hollow, and diverging at the termination. This variety, I suspect, owes its present appearance to decomposition.

In one specimen, I found some of the crystals of zeolite covered with a *sheath* of calcedony, open at the top, and partly hollow, owing to the decomposition of the apophyllite. It would be almost endless to proceed, with a detail of all the peculiarities which occur, in the combinations of these different substances. I have said enough to mark several curious circumstances relating to them; many of which will perhaps have an effect quite the reverse of affording any clew towards those hidden arts by which the hand of nature has accomplished their formation. We have proofs, most unequivocal, of the igneous origin of the rock in which they are imbedded, and *consequently* of their own. But granting this, as also the construction of the cavities, and the production of the stalactites of calcedony, whence came the second coating, which so frequently occurs, lining them entirely and uniformly throughout? whence the different alternations of quartz and calcedony? and whence the substance of those beautiful crystallizations of zeolite I have just mentioned?

Like

Like all other trap countries, the Faroe islands are intersected by innumerable dykes. Those we examined were principally of fine-grained greenstone, and more or less formed of prismatic distinct concretions. They frequently occur in the hollows, between hills, and in gulleys which intersect the precipices, being, from their proneness to decompose, themselves the cause of these indentations. They do not appear to have any peculiar direction, although they be generally very nearly perpendicular. Many we perceived cutting the hills from top to bottom. Some we could trace from one island to another; and even where covered with soil, we often observed their track, by the superiority of the verdure on the surface; the dyke, from its looseness of texture, having acted as a drain, and rendered the bottom more nourishing to the vegetable root. This circumstance proves how permeable these dykes are to moisture, and helps to account for the singular fissures that often present themselves on the shore. They are considered by LANDT as marks of some violent convulsion in nature; and to a common observer, they have very much that appearance. The effects of the weather, however, without any such assistance, are quite sufficient to accomplish this end; nor will its operations be tardy; the constant action of the surface-water on the summit, and the continued lashing of the waves at the base, are agents of sufficient power; and we have thus dykes washed from their *sockets*, for an extent of several hundred feet, leaving a frightful chasm in rocks of enormous height.

Two of the most singular dykes we met with, are between Thiornivig and Westmanhayn, on the north-west coast of Stromoe. The first intersects a mural precipice, a little beyond Stakken. This is a double dyke: immediately under the edge of the cliff, it divides, and shortly after joins again;

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it then separates a second time; and the left branch continues its course downwards to the sea; while that on the right diverges, and breaks off in a point; a little beyond, but somewhat above the level of this, it recommences, and continues downwards to the edge of the water, in a line parallel, but at a distance from the other. Both terminate in caves, which often occur at the base of dykes.

The other which attracted our attention, is a little south of the entrance to the bay in which Saxen is situated. At the top, it cuts the edge of the cliff, in the usual manner. After descending for some length perpendicularly, it makes a curve to the left, in the form of a hook, and becomes evanescent. Within this curve another takes its rise, and after describing a figure similar to the letter S, it again vanishes. To this another succeeds, somewhat similar in form, but not quite so regular, commencing and vanishing in the same way; and from within the lower curve of the last, another sets off; and from this the dyke continues perpendicularly till it reaches the water. The cliffs in which these two dykes are, being principally composed of trap-tuff; the colour of which is dark-brown, while the dykes themselves are almost black, and of the prismatic structure; the contrast renders them conspicuous and well defined.

I have seen many veins of basalt, but never found one presenting features at all analogous to those of the last mentioned. From the appearance of the tuff in its immediate vicinity, one would almost imagine, that the whole mass, from top to bottom, not less, I think, than about 1000 feet, had been in a soft state when invaded by the dyke. In many of these rocks there is a parallel disposition of the materials observable; and in some, myriads of minute particles of zeolite were arranged in such regular lines, as to give them a decided stratified appearance.

ance. Some such lines existed in this cliff; which, near the dyke, were drawn as it were into the curves it has described, which could not easily have been the case, had the rock been in a solid state when it was traversed by this dyke.

The soil produced by the degradation of trap-rocks is usually found to be fertile, and favourable to vegetation. These islands, however, where no other rocks exist, have no such happiness to boast of. In Faroe, there is no flat land, on which the reduced materials can rest; and where not occupied by impracticable cliffs, they present a solid smooth surface, always highly inclined, on which vegetation, by degrees, arrests the crumbling particles, and in time forms a sward upon the arid rock. Here, however, from the impenetrable nature of the mass, no support can be obtained by the vegetable fibres; accumulation, therefore, becomes fatal, and, sinking under its own weight, the soil slips from the surface, and leaves the rock in its original naked state.

The hills being placed so near to each other, there are no valleys to enrich; consequently, whatever falls from above, is swept away by the mountain torrents, and affords no farther benefit. So slight is this vegetable covering, that it often gave way under our feet, leaving the smooth rock exposed. This was likewise the case among the more recent debris, on which, when expecting safe footing at least, we often found an inclined and slippery surface, covered with wet clay, which rendered a considerable degree of caution necessary. It is between the sward thus formed and the rock, that the rain-water must find its way. Hence the cold damp bottom on which the verdure rests, renders it totally unfit for cultivation or improvement.

Where the torrents poured down the side of a hill, we had an opportunity of observing the smooth and solid nature of the
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the rock, on which the action of the water had produced no other effect, than displacing the soil, the course being simply marked by it on each side; and LANDT informs us, that there is no soil in any part of the country above three feet thick. It would be curious to investigate, whether this smoothness on the sides of the mountains could be traced to any external cause, such as that which has been observed by Sir JAMES HALL, on Corstorphine Hill, and other parts of this country, indicating the passage of heavy bodies along the surface. Near Eide, I observed a very remarkable example of this description. There the rock was scooped and scratched in a very wonderful degree, not only on the horizontal surface, but also on a vertical one, of thirty to forty feet high, which had been opposed to the current, and presented the same scooped and polished appearance with the rest of the rock, both above and below.

Had any doubt respecting the igneous formation of trap rocks remained upon my mind, previous to my visit to Faroe, it would have been completely removed, by the facts we there met with, which have been so ably described by my fellow traveller. These we first observed in the Island of Nal-soe, and afterwards at Eide, and at Waaij. The specimens bearing marks of this phenomenon, which we brought home, in order to present to the Society, will sufficiently convey the same impression to all who examine them. No production of a furnace can tell its tale in plainer language, nor any slag bear more distinct marks of the effects of heat.

In Nal-soe, it was only on the surface of one bed of amygdaloid that we observed these marks of fusion. They were the first I had seen, and of course created no small degree of surprise. This was distinctly the upper surface of the bed, be-

ing immediately overlaid by the brick-coloured trap-tuff, passing into the superincumbent bed. At Eide and Waaij, the appearances differed from those of Nalsole. They did not occupy the surface, but were dispersed through the bed, at different elevations, yet all parallel, as if it had been formed of a multitude of partial flowings, each moulding on the consolidated surface of its predecessor, and extending, as the supply of fluid matter permitted; and, so far as we observed, this was always in the direction of the declivity of the hill or cliff on which we perceived them.

By the regularity of the folds and wrinkles which characterize their surfaces, and their usual horizontal position, they seem to have flowed undisturbed; and although we never met with any very extensive surface exposed at one place, in consequence of finding them always on the declivity of a hill, yet we had the most perfect evidence of their passing under the incumbent rocks, but to an extent of which we could of course form no estimation. There was enough, however, seen, to mark it as a fact of a general nature, and one which, more than any other I have ever met with, denotes the origin of trap-rocks.

There is a wide difference between ascertaining the agency by which a rock has been formed, and the manner in which that agent has been applied. It is an inquiry perhaps of no great utility, but, in prosecuting the one, the other naturally presents itself, and the inquisitive mind cannot be expected to stop short, particularly at a point, where, for the first time, it has met with data quite unequivocal. Here, however, the multiplicity of proof in favour of the former, throws a difficulty of great magnitude in the way of the latter. Had each bed been the operation of a volcanic eruption, the appearances of fusion would

would have been confined to the surface, as was the case in Nalsole; but when these occur throughout the whole mass, all in parallel lines, it is difficult to comprehend how the minute portions constituting a series of distinct and separate flowings, could have been produced. From the smallness of their bulk, it appears evident, that they never were exposed to the action of a superincumbent ocean, as they could not possibly contain heat enough to counteract its effects; and if the whole bed had been in fusion at once, the internal marks, if not obliterated, would, at all events, have been deranged. In order to conceive a bed of lava flowing under an ocean, I should imagine one necessary postulate is, that the bulk of heated matter must, in all cases, be equal to sustain the whole in a state of fusion, until it has flowed into the situation it was destined to fill; and from thence, again, other operations are requisite to raise it to the station it now occupies, above the level of the sea.

The extent of the country, the uniform inclination of the beds, their regularity and symmetry, all present difficulties to this mode of formation; and while the igneous appearances in the rocks of trap, bespeak a source in some near neighbourhood, throughout the islands, there is not one spot that can be fixed upon, more readily than another, as the site of a volcano; the highest hills in the country, Slattertint and Skeelingfeld, being surmounted by beds of trap, nearly horizontal.

To my mind, the subject remains loaded with difficulty. It is something to have obtained such unquestionable corroboration of the igneous origin of trap. But the circumstances under which that powerful agent has performed its office, are to me, I confess, as inexplicable as ever.

VIII. *Account of the Structure of the TABLE MOUNTAIN, and other Parts of the PENINSULA of the CAPE. Drawn up by PROFESSOR PLAYFAIR, from Observations made by Captain BASIL HALL *, R. N. F. R. S. EDIN.*

(Read 31st May 1813.)

THE Paper which I have the honour of presenting to the Society, is drawn up from letters written by Captain HALL to some of his friends in this country, after a visit made to the Cape of Good Hope, and an excursion to the Table Mountain, in July last. I have given the description, as much as possible, in his own words, and have only connected parts, which, from the nature of the communications, were necessarily disjoined from one another. One of the letters being written to myself, and containing a general view of the whole, has been my guide for arranging the rest.

Captain HALL intended to have reduced his observations into order, for the purpose of laying them before the Royal Society, which, however, his professional avocations have not left him leisure to accomplish. In the mean time, as they make known a new fact in Geology, and one which, though from analogy, we might suppose that it was somewhere to be

VOL. VII. P. II. M m

* Then Lieutenant.

met with, has not hitherto occurred, it was thought right to lay them before the Society; and I have undertaken the arrangement of the materials the more willingly, that the descriptions, though coming forward without the accurate finishing which the author alone could be expected to give them, do great credit to his skill and judgment, and will, I am sure, be received by the Society as a most promising indication of the talents of a young and ardent Geologist, from whom much important information may hereafter be expected.

The Peninsula of the Cape is a narrow mountainous ridge, stretching nearly north and south, for thirty or forty miles, and connected on the east side, and near its northern extremity, with the main body of Southern Africa by a flat sandy isthmus, about ten miles broad, having Table Bay on the north of it, and the spacious indenture, or rather gulf of False Bay on the south. The southern extremity of this peninsula projecting into the sea, with False Bay on the east, and the ocean on the south and west, is properly the Cape of Good Hope, and is the most southern point of the African Continent. At this point, the chain which constitutes the peninsula, though rugged, is lower than at the north end, where it is terminated by the Table Mountain, and two others, which form an amphitheatre, overlooking Table Bay, and opening to the north. The sketch Plate XIII. represents the positions here referred to.

Table Bay is open to the north and west, and is therefore an unsafe station for ships while the sun is in the northern hemisphere, and while the wind blows strong, as it always does, at that season, from the north-west. The ships then rendezvous in False Bay, where they have complete shelter from the prevailing wind. The ship in which Mr HALL was, arrived on the

coast

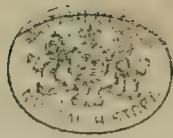
PLATE XIII.

Royal Society, Trans. Phil. Soc. Lond. 1701.

SKETCH of the PENINSULA of the CAPE



Admiral Cook's Chart



coast in July ; it was obliged to go round the Cape to the latter station ; and as they stood close in-shore when they worked into the Bay, he had an opportunity of seeing the general distribution of the strata in the peninsula, which seemed nearly horizontal, but dipping somewhat to the south.

On the east side of False Bay, the outline of the hills is extremely rugged, often running into sharp conical peaks, and altogether having the aspect of extreme asperity and barrenness.

The three hills which terminate the peninsula on the north, are the Table Mountain in the middle ; the Lion's Head, sometimes called the Sugar Loaf, on the west side ; and the Devil's Peak on the east. The Lion's Head, which is about 2100 feet high, is separated from the Table Mountain by a valley, that descends to the depth of 1500 or 2000 feet below the summit of the Table Mountain, which is itself 3582 feet above the level of the sea. On the west of the Lion's Head, the ground, after falling, rises again, forming an inconsiderable elevation, known by the name of the Lion's Rump, from which the ground descends gradually to the sea. The amphitheatre, formed by these three mountains, is about five or six miles in diameter, in the centre of which is placed the Cape Town. The situation is magnificent, with the sea stretched out before it on one side, and the perpendicular cliffs of the Table Mountain overhanging it on the other. The Town itself is built with uncommon regularity, but has so many squares, gardens and trees interspersed, that the stiffness and formality of streets, either parallel or cutting one another at right angles, is entirely taken off. All round the town, on the land side, up to the base of the cliffs, where the piles of ruins from above effectually prevent cultivation, the ground is

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covered,

covered with gardens, laid out in the Dutch style. In this space, of course, the rock is nowhere to be seen. Exactly in the middle of the perpendicular face of the Table Mountain, a ravine indents it to a considerable depth. Down this a stream constantly runs, which is often suddenly swelled into an impetuous torrent, and has acted very powerfully in clearing away the earth and rubbish, and in laying bare the rock to a considerable extent. The ascent to the top is by this ravine, and, though the distance is not great, is a work of considerable labour, on account of the steepness, and still more, on account of the vast fragments of rock, which one must clamber over continually, and which seem so exactly poised, that the least addition to the weight of the projecting side, must precipitate them to the bottom. The cut thus made in the mountain is about twenty or thirty yards deep, and from ten to fifteen wide at the bottom, though, at the upper part, the walls are not distant from one another by more than eight or ten feet.

Captain HALL, in a letter to his father Sir JAMES, gives the following account of his first ascent.

“ The day before yesterday, I set out on an excursion to the Table Mountain, with the ardour and impatience that you, I have no doubt, on similar occasions, have often experienced. I set out with the intention of following the course of a stream that descends from the ravine in the face of the mountain. For a considerable distance at the bottom of the mountain, the soil covers the rock; and a little higher up, I found large fragments of sandstone, and now and then a block of granite, which had come down from above. I came, after a short ascent, to a space where many yards of the rock were laid perfectly bare, and I found myself walking on vertical Schistus, or on what might be called *Killas*. This rock was in beds highly inclined,
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and stretching from east to west, which is nearly the direction of the mountain. It was not always easy to ascertain, which way these beds inclined. They were often almost vertical, and in some places I found them inclining *from* the mountain. On looking forward a little higher up, I saw another portion of rock that was also laid bare, and which appeared to be Granite. I had now no doubt of reaching in a few minutes the precise junction of the two rocks, and I ventured to predict to my companion, who was not a little surprised at the pleasure I seemed to feel on this occasion, that we should immediately see veins from the main body of the granite, penetrating into the rock on which we were now standing. In this I was not deceived; the contact was the finest thing of the kind I ever saw; the *Windy Shoulder** itself not excepted. The number of veins that we could distinctly trace to the main body of the granite was truly astonishing; and the ramifications, which extended on every side, were of all sizes, from the breadth of two yards to the hundredth of an inch. Masses of killas, cut off entirely from the main body of that rock, floated in the granite, without numbers, especially near the line of contact, and the strata appeared there broken, disordered, and twisted in a most remarkable degree.

“ Near this place I found a mass of killas in a state of decomposition; it had crumbled away, and left the granite dykes with many of the slender ramifications standing. The word *ramification* does not, however, properly express the nature of these smaller dykes; which are not branches, but plates or thin slender walls. There is nothing here that might not be expected,

* The spot which Mr HALL refers to, is on the side of Loch Ken in Kirkcudbrightshire, and is remarkable for veins of Granite, of the same kind with those here described. An account of it will be found in the preceding part of this volume, p. 99.

pected, on finding a vein partly stript of the containing rock ; but as a specimen of this sort had never occurred to me before, I thought it worth mentioning.

“ From this point, following up the course of the stream for about 300 yards, I found the whole a solid mass of granite. The granite is characterised by large crystals of felspar, which, indeed, is true of all the granite which I met with at the Cape. Besides quartz and mica, large masses of hornblend enter occasionally into the composition of this rock.

“ After ascending about 300 yards farther, I came to a line where the granite ceased, and was succeeded by strata of superincumbent Sandstone. These strata were horizontal, and without any symptom of disturbance or violence whatsoever. There was not a *shift* nor a vein ; and this junction formed a most marked contrast with that which we had left below.

“ Looking round from the point where I now stood, to all the parts of the amphitheatre, in the centre of which I was placed, I could trace the same line of junction, extending horizontally on every side.

“ From this point, where the sandstone was first discovered, for about 150 or 200 feet perpendicular, the rock continued of the same kind, viz. a red sandstone, in horizontal beds of no great thickness. From thence all the way to the summit the sandstone was of a much more indurated kind, quite white, and having pieces of water-worn quartz imbedded in it, from the size of a pea to that of a potatoe. The top is a plane of about ten acres, somewhat uneven, though, on the whole, nearly level. The weather acting very powerfully in this exposed situation, has worn holes, and laid open the strata in some places to a considerable depth ; and the cement of the sandstone being softer than the included masses of quartz, these last, when they are exposed at the surface, are always presented

sented with considerable relief. From the same cause, the bottom of every excavation is covered with a little beach of quartz pebbles, which have belonged to strata now worn away.

“The day was remarkably fine, so that the prospect from the top of the mountain could hardly be seen to greater advantage than at this moment. It was, indeed, uncommonly grand, and the interest was heightened by the novelty of the objects, and, perhaps, not a little by the reflection, that the point on which we stood is so remarkable in the constitution of the earth’s surface, and so memorable in the history of nautical discovery. A carpet of the richest heath embellished the summit, and I sadly regretted, that my knowledge of botany was not sufficient to enable me to describe the beautiful varieties of that plant for which the Cape is so justly celebrated.

“The same structure that is found in the Table Mountain, extends to two others in its vicinity; particularly the upper part of the Lion’s Head, which consists of sandstone. Under the sandstone is found granite; and on the north side, when you descend farther, you come to killas in vertical beds, which extend farther to the eastward, forming the Lion’s Rump, where the strata are not strictly vertical, but incline to the north, at an angle of about 8° from the perpendicular; their direction or stretch is across the peninsula, or from east to west, as at the Table Mountain. I had not time to undertake a particular examination of the Devil’s Peak. The upper part of it is certainly composed of sandstone; the junction of which with the granite is distinctly visible from many places.

“The same structure, with the variation of some circumstances, seems to take place all over the peninsula. The line of the junction of the granite and the sandstone, lowers in its level, however, as you approach the southern extremity, and the
killas

killas at last disappears entirely. I uniformly found the rock, which the sea washed towards the south part of the peninsula, to be granite, and five or six yards higher was the sandstone. That which lay next the granite, was the red, as before, and above that was the white and more indurated sandstone; nor did it seem that there was much difference between the thickness of the sandstone mass here and at the Table Mountain. I must not omit to remark, that at the entrance of Simon's Bay, a creek in the west side of False Bay, where our ship lay, there rose from the sea an oblong rock of granite, about ten yards long, four or five yards wide, and eight or ten feet high, called Noah's Ark: abreast of this, on the beach, I found two whin dykes, a foot wide each, cutting the granite.

"Were it not," continues Captain HALL, "too great presumption in me to step out of the province of simple description, to wander in the regions of theory, I would propose as a conjecture, That the great mass of Sandstone which forms the summit of the Table Mountain, and of all the hills in the peninsula, had been raised from its original horizontal position, to the elevated situation it at present occupies, by the Granite forcing it up from below; that the rest of the peninsula had been raised in the same manner, but not so high, or that it had been lifted up a little more on the east side than the west. The highly inclined position of the killas, at the base of the Table Mountain, and the disjointed nature of its junction with the granite, seem conclusive as to its having undergone a great change; while the veins of granite which traverse the strata, both by their appearance when they insulate the fragments of killas, and by their minuteness of ramification, seem to have been in a state of fusion, accompanied by very considerable violence."

Thus

Thus far Captain HALL; and the concluding remark, so far from requiring apology, seems to me a very fair interpretation of the phenomena he has described. Conformably to this view of the matter, I would only farther remark, that the phenomena here described point out two separate epochas, distinguished by very different conditions of the substances which now compose the peninsula of the Cape. That peninsula, it now appears, is a wall of granite, highest at the northern extremity, and lowering gradually to the south; faced, at its base, with grauwacky, and covered, at its top, with a platform of horizontal sandstone. The penetration of the killas or grauwacky, by veins from the mass of granite which it surrounds, proves that the killas, though the superior rock, is of older formation than the granite. The granite, therefore, is a mineral that has come up from below into the situation it now occupies, and is not one of which the materials have been deposited by the sea in any shape, either mechanical or chemical. It is a species, therefore, of subterraneous lava, and the progeny of that active and powerful element, which we know, from the history both of the present and the past, has always existed in the bowels of the earth.

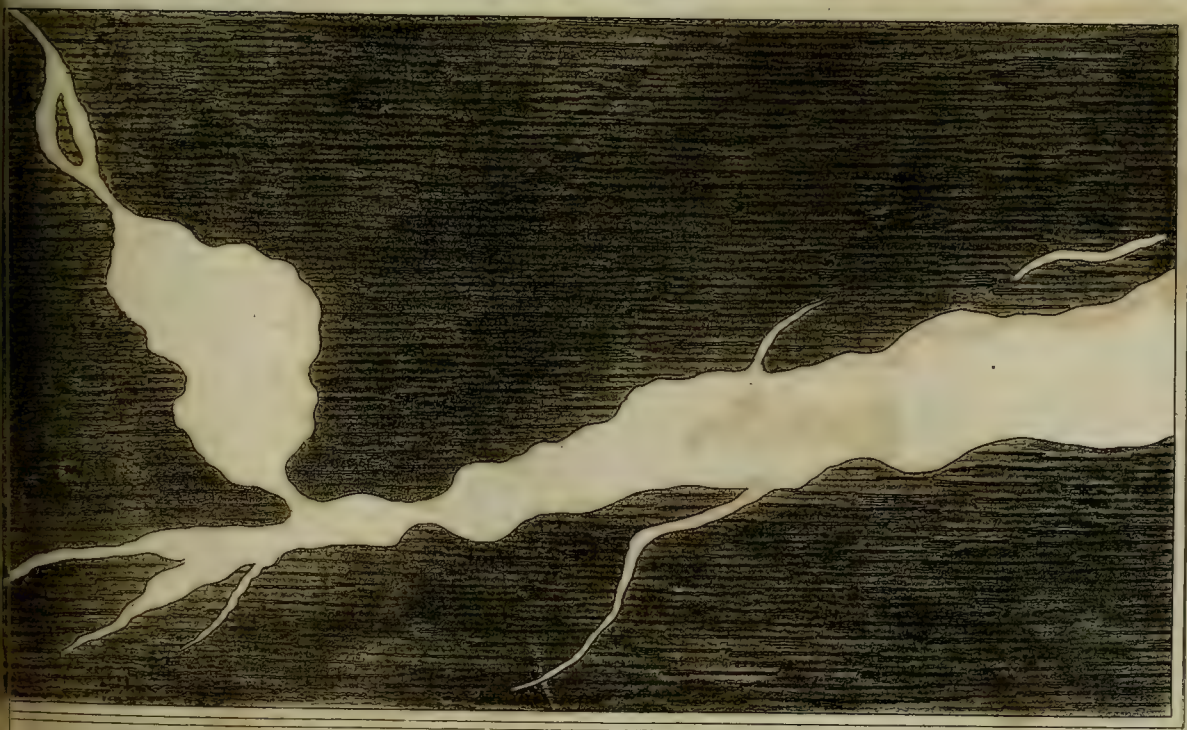
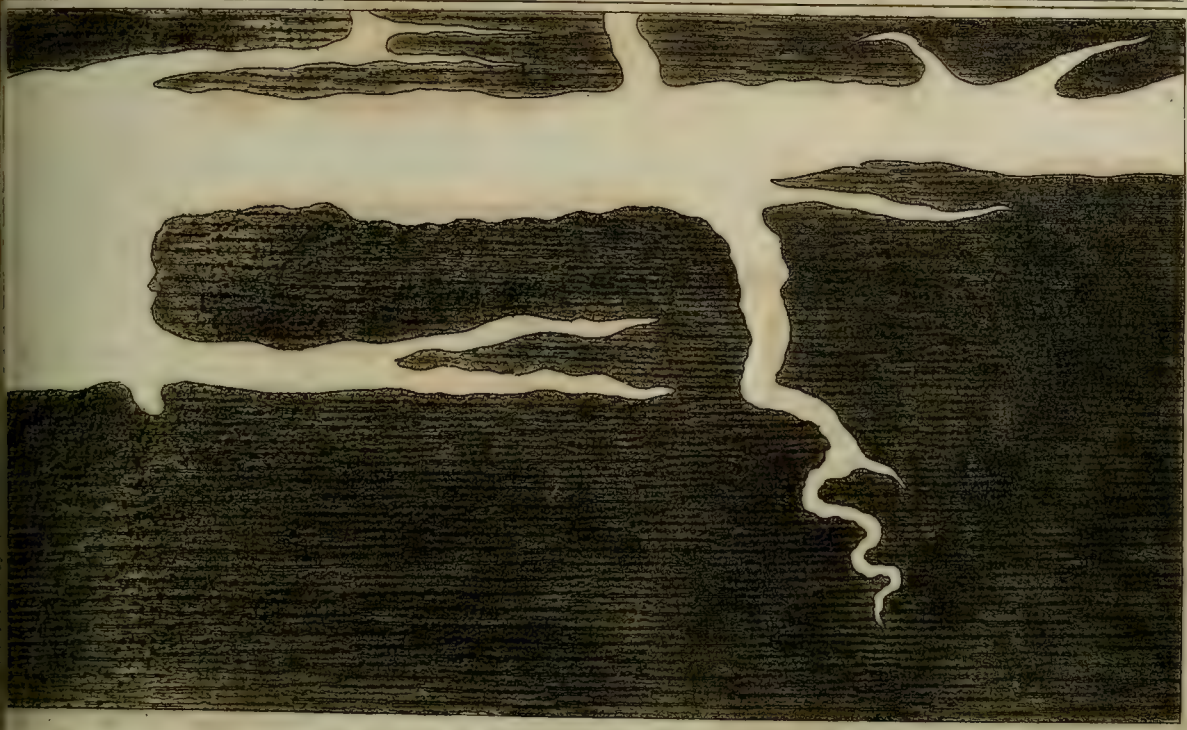
The introduction of the granite into the situation it now occupies, must have taken place while the whole was deep under the level of the sea: this is evident from the covering of sandstone which lies on the granite, to the thickness of 1500 feet; for there can be no doubt whatever that this last was deposited by water. After this deposition, the whole must have been lifted up, as Captain HALL supposes, with such quietness and regularity, and in so great a body, as not to disturb or alter the relative position of the parts. Thus the granite is shewn, I think with great probability, to be newer than one of the rocks incumbent on it, and older than the other. I know not that we have ever before had an example of a fact which so directly ascer-

tains the place which granite really occupies, in respect of the other parts of the mineral kingdom; it is one that from analogy might be expected to take place, and it is highly favourable to the opinion, that granite does not derive its origin from aqueous deposition. It seems, indeed, to be an *instantia crucis*, with respect to the two theories concerning the formation of rocks.

The sandstone, which covers the granite at the Cape, has the appearance of being a very extensive formation; and is probably a part of the same which BARROW, LICHTENSTEIN, and other travellers, have mentioned as composing many of the principal mountains over a great tract of Southern Africa. The tenuity of the parts holding together large masses of that rock, and indicating a singular approach to equilibrium between the causes that produce, and those that resist decay, was observed on the summit of the Table Mountain, as already mentioned; and similar appearances excited the surprise of the two travellers just named, in some of the most inland tracts which they visited.

Plates XIV. and XV. are representations of veins of granite connected with the main body of that rock, and penetrating into beds of killas or grauwaky, which are nearly vertical.

Amos, 1847, Atlas



GRANITE VEINS TRAVERSING THE KILLAS IN THE TABLE MOUNTAIN





In many of the Veins which traverse the Killas, the Stratification becomes very indistinct, and frequently bends considerably more than any of these Sketches represent, most of the detached fragments are contorted, and appear as if they had been softened.

Lévesque Sculpt. Killas

GRANITE VEIN IN THE TABLE MOUNTAIN A YARD & A HALF WIDE.



IX. *Comparison of the North Polar Distances of Thirty-eight Principal Fixed Stars, on the 1st of January 1800, as determined by Observations made at Greenwich, Armagh, Palermo, Westbury, Dublin, and Blackheath. By S. GROOMBRIDGE, Esq. Blackheath, F. R. S. Lond. Communicated by Dr BREWSTER.*

[*Read 16th November 1812.*]

BY comparing the longitudes and latitudes of the fixed stars, as computed from their right ascensions, and north polar distances, determined by observations made at distant periods, Astronomers have been enabled to ascertain, with the utmost accuracy, the precession of the equinoxes. The great accuracy, however, which has lately been introduced into the construction of astronomical instruments, and the more general cultivation of Practical Astronomy, will hereafter be the means of ascertaining the minute changes in the places of the fixed stars, which have been denominated their *proper motion*, and which, probably, arise from a motion of the solar system in absolute space.

The observations made at Greenwich during the last fifty years, have been chiefly upon thirty-six of the principal fixed stars. Observations were also made upon γ *Draconis*, as being
near

near the zenith, for determining the collimation of the quadrant; and upon *Polaris*, to determine the distance of the pole for the co-latitude. In consequence of the erection of circular instruments in several fixed observatories, since the year 1790, they have been employed in observing the same thirty-eight fixed stars, and it is from the results of these observations that we have been enabled to make the comparison contained in the following Table.

The first column contains the observations made by Dr MASKELYNE at Greenwich, with an eight feet mural quadrant: The second, contains the observations made at Armagh by Dr HAMILTON: The third, those made at Palermo by M. PIAZZI, with a five feet circle: The fourth, contains those made by Mr POND at Westbury, with a two and a half feet circle: The fifth, contains the mean of the observations made by the preceding circular instruments, as deduced by Mr POND: The sixth, contains the observations made by Dr BRINKLEY at Dublin, with an eight feet circle: And the seventh, contains the observations made at Blackheath, by the author of this paper, with a four feet mural circle.

The differences among the results obtained by these instruments, may arise from various causes;—from errors in the division of the instrument;—from the assumption of different formulæ of refraction, which will affect all the zenith distances as well as the co-latitude;—from a probable deviation of the plumb-line; and from the uncertainty of the proper motion of the stars, in reducing to the same epoch the observations made from 1792 to 1810, the quantity of which is very considerable in Arcturus and Sirius. The mural circle of six feet, erected at Greenwich in 1812, may be expected to reconcile several of these discrepancies, leaving doubtful only the small quantity that

that may be supposed to arise from the deviation of the plumb-line.

The observations made at Palermo differ from those given by Mr POND, in his paper on this subject; having been copied from the recent catalogue of M. PIAZZI, published in 1803.

TABLE

North Polar Distance of Thirty-eight principal

STAR.	Greenwich.	Armagh.	Palermo.
Polaris,	1° 45' 33.7	1° 45' 33.3	1° 45' 36.2
γ Draconis,	38 28 53.0	38 28 53.8	38 28 53.6
Capella,	44 13 21.6	44 13 21.5	44 13 24.0
α Cygni,	45 25 41.5	45 25 39.5	45 25 37.6
α Lyræ,	51 23 41.4	51 23 37.4	51 23 37.8
Castor,	57 41 14.4	57 41 9.8	57 41 14.0
Pollux,	61 30 10.2	61 30 4.6	61 30 11.5
β Tauri,	61 34 31.3	61 34 32.8	61 34 32.9
α Andromedæ,	62 0 46.2	62 0 45.5	62 0 48.5
α Coronæ borealis,	62 36 10.9	62 36 7.9	62 36 10.8
α Arietis,	67 29 20.7	67 29 22.7	67 29 20.0
Arcturus,	69 46 8.5	69 46 11.8	69 46 11.2
Aldebaran,	73 54 17.3	73 54 19.1	73 54 18.0
β Leonis,	74 18 35.3	74 18 34.9	74 18 35.0
α Herculis,	75 22 7.4	No observation	75 22 11.4
α Pegasi,	75 51 57.8	75 52 1.9	75 52 1.1
γ Pegasi,	75 55 37.1	75 55 38.4	75 55 38.5
Regulus,	77 3 36.0	77 3 32.9	77 3 33.5
α Ophiuchi,	77 16 54.9	77 16 54.3	77 16 54.0
γ Aquilæ,	79 51 44.6	No observation	79 51 45.2
α Aquilæ,	81 38 53.0	81 38 51.8	81 38 54.5
α Orionis,	82 38 31.8	82 38 33.0	82 38 34.0
α Serpentis,	82 56 2.3	82 56 1.0	82 56 5.8
β Aquilæ,	84 4 52.2	No observation	84 4 45.7
Procyon,	84 16 18.5	84 16 20.6	84 16 22.0
α Ceti,	86 42 7.3	86 42 8.7	86 42 8.2
β Virginis,	87 6 27.6	87 6 30.4	87 6 28.5
α Aquarii,	91 17 1.2	91 17 6.2	91 17 3.7
α Hydræ,	97 47 50.9	97 47 50.8	97 47 49.0
Rigel,	98 26 30.7	98 26 35.1	98 26 35.5
Spica Virginis,	100 6 39.0	100 6 41.0	100 6 42.8
1α Capricorni,	103 6 46.7	No observation	103 6 49.7
2α Do.	103 9 5.4	103 9 15.8	103 9 9.2
1α Libræ,	105 9 13.9	No observation	105 9 20.0
2α Do.	105 11 58.0	105 12 3.8	105 12 3.7
Sirius,	106 26 58.9	106 27 7.1	106 27 5.0
Antares,	115 58 18.7	115 58 29.4	115 58 24.9
Fomalhaut,	120 40 36.5	No observation	120 40 37.6

Fixed Stars, on 1st January 1800.

STAR.	Westbury.	Mr POND'S mean	Dublin.	Blackheath.
Polaris,	1° 45' 36".2	1° 45' 35".0	1° 45' 35".70	1° 45' 35".01
γ Draconis,	38 28 53.6	38 28 53.0	38 28 53.11	38 28 55.96
Capella,	44 13 18.4	44 13 19.4	44 13 19.79	44 13 19.51
α Cygni,	45 25 36.9	45 25 38.2	45 25 40.26	45 25 42.08
α Lyrae,	51 23 36.1	51 23 36.7	51 23 39.17	51 23 40.44
Castor,	57 41 14.2	57 41 13.6	57 41 12.92	57 41 16.58
Pollux,	61 30 13.9	61 30 12.6	61 30 10.83	61 30 13.39
β Tauri,	61 34 33.9	61 34 33.6	61 34 31.12	61 34 35.45
α Andromedæ,	62 0 50.2	62 0 47.8	62 0 49.47	62 0 52.16
α Coronæ borealis,	62 36 13.3	62 36 10.4	62 36 9.99	62 36 16.63
α Arietis,	67 29 21.1	67 29 21.9	67 29 23.89	67 29 24.60
Arcturus,	69 46 8.0	69 46 10.2	69 46 12.57	69 46 13.29
Aldebaran,	73 54 16.0	73 54 17.5	73 54 16.85	73 54 21.57
β Leonis,	74 18 33.1	74 18 34.0	74 18 38.79	74 18 38.19
α Herculis,	No observation	No observation	75 22 12.33	75 22 18.05
α Pegasi,	75 51 58.6	75 52 0.6	75 52 3.76	75 52 5.86
γ Pegasi,	75 55 37.6	75 55 39.3	75 55 41.49	75 55 45.93
Regulus,	77 3 34.7	77 3 34.7	77 3 39.00	77 3 41.53
α Ophiuchi,	77 16 54.2	77 16 54.0	77 16 58.68	77 17 2.85
γ Aquilæ,	No observation	No observation	79 51 46.50	79 51 51.82
α Aquilæ,	81 38 52.3	81 38 52.7	81 38 54.47	81 38 59.50
α Orionis,	82 38 32.3	82 38 32.7	82 38 32.45	82 38 35.97
α Serpentis,	82 56 3.1	82 56 3.3	82 56 3.42	82 56 10.66
β Aquilæ,	No observation	No observation	84 4 51.80	84 4 58.90
Procyon,	84 16 22.5	84 16 21.6	84 16 22.00	84 16 25.24
α Ceti,	86 42 11.4	86 42 10.2	86 42 10.28	86 42 15.07
β Virginis,	No observation	87 6 29.3	87 6 26.58	87 6 32.52
α Aquarii,	91 17 6.0	91 17 5.4	91 17 5.24	91 17 8.89
α Hydræ,	97 47 54.8	97 47 53.6	97 47 53.00	97 47 59.67
Rigel,	98 26 38.2	98 26 36.3	98 26 34.58	98 26 39.50
Spica Virginis,	100 6 44.8	100 6 42.9	100 6 43.06	100 6 47.37
1α Capricorni,	No observation	No observation	103 6 49.94	103 6 55.62
2α Do.	103 9 10.1	103 9 9.8	103 9 9.54	103 9 12.34
1α Libræ,	No observation	No observation	No observation	105 9 27.82
2α Do.	No observation	105 12 3.9	105 12 1.80	105 12 7.60
Sirius,	106 27 4.4	106 27 5.7	106 27 3.55	106 27 7.57
Antares,	No observation	115 58 27.8	115 58 24.30	115 58 29.62
Fomalhaut,	No observation	No observation	120 40 42.44	120 40 44.22

X. *On the Optical Properties of Sulphuret of Carbon, Carbonate of Barytes, and Nitrate of Potash, with Inferences respecting the Structure of Doubly Refracting Crystals.*
By DAVID BREWSTER, LL. D. F. R. S. EDIN. & F. A. S. E.

(*Read February 7. 1814.*)

IN examining the changes which light undergoes during its passage through transparent bodies, we not only receive information respecting the properties of that mysterious agent; but we are in some measure made acquainted with the composition of the substances themselves, and with the manner in which their ingredients are combined. The optical phenomena, therefore, which bodies exhibit in their action upon light, are so many tests, to which the philosopher may have recourse, either in supplying the place of chemical analysis, or in correcting and modifying its results. A difference in the optical properties of two bodies, is generally an infallible indication of a difference in their elementary principles; and whatever confidence we may place in the skill of the chemist, or in the accuracy of his methods, the mind can never rest satisfied with the results of an analysis which is directly opposed by optical phenomena.

It is highly desirable, therefore, that the Chemical Philosopher would avail himself more frequently of the agencies of light in the prosecution of his inquiries. The various products to which his attention is constantly directed, cannot always be preserved for subsequent examination, and can seldom be procured by the Experimental Philosopher. An opportunity is thus lost of confirming his own results, and of contributing most essentially to the progress of optical knowledge. It is by the alliance, indeed, of Chemistry with Optics, that great revolutions are yet to be effected in Physics; and the time is probably not very distant, when, by their united efforts, we shall be able to develop those mysterious relations among the elementary principles of matter which hypothesis has scarcely ventured to anticipate.

In the following paper, I propose to describe the optical properties of Sulphuret of Carbon, Carbonate of Barytes, and Nitrate of Potash, and to illustrate the conclusions to which some of these properties lead, respecting the structure of doubly refracting Crystals.

I. SULPHURET OF CARBON:

This remarkable fluid was lately discovered by LAMPADIUS. It is pure and colourless like water; has a specific gravity of 1.272; is remarkable for its extreme volatility; and is composed of 85 parts of sulphur, and 15 of carbon.

Owing to the great length of spectrum which this substance produces, I found considerable difficulty in measuring the mean index of refraction. By taking the bisecting ray beyond the green rays, and very considerably advanced upon the blue-space, I obtained the following results:

Angle

Angle of the prism,	8° 10'
Angle of refraction,	5° 38'
Refractive power,	1.687

By considering the bisecting ray, as passing through the green space, and near its confines with the blue, the following measures were obtained :

Angle of the prism,	8° 10'
Angle of refraction,	5° 27'
Refractive power,	1.6632

As the sulphuret of carbon has nearly the same action upon the red and green rays, as balsam of Tolu, I have no doubt but that the bisecting ray is considerably advanced upon the blue space, and that the mean index of refraction is nearly 1.680.

A prism of flint glass, with a refracting angle of 20° 53', corrects the colour produced by a prism of sulphuret of carbon, having a refracting angle of 8° 10'; the light being incident perpendicularly upon the fluid prism. Hence it follows, that the dispersive power of the sulphuret, or the value of

$\frac{dR}{R-1}$ is 0.115, R being the index of refraction, and d the part

of the whole refraction to which the dispersion is equal; and that the refractive power of the extreme red ray is 1.623, and the refractive power of the extreme blue ray 1.737.

From these experiments we conclude, that the *sulphuret of carbon exceeds all fluid bodies in refractive power, surpassing even flint-glass, topaz and tourmaline; and that in dispersive power, it exceeds every fluid substance, except oil of cassia, holding an intermediate place between phosphorus and balsam of Tolu.*

These relations will be better understood from the following short Tables :

Refractive Powers:

Sulphur, native,	2.115
Boracite, -	1.701
<i>Sulphuret of Carbon,</i>	1.680
Tourmaline, -	1.668
Blue topaz, -	1.636
Flint glass, -	1.616

Dispersive Powers.

Oil of cassia, -	0.139
Sulphur, -	0.130
Phosphorus, -	0.128
<i>Sulphuret of Carbon,</i>	0.115
Balsam of Tolu, -	0.103
Flint glass, -	0.052

Although oil of cassia surpasses the sulphuret of carbon in its power of dispersion; yet, from the yellow colour with which it is always tinged, it is greatly inferior to the latter, as an optical fluid, unless in cases where a very thin concave lens is required. The extreme volatility of the sulphuret is undoubtedly a disadvantage to which the oil is not liable; but as this volatility may be restrained, we have no hesitation in considering the sulphuret of carbon, as a fluid of great value in optical researches, and which may yet be of incalculable service in the construction of optical instruments. All other fluids are separated from these two, in their optical properties, by an immense interval; and hence we are of opinion, that oil of cassia will
yet

yet be found to consist of ingredients as remarkable as those which enter into the composition of sulphuret of carbon.

II. CARBONATE OF BARYTES.

The native Carbonate of Barytes possesses, like the agate, the remarkable faculty of forming two images, one of which is bright, and the other nebulous. The shapeless appearance of the agate; its heterogeneous and imperfect structure, and its anomalous character in the mineral kingdom, corresponded well with the singularity of its optical properties, and discouraged the anticipation of analogous phenomena, in minerals of a more perfect structure. I was, therefore, surprised, to find the same character in carbonate of barytes, a mineral which has a regular crystalline form, and possesses two distinct refractive powers. The index of refraction for the perfect or least refracted image is 1.540; and its dispersive power 0.0285.

In order to observe with accuracy the phenomena presented by the carbonate of barytes, I formed nine prisms, cut in different directions, from the same specimen. In one of these prisms, which was bounded by planes parallel to the striæ or longitudinal joints, the least refracted image was extremely distinct, while the other was a faint nebulous image, of a brownish-red hue. It was small and round, and the intensity of its light was inconsiderable, when compared with that of the bright image.

When the image of a candle polarised by reflection, was viewed through this prism, having the longitudinal joints parallel to the plane of reflection, the light which formed the bright image of the candle was wholly reflected, while the nebulous light alone, penetrated the mineral. But when the
longitudinal.

longitudinal joints were perpendicular to the plane of reflection, the light image became extremely distinct, in consequence of the nebulous light having now refused to penetrate the prism.

In a second prism, the nebulous, or most refracted image was more luminous than in the first, and approached to a definite form; the general shape of the candle being distinctly visible.

In a third prism, the nebulous image was more luminous than in the last case, and the form of the candle still more distinctly seen; but it had now the appearance of an assemblage of incoincident images.

In a fourth prism, in which the plane of refraction was parallel to the longitudinal joints, both the images were imperfect, and the most refracted image was extremely faint. By inclining the prism, an image appeared on each side of the least refracted image; but they were polarised in the same manner, and were probably analogous to the two images which are frequently seen in specimens of calcareous spar.

In a fifth prism, which was formed by planes nearly perpendicular to the longitudinal joints, *four* images were plainly visible, all of which were imperfect, and consisted of circular arches of nebulous light. The two middle ones, which were the principal images, were equally luminous, and were polarised in an opposite manner, like all other double images; but each of the two outer images was polarised in the same manner as the bright image farthest from it. The *most refracted* of the two principal images, was in this case the most perfect of the two, and exhibited a degree of prismatic colour so much greater than the other, that it obviously belonged to a higher dispersive power. When the light enters the prism, and emerges from it at equal angles, the four images are not distinctly separated,

separated, and are extremely imperfect. When the angle of incidence at the first surface of the prism is increased, the images become more and more distinct, and better separated; but, by diminishing the angle of incidence, all the images approach one another, and are confounded into one mass of nebulous light.

With a plate of Carbonate of Barytes, which was about two-tenths of an inch thick, and which had its surfaces at right angles to the direction of the longitudinal joints, the image of a candle was a large circular mass of light, when the incidence was perpendicular. By inclining the plate, this mass was changed into an annular image: By increasing the inclination, it assumed the form of a crescent, and at a considerable angle of incidence, it was separated into three imperfect images, or circular arches of nebulous light, similar to those which were seen with the fifth prism. The middle image, which was the brightest, consisted of the ordinary and extraordinary image, which were not separated, in consequence of the parallelism of the refracting surfaces. In one position of the plate, these arches were crossed by other three similar arches, inclined to the first at an angle of 10° or 12° .

The phenomena which have now been described, differ in several respects from those which are presented by the agate. In the *Carbonate of Barytes*, the two images are distinctly separated, and are, therefore, formed by two separate refractive powers; whereas in the *Agate*, the bright image is placed in the centre of the nebulous mass. In the *Carbonate of Barytes*, the imperfect image occupies a small space; but in the *Agate*, it is an elongated mass of light, extending about $7\frac{1}{2}^{\circ}$ in length, and about $1^{\circ} 7'$ in breadth, on each side of the bright image. These differences, however, are probably owing to the different ways in which the two minerals have been cut; but it is not
easy

easy to submit this point to direct experiment, on account of the difficulty of procuring a mass of agate, from which a variety of transparent prisms could be obtained. It follows, however, from the theory of the depolarisation of light, which I have explained in another place, and which is supported by all the evidence which any theory can possess, that the specimens of agate which depolarise light must necessarily form two distinct images,—a phenomenon to which we have found a rapid approximation in the carbonate of barytes.

The property which has now been explained, forms a simple and infallible mineralogical character of the striated carbonate of barytes ; and is particularly valuable to those who have been perplexed by the numerous marks with which some writers have laboured to distinguish it from its kindred minerals. The assistance, indeed, which optics affords in discriminating minerals, is of the most extensive kind ; and it is much to be wished, that mineralogists would exchange many of their vague distinctions for those unambiguous characters which bodies exhibit in the modifications they impress upon light.

The Abbé HAUVY has, in some measure, begun this reformation, and has set a brilliant example of what may be effected by the aid of mathematical and physical acquirements. In his admirable work on Crystallography, which has never been duly appreciated in this country, he has created a new science, in which he has shewn how to determine the integrant molecules of crystallised bodies ; and how, from a few primitive forms, may be derived that endless variety of secondary crystals which adorn the mineral kingdom. The recent discoveries which have been made in optics, enable us to give a new direction to these interesting inquiries ; to determine the forms, and even the angles of crystals, from their optical properties ; and out of a mass of shapeless fragments, to reconstruct an artificial crystal,

tal, of which all the parts shall have the same relation as they had in nature to the axes and sides of the primitive crystalline form.

III. NITRATE OF POTASH.

This salt possesses the most remarkable optical properties of any crystal that is at present known, and its various actions upon light are of the most anomalous and instructive character.

The crystals which I employed were all equiangular hexaedral prisms; and the light was transmitted through two natural faces, separated by another face, so that they were inclined to each other at an angle of about 60° . This inclination is by no means convenient for measuring refractive and dispersive powers; but I attempted in vain to form artificial faces inclined at a less angle, and those means which I had found successful with other soft crystals, completely failed when applied to this salt.

When a candle was viewed through the nitrate of potash, I observed a double refraction very much greater than that of calcareous spar,—a phenomenon which gave me the more surprise, as the Abbé HAUY, who examined many splendid crystals of this salt, ascribes to it the property of simple refraction.

The least refracted image was a circular mass of white nebulous light, condensed at its centre, into a very faint image of the candle, but without any strong prismatic tinge; while the light which had suffered the greatest refraction, formed a distinct and highly coloured image. The great interval between

the two images; the achromatic nebulosity of the first, and the distinctness and deep colours of the second image, formed altogether a singular phenomenon, and, at the same time, afforded an *ocular demonstration of the existence of two dispersive powers in doubly refracting crystals.*

The following measures of the refractive powers of the two images were taken with the greatest care :

Angle of the prism,	60° 21'
Angle of refraction for the 1st image,	24° 8'
Angle of refraction for the 2d image,	38° 54'
Index of refraction for the 1st image,	1.3374
Index of refraction for the 2d image,	1.5156

In order to confirm these results, I formed a new prism, and obtained the following measures :

Angle of the prism,	62° 12'
Angle of refraction for the 1st image,	24° 48'
Angle of refraction for the 2d image,	40° 39'
Index of refraction for the 1st image,	1.3326
Index of refraction for the 2d image,	1.5134

By taking a mean of these results, which are extremely near to each other, we obtain for the

Least refractive power,	1.3350
Greatest refractive power,	1.5145

Hence it follows, that the least refraction of nitrate of potash, is almost exactly the same as that of *Water*, which is 1.3358,—a result of such an extraordinary nature, that I felt it necessary to confirm it by repeated observations.

In

In measuring the dispersive power of this salt, we cannot expect the same accuracy of result on account of the great angle of the prism. Owing to the nebulosity of the first image^a it is impossible to measure its dispersive power; but it evidently corresponds with its low power of refraction. In order to correct the dispersion of the second refraction, it requires a prism of flint glass, with an angle of nearly 60°. With an angle of 66°, the dispersion is more than corrected; but with an angle of 56° the correction is not nearly completed. The dispersive powers due to these different angles, are contained in the following Table:

Angles of the flint glass prism,	$\left\{ \begin{array}{l} 66^\circ, \\ 60 \\ 56 \end{array} \right.$	Dispersive powers,	0.0613
			0.0573
			0.0546

By taking a mean between the two extreme observations, we obtain 0.058 for the approximate dispersive power,—a result which could scarcely have been anticipated from the substances which enter into the composition of nitre. The following Table will shew the relation which this measure bears to the dispersive powers of other bodies:

Sulphate of lead,	0.060
Nitrate of potash, 2d refraction,	0.058
Flint glass,	0.048
Water,	0.035

In order to examine the character of the rays which form the two images, I polarised the light of a candle by reflection from glass, and viewed it through two of the parallel faces of a hexaedral prism of nitre. When the edges or common sec-

tions of its faces were parallel to the plane of reflection; a bright image of the candle was seen in the middle of a mass of nebulous light, exactly similar to what happens in the agate when its veins are parallel to the plane of reflection. But upon turning round the crystal of nitre, the bright image gradually vanished, while the nebulous light increased; and when the edges of the crystal were perpendicular to the plane of reflection, the bright image was extinguished, and the nebulous light a maximum. When the reflected image of the candle is viewed through two inclined faces of the nitre, the two images vanish alternately, like those formed by all doubly refracting crystals.

A prism of nitrate of potash, having its refracting surfaces equally inclined to the axis of the hexaedral crystal, possesses the faculty of depolarising light; and hence it follows, from the theory of depolarisation, that the prism must, in this case, form two distinct images.

The two neutral axes of this salt, are parallel and perpendicular to the sides of the hexaedral prism; and the depolarising axes are parallel to the diagonals of the square base common to the two pyramids which compose its primitive rectangular octaedron. The least refracted image is that which is produced by the extraordinary law of refraction.

The beautiful coloured rings which I exhibited to the Society, as produced by the action of topaz upon polarised light, and which I have also discovered in the agate, and in a great variety of other bodies*, exist also, but in a very singular manner, in the nitrate of potash.

By

* See *Phil. Trans. Lond.* 1814. Part i. p. 218.

By comparing, in a rude manner, the coloured rings formed by different bodies, with the thickness of the plates by which they were produced, I concluded that the conjugate diameters of the rings were nearly as $\frac{1}{(m-1)^3}$, m being the index of refraction. In the nitrate of potash, however, their magnitude is quite anomalous, as it produces along the axis of the hexaedral prism a series of miniature rings, nearly *eight* times less than they should have been according to the preceding law. The beautiful generalisation of the phenomena of coloured rings, which we owe to the genius of the celebrated Biot, may perhaps afford an explanation of this apparent anomaly.

The *Carbonate of Potash* forms also two images, one of which is bright, and the other nebulous. They are polarised in an opposite manner, like those formed by the nitrate of potash, but the nebulous image is more distinct in the carbonate. With a prism bounded by natural faces, and having a refracting angle of $49^\circ 53'$, I obtained the following measures of its mean refractive power :

Index of refraction for the nebulous image, 1.379

Index of refraction for the bright image, 1.482

IV. ON THE STRUCTURE OF DOUBLY REFRACTING CRYSTALS.

Notwithstanding the numerous discoveries which have recently appeared respecting the polarisation of light, no attempt has been made to apply them in solving the problem of double refraction. They furnish us, indeed, with a variety of beautiful phenomena, analogous to the polarisation of light, which always accompanies the production of two images ; but they afford no ground of conjecture respecting the separation of the pencil into two parts.

When

When I discovered the property possessed by the agate, of forming a bright and a nebulous image, and of polarising them in an opposite manner, like all doubly refracting crystals, I was sufficiently aware of the conclusions which it authorised *; but as no other crystallised body exhibited analogous phenomena, I contented myself with stating these conclusions as mere conjectures, which required the sanction of numerous experiments.

In the carbonate of barytes, however, and in the nitrate and carbonate of potash, we are presented with properties analogous to those of the agate, and are therefore enabled to resume this subject, with that confidence which can only be derived from multiplied observations.

When we examine the two images formed by calcareous spar and other perfectly transparent crystals, we find that they have the same magnitude, and are equally luminous and distinct. There is, therefore, no circumstance which can lead us to suppose, that the light which forms the one image passes through a part of the crystal, having a different structure from that which transmits the light of the other image. In the carbonate of barytes, however, where the transparency of the crystal is imperfect, one of the images is nebulous and imperfect; and as the same phenomenon is exhibited in the agate and in the imperfectly transparent crystals of the nitrate and carbonate of potash, we are entitled to conclude, that the light which forms the imperfect image is transmitted through the imperfect structure; while the light which forms the bright image, is transmitted through a structure of a more perfect kind. The imperfect transparency, therefore, of the crystal, and the
nebulous

* See *Phil. Trans. Lond.* 1813, Part 1. p. 101.

nebulous character of one of the images, can be considered in no other relation than that of cause and effect.

From the optical properties of the agate, this conclusion derives a still higher degree of probability. The two images formed by this mineral are not similar to each other, like those of calcareous spar, though they possess exactly the same properties. One of them is bright and distinct, and the other is a mass of nebulous light. Now it happens, that the agate possesses two different kinds of structure, corresponding to the characters of its two images, and distinctly perceptible even to the naked eye. One of these structures is composed of small serpentine lines, like the figures 3333, resembling the surface of water ruffled by a gentle breeze; and I have a specimen in my possession, one-half of which has much larger serpentine lines than the other. The light which passes through the serpentine lines, is that which forms the nebulous image; while that which passes between them forms the distinct image. This may be demonstrated by a variety of experiments.

When the light is transmitted through a part of the agate that has the largest serpentine lines, the nebulous image has an appearance different from that which it has when the light is transmitted through the other part where the serpentine lines are smaller. If the agate is inclined in the direction of the serpentine lines, so as not to prevent the rays from passing between them, the bright image will be distinctly visible as before; but when the agate is inclined in a direction at right angles to this, so as to prevent the rays from passing between the serpentine lines, the whole of the transmitted light is nebulous. Hence it follows, that the nebulous image is produced by the imperfect structure of the agate, indicated by the
serpentine

serpentine lines; while the bright image is produced by a structure the same as that of all other transparent bodies.

The curvature of the nebulous light, in some specimens of agate, with incurvated veins, and its constant parallelism to the laminæ, and to the direction of the serpentine lines, give additional probability to this conclusion.

Here, then, we have a case of the most unequivocal kind, in which one image of a doubly refracting crystal is produced by one structure, or by one part of the crystal, while the other image is produced by another structure, or another part of the crystal; and hence we are led to conclude, in general, that the two images exhibited by all doubly refracting bodies, are formed by two different structures, related to some axis or fixed line in the primitive crystal. Whether this difference of structure is produced by a difference in the arrangement of the elementary molecules, or is owing to a combination of different ingredients, is a point which still remains to be determined.

The phenomena presented by the agate and the carbonate of barytes, convey still farther information respecting the structure of these imperfect crystals. In one direction, the light transmitted by the agate is wholly nebulous; the perfect image being converted into a shapeless cloudy mass, and confounded with the nebulous image. In another direction, one of the images is distinct and perfectly formed; and, in one specimen, which has the faculty of depolarisation, there must necessarily be two perfect images. In a prism of the carbonate of barytes, both the images were imperfect. In a second prism, the one image was nebulous, and the other distinctly formed; while, in other prisms, there was a rapid approximation to two perfect images. Hence it follows,
that

that the imperfect structure, which, in general, transmits only a mass of nebulous light, allows a distinct image to be formed, when the rays are incident in one particular direction; while the perfect structure, which in general gives a distinct image, allows an imperfect image to be formed, when the light penetrates it by a particular path.

These inferences, which I conceive to be irresistible, have a higher degree of importance than we may at first be disposed to attach to them. They form a real step in the explanation of double images, and indicate a part of that structure which is necessary to their formation. The other phenomena of double refraction are still involved in obscurity. The opposite polarisation of the two pencils, may be explained by supposing the crystal to consist of laminæ inclined in various directions; and, as I have shewn in another place*, the same phenomena may be actually produced by an artificial crystal, composed of bundles of glass plates. The most perplexing point, however, is the extraordinary refraction which takes place at a perpendicular incidence. Whether this phenomenon is the result of an extraordinary law of refraction, as HUYGENS and NEWTON supposed, or is produced by forces dependent on the elementary structure of the crystal, is a question which still remains to be determined. The extraordinary reflection and refraction arising from the last of these causes, which I have discovered in Mother of Pearl †, present an analogy, by no means remote, to the phenomena of double refraction.

* *Phil. Trans. Lond.* 1814, Part I. p. 280.

† This substance, whose remarkable optical properties I have explained in another place, resembles the Agate, the Carbonate of Barytes, and the Nitrate and Carbonate of Potash, in giving a bright and a nebulous image, when the

light is transmitted in one direction, and two bright images, when the light is transmitted in another direction; but it possesses this property under circumstances of such an extraordinary nature, that I could not with propriety have introduced any account of it into this paper.

A number of soft substances, of animal and vegetable origin, have likewise the faculty of forming a bright and a nebulous image, under various singular modifications. A full account of the results which I have obtained with this class of substances, will be found in another paper.

XI. *An Account of Observations, made by Lord WEBB SEYMOUR and Professor PLAYFAIR, upon some Geological Appearances in Glen Tilt, and the adjacent Country. Drawn up by Lord WEBB SEYMOUR.*

(*Read May 16. 1814.*)

1. **T**HE river Tilt is a principal branch of the Tay, which rises on the borders of Aberdeenshire, and runs towards the south-west, through the north-eastern part of the county of Perth. A portion of the valley along its course, for about ten miles above Blair of Atholl, is called *Glen Tilt*.

2. The adjacent country presents the common character of the Highlands. It is mountainous and rugged, and the surface, except in the lower parts of the valleys, is chiefly covered with heath. Peat-moss frequently occurs. The rock beneath consists entirely of those substances, which belong either to the *Primitive Formation*, or the *Overlying Primitive Formation*, of WERNER, and of which the stratified part is by Dr HUTTON denominated *Primary*. This account is warranted by all the observations which have been made upon these mountains; and their general aspect furnishes a confident inference for the whole. To the north and north-east, the mountains about the head of the river Dee, two or three of which are little inferior

in height to Ben Nevis *, are said to be mostly of granite ; and, it is probable, that either granite, or substances akin to it, occur in many places between these mountains and Glen Tilt. From the south-east, round by the south, to west, there is little room to doubt that the rock is almost wholly stratified, and that its chief components are gneiss, mica-slate, granular limestone, and granular quartz, with their intermediate gradations. This opinion was supported by our own observations, at the Pass of Killicranky, a few miles to the south-east of Blair, where there is a fine transverse section of the strata, and by others upon the rocks in the channels of the Garry and the Bruar, to the west and north-west of Blair. Without dwelling upon these †, I shall enter upon a detailed description of what appears nearer Blair, and proceed with it up Glen Tilt ‡.

3. In-

* Their height is about 4000 feet ; that of Cairngorm, one of the highest, is 4060.

† See Note A, at the end of the paper.

‡ The minute details, which continually occur in the following pages, must, to most readers, appear tedious and useless. It is necessary, therefore, to mention, that Glen Tilt was the first scene, in which Dr HUTTON met with what he considered as a confirmation of his views, respecting the relations that subsist between granite and the strata adjacent to it ; and, in the controversy that has since prevailed, between his followers and those of WERNER, the former have insisted much upon the phenomena in this Glen, while the latter have repelled their arguments by a very different account of the structure of the rocks, and of the substances that compose them. It became of importance, therefore, to ascertain the facts more precisely. With this intention, Mr PLAYFAIR and myself passed some days in examining Glen Tilt, in the autumn of 1807 ; and I returned again, for a short time, in 1808. In my description, I have endeavoured to avoid the language of theory as much as possible. When any thing has been noticed which is trivial, and does not immediately bear upon the main question, it will

3. In the immediate neighbourhood of Blair, the river Tilt, and the small stream of the Bannavie, which runs through the Duke of Atholl's pleasure-grounds, afford, each of them, a deep section of the rock, and shew that it is almost wholly formed of strata. The slight examination which we gave to these, led us to conclude, that they consist, for the most part, of granular quartz, mica-slate, hornblende-slate, and granular limestone. An observation made with an instrument for the purpose*, upon the strata by the side of the Tilt, not far above its junction with the Garry, determined the stretch to be about N. 60° E. ; and another upon the strata in the bed of the Bannavie gave N. 58° E. This near coincidence must, however, be regarded as accidental ; for the position of these strata is varied by many inflexions of their planes, especially on the banks of the Tilt. The dip is every where southerly, at an angle which varies considerably, but is commonly small for
primary

will be remembered, I hope, that the occasion called for a statement of the *whole case* ; and, besides, while the mere outlines of theory are still under discussion, it seems expedient, in describing phenomena which strongly affect any single point of importance, to enter largely into the concomitant circumstances ; since it is impossible to foresee what may, or may not, become subservient to a more precise determination of the truth.

* This instrument was a *Clinometer*, in its earliest form, without the brass plate. Though it was by no means so accurate as the improved one, the observations may, in general, be considered as within a few degrees of the truth. Some error may occasionally have arisen, from the surface of the rock being rather uneven, or from its not being quite parallel to the plane of stratification. For a more convenient comparison of the various positions of the strata, the bearing of the line of stretch is, throughout, indicated by the number of degrees east of north. As the line of dip is always at right angles to that of the stretch, it was thought sufficient to mark on which side the depression lay, by mentioning that of the four, or of the eight, principal points of the compass, which was nearest to its bearing.

primary strata. Where observed by the side of the Tilt, it was between 20° and 30° ; in the bed of the Banavie, it was 32° .

4. On the western bank of the Tilt, between two and three miles above its junction with the Garry, we observed some sienite in a scar, by the side of a walk through the Duke's plantations; and in another, not far from it, some gneiss. The soil that covered the neighbouring ground, prevented us from ascertaining the geological relations of the sienite. The gneiss is singular for having its component substances so disposed, that the structure of the stone bears no marks of stratification; but its structure differs from that of granite, in being less perfectly granular, and less highly crystallised.

5. The strata in the bed of the Banavie, a little below a bridge, that crosses it about a mile above Blair, are cut by a vein, or dyke, of felspar-porphry. The felspar base is chiefly of a red colour, but partly also grey. The vein is vertical, and about ten feet broad*.

6. There is a good road carried up Glen Tilt, as far as Forest Lodge, one of the Duke of Atholl's hunting-boxes, nearly seven miles from Blair. This road, after crossing two or three small streams, in the beds of which there is mica-slate, descends, about the end of the second mile, to the side of the river. There is here a bridge, called Gilbert's Bridge, and about fifty yards below it, we found in the channel gneiss, interstratified with granular limestone. From the bridge the road follows the bottom of the valley, and being often carried along the bank of the river, affords good opportunities of examining the

* Dr HUTTON mentions, (in a manuscript intended for his third volume, and never published) that he found several dykes, both of grey and red porphyry, in the course of this stream.

the rocks, which the violence of the stream has exposed in many parts of the channel. The substances of which they consisted, to a point rather more than a mile and a quarter below Forest Lodge, and marked by the letter A in the map* (Plate XXI.), and the plan of the river (Plate XVI.), were, as far as we observed them, gneiss, mica-slate, and granular limestone.

7. On the west side of the valley, between Gilbert's Bridge and the opening of Glen Merk, a great quantity of fragments, brought down by torrents from the steep bank of the mountain, consisted chiefly of mica-slate; though there were among them a few small ones of sienite. Mica-slate may, therefore, be inferred to be the prevailing rock on this side, and some strata of it, near the brow of the declivity, were distinguishable in the bed of a torrent, even from a distance. Strata of mica-slate occur also at the falls of the Merk, near its junction with the Tilt. Farther up, on the same side of the valley, where the road crosses the Criny, that stream flows over some beds of granular quartz.

8. On the eastern side of the valley, we often found granular limestone near the road, between Gilbert's Bridge, and another about two miles farther up, called Gow's Bridge; and a quarter of a mile beyond this, limestone is visible from a distance, in a scar on the same side, and three or four hundred feet above the bed of the river.

9. Through all this part of the Glen, the stretch and dip of the strata are tolerably uniform. In four observations † made between a point a little below Gilbert's Bridge, and Gow's
 Bridge,

* The outline of the map is copied from STOBIE's Map of Perthshire. An attempt has been made to give a better representation of the mountains near Glen Tilt; some names have been added, and others have been differently spelt, with a view of conveying the Gaelic pronunciation more distinctly.

† An observation at the falls of the Merk is included.

Bridge, the two extremes of the stretch were N. 38° E., and N. 51° E., and the angle of dip, which was southerly, varied from 26° to 38°. As far as the eye could judge, we had no reason to suppose that the irregularity any where exceeded these limits.

10. Fifty yards below Gow's Bridge, the strata, which are here of mica-slate, are cut by a vein of greenstone-porphry. The imbedded crystals of felspar are few and small, and the base is very minutely crystallised, containing, in the place of hornblende, what seems to be common actinolite; but this distinction is, perhaps, of little consequence, since HAVY has been induced to unite these two substances under the same species, on account of their agreement, both in the structure of the crystal, and in chemical composition, as proved by the analyses of VAUQUELIN. The vein is about ten feet thick, and dips westward at a large angle.

11. At the point A, the rocks in the bed of the river undergo a material change, and for upwards of three miles, they consist in some places of sienite, and in others of primary strata; and sometimes they exhibit masses of gneiss, granular limestone, granular quartz, hornblende-slate, and other materials of primary strata, interspersed among unstratified masses of sienite. An idea of the irregular succession of the stratified and unstratified masses may be derived from the plan *, which includes the course of the river between this point and a bridge above Forest Lodge. Appearances of the same kind occur for a mile and a half farther up.

12. In

* This plan was laid down, by taking bearings with the compass along the bank of the river, according to its changes of direction and by pacing the distances. The length of a certain number of paces was determined by measurement.

12. In some of these rocks, the sienite is seen in contact with various materials of the primary strata, and it was our particular object, to examine the phenomena in such cases. But, before I enter upon a detailed account of them, it may be of use to give a general sketch of this part of the valley.

13. Glen Tilt, for about two miles below Forest Lodge, and for three miles above it, to the junction of the River Chlochan, runs nearly in a straight line, in the direction of N. from 46° to 48° E., and therefore not far from N. E. and S. W.

14. The elevation of this part of the valley is considerable. According to General Roy's measurement, the lawn at Blair is 425 feet above the level of the sea; and Mr PLAYFAIR's observations with the barometer, indicated the difference of level between Blair and Forest Lodge to be about 400 feet. At the time of these observations, the barometer was falling, and the result is consequently too great; but after allowing for any probable error in excess on this account, the height of Forest Lodge above the sea can hardly be estimated at less than 700 feet.

15. The bottom of the valley is narrow, rarely exceeding a furlong in breadth, and generally much less. The lowest banks of the mountains rise on either side with very steep declivities; on the south-east, to the height of from 600 to 800 feet above the river; on the north-west, to between 1000 and 1200 feet. They are not cut by any lateral valleys; except, that on the south-east side, two or three deep ravines, descending from the sides of Ben y Gloe, join Glen Tilt between Glen Chlochan and Forest Lodge; and on the north-west side, a mile and a half above the Lodge, a brook, called the Crochie*, has deeply indented the ridge of the moun-

VOL. VII. P. II.

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tain,

* This stream is called in the country Auld Crochie. *Auld* signifies *brook* in Gaelic.

tain, though it has still to find its way by a succession of falls to the bottom of Glen Tilt. Towards the upper end of the valley, the breadth of it is contracted to that of the river, and the declivities are lower.

16. To the south-east of Forest Lodge, the lowest steep of the mountain is succeeded by a more gradual ascent to the top of Cairn Moorchie, which is from 1500 to 1800 feet above the valley. Beyond this, after a small intervening descent, an easy slope leads to the top of Cairn Kin Callum, one of the three broad summits of Ben y Gloe, the highest ridge in this neighbourhood. Cairn Gowar, the most lofty of the three, was ascertained by General Roy to be 3650 feet above the level of the sea, and Cairn Kin Callum cannot be much lower. The third summit is Cairn Leea, which lies at the south-west end of the ridge.

17. To the north-west of the Lodge, the bank that forms the valley, is immediately overtopped by another steep, of which the highest point is called the Griurnon. Mr PLAYFAIR determined the elevation of this above the valley, by means of a base measured in a flat close to the Lodge, and found it to be 1590 feet. From the Griurnon, a broad ridge extends, in a north-westerly direction, for nearly a mile and a half, and rises a few hundred feet to the pointed summit of Cairn y Chlan-nan. The height of this mountain above the sea is probably about 2800 feet.

18. The opposite sides of this part of Glen Tilt present a striking contrast in their surfaces. The south-eastern side is mostly covered with soil, bearing either good herbage, or heath, and shews but little of the rock, either fixed or loose. Along the north-western side, the mountain has in many places a craggy brow, the ruins of which lie thickly scattered over the declivity below, so as frequently to cover the ground. This is particularly

particularly the case between the Crochie, and a point more than three quarters of a mile below Forest Lodge, and opposite to M in the map, and the plan of the river.

19. Such a difference of aspect in the surface, gave reason to expect a difference in the nature of the rock beneath. Accordingly, on the south or south-east side, we found it to consist of strata, of which the chief materials were granular limestone, granular quartz, gneiss, and mica-slate. The general stretch of these strata coincides nearly with the direction of the valley, and their dip is towards the south-east, into the face of the mountain, at a large angle; though the stretch and dip are occasionally somewhat varied by inflexions. On the north or north-west side, the mountain is chiefly composed of grey sienite, or rather sienitic greenstone, which bears no marks of stratification. In the bottom of the Glen, therefore, is the line of junction between the strata on the south-eastern side, and the unstratified masses on the north-western; and it seems as if the course of the Tilt had here been very much determined by the position of the limestone strata, which occur frequently throughout the Glen, on the left bank of the river, and have been cut through by the stream. The facility with which limestone is worn away by water, in comparison with most of the other materials of a primary country, must have struck every mineralogist, who has met with it in that situation.

20. This general account of the rocks, that form the opposite sides of the valley, rests on the following observations.

21. We saw no sienite on the south side of the river, except immediately on its bank. The rock, where it appears, within a hundred feet above it, is stratified, and often of limestone. In order to ascertain its nature higher up, we ascended the mountain in two places, following, in each, a line almost at right

angles to the direction of the valley, and therefore also at right angles to the stretch of the strata.

22. One of these lines was in the course of a brook, which falls down the mountain within a quarter of a mile to the eastward of the point A, and has made a deep section of the rock, though not in the immediate vicinity of the river, where the declivity is gentle. After passing over this declivity, I found granular limestone and mica-slate, from where the rock first appears, to the height of about two hundred feet above the river. For the next two or three hundred feet, there followed a gneiss, resembling that which I have already mentioned as occurring near Blair, on the western side of the Tilt. It is rarely stratified in its texture, and its stratification can only be learned from the outgoings of its large beds. These, as well as the limestone and mica-slate below them, dip into the mountain to the south-east, at an angle sometimes considerably more than 45° , and sometimes a little less. To the gneiss succeeds mica-slate, conformably stratified; and to that, another gneiss, a good deal similar in its texture to the last. Its beds are crossed by numerous slaty fissures, at right angles to their planes, which give it the delusive appearance of a stratification dipping at right angles to that of the rocks below; while, in fact, the beds lie conformably. Above, are distinct strata of granular quartz, also conformable. This quartz was the highest rock that I examined, and might be about six hundred feet above the river. About a quarter of a mile farther to the eastward, the strata are intersected by another brook; and, as far as I could judge from a distant view, they correspond with those in the first, as to the limestone, and the lower beds of gneiss.

23. The other line, in which the rock was examined, on this side of the valley, sets off from the Tilt, at a bridge near half a
mile

mile above Forest Lodge, a spot remarkable for its distinct exhibition of a junction between the strata and the sienite, in the bed of the river. The southern bank of the river is here the foot of a declivity, several hundred feet in height, and excessively steep, on which the rock appears only at intervals.

24. In the ascent of this declivity, Mr PLAYFAIR saw nothing but granular limestone, to the height of perhaps more than three hundred feet. The beds of the limestone dip southerly, into the face of the mountain. Over the limestone is a bed of porphyry, twelve or fifteen feet thick. The base of the porphyry is a grey compact felspar, and contains numerous small and imperfect crystals, of a reddish felspar, hornblende, and mica. At the foot of the declivity, and close to the bridge, there are lying many blocks of a porphyry, which has a base of grey compact felspar, and is singular for containing distinct crystals, not only of white felspar and quartz, but also of mica in thin hexagonal plates. From the sharp angles of the blocks, they must be supposed to have fallen from the declivity above; but whether they are a variety of the bed just mentioned, or from another rock, we found no opportunity to determine.

25. Above the porphyry is mica-slate, alternating with beds of granular limestone; and Mr PLAYFAIR observed granular limestone upon the brow of this steep face, where it changes to a more gentle slope. This is the highest point that is visible from the Glen below, and, by comparing its level with that of another on the same face, immediately above the Lodge; the height of which was afterwards determined by trigonometrical measurement, he computed it to be about eight hundred feet above the Tilt.

26. From hence the ascent is more gentle for a considerable distance, and the ground is covered with long heath. The rock is rarely seen; but what appears is mica-slate. The line in which Mr PLAYFAIR ascended, conducted him to a
ridge,

ridge, formed on one side by the slope he had passed over, and, on the other, by the declivity of a deep ravine, through which flows a stream that rises in the higher part of Ben y Gloe, and joins the Tilt about three quarters of a mile above the bridge, by a course almost due north, while that of the Tilt is to the south-west. On this ridge he found mica-slate, stretching N. 38° E., and dipping to the south-east at an angle of about 35°.

27. In the direction of south, and perhaps a little west, from this spot, and at the distance of about a quarter of a mile, is a conical eminence, of considerable height, forming the point of Cairn Toorchie. All the way to its summit, he observed the rock to be a granular quartz, in which there occur thin layers of felspar and mica; and thus the stone is a gradation into gneiss. The granular quartz lies above the mica-slate last mentioned, and, as far as he could judge, has the same stretch and dip. Another high and bare point, at the distance of about half a mile on the other side of the ravine, appeared pretty evidently to consist of a similar rock.

28. Mr PLAYFAIR descended towards the north, along the sloping edge of the ridge, by the side of the ravine, and, in the course of his walk, went more than once to the bottom of the ravine, in order to examine the rock. At first, he found mica-slate upon the ridge, and, in the bottom of the ravine, granular quartz. Lower down, there was granular limestone upon the ridge, of the same characters with that which he had seen in ascending the steep declivity above the bridge, and he had no doubt that it extended all the way to the side of the Tilt. The rock, on this part of the ridge, is indeed generally covered with a thick coat of earth; but, in the bottom of the ravine, for several hundred yards before it opens upon Glen Tilt, there is limestone, which many falls in the stream have worn down, and scooped out, into a variety of irregular forms.

We

—We were prevented from examining the higher parts of Ben y Gloe, by an unusually inclement season, which brought showers of snow, for several days together, before the middle of September.

29. On the northern side of Glen Tilt, there is not much of the fixed rock to be seen upon the lower parts of the declivities. At the foot of the lowest fall of the Crochie, and on the west side of it, there is grey sienite, approaching to sienitic greenstone. In the bed of the first torrent to the east of the bridge above Forest Lodge, there is, on the lower part of the mountain, granular quartz, stratified, and dipping at a large angle to the south-west, but irregular. Higher up the same stream, there is sienitic greenstone.

30. The rock was examined in the bed of another torrent, which joins the Tilt between the bridge and a fall a few hundred yards below it. On ascending the course of this stream, the first fall in it discovers hornblende-slate, obscurely stratified, but, as far as could be judged, conforming to the ordinary stretch of the strata on the south side of the valley. A little higher, there is sienitic greenstone, and this continues, as you proceed, to a considerable distance. The hornblende-slate seemed to lie over the sienitic greenstone. In a spot not far below the Lodge, and at the height of perhaps three or four hundred feet above the river, the rock is partly sienitic greenstone, and partly granular quartz.

31. We examined the fixed rock of the crags on the brow of the mountain in one or two places only, nearly opposite to the point M, and it was there a small-grained sienitic greenstone. But very satisfactory information, with respect to the nature of these crags, was afforded by the angular blocks, and loose fragments, strewed over the steep declivity below. We examined numbers of them through almost the whole length of the valley, and found them to consist chiefly of gradations
between

between grey sienite and sienitic greenstone, all bearing a strong resemblance to one another in their general character, as well as to those which have been already mentioned, as occurring in the fixed rocks on different parts of the mountain. In these aggregates, the felspar is commonly whitish, but has various shades of colour, and is sometimes tinged with red. The proportion of the hornblende to the felspar is generally large. The grains are in some instances large and distinct, and in others extremely minute. Quartz is seldom an ingredient, and mica still more rarely. I conceive the most appropriate name for all the varieties to be Sienitic Greenstone, as the majority of them approach more nearly to that substance, than to sienite.

32. Among the angular blocks of sienitic greenstone, there occur also a few of gneiss and granular quartz; and a little below the point marked B in the plan of the river, I saw, on the side of the mountain, some fragments of hornblende-slate and greenstone-porphry.

33. Along the declivity, from a point over against M, to another a little above Forest Lodge, there are occasionally fragments of a sienite, similar to that which appears so often in the bed, and on the banks, of the river. This differs, in several respects, from that which approaches to sienitic greenstone. Its chief ingredient is felspar, sometimes grey, but most commonly red. It also contains less hornblende, and quartz in various proportions. Nearly opposite to the point B, I found a few fragments of it a little below the crags, and about seven hundred feet above the Tilt.

34. To these remarks on the northern bank of the valley, it may be added, that the Griurnon, a point several hundred feet above the crags of sienitic greenstone, consists of granular quartz,
containing

containing specks of felspar, or small cavities, which may be supposed to have been once filled by it.

35. I return now to a farther consideration of the rocks along the bed of the river, which consist partly of strata, and partly of sienite. Our examination of these, and indeed of the whole Glen, was much facilitated by the kind hospitality of the Duke of Atholl, who allowed us to fix our residence at Forrest Lodge for several days.

36. Among the substances that compose the strata, gneiss, hornblende-slate, granular quartz, and granular limestone, have already been mentioned. Mica-slate occurs more rarely. Besides these, there are many compounds of quartz, felspar, mica, hornblende, actinolite, compact dolomite *, chlorite, talc, steatite, and serpentine, which, in different rocks, are found intimately mixed in a great variety of combinations, and in different proportions. As examples of these compounds, I may mention that, among the specimens we collected, one is a compact dolomite, penetrated by chlorite and talc; a second, hornblende-slate, penetrated by carbonate of lime; a third consists of actinolite, felspar, mica, and carbonate of lime; a fourth, of felspar, with hornblende and carbonate of lime; a fifth, of quartz, compact dolomite, and brown felspar; a sixth, of quartz, penetrated with chlorite, talc, and carbonate of lime; and other varieties might be added. We found steatite and

VOL. VII. P. II.

S s

serpentine

* By Compact Dolomite, I mean a magnesian limestone, differing from common dolomite, in having its structure compact, and such as to render the fracture not granular, as in common dolomite, but splintery, and passing to the even. The fracture is thus well described by Mr JAMESON, in his account of the dolomite of I-columb-kill, in his *Mineralogy of the Scottish Isles*; and the name of Compact Dolomite has been used by the Comte de BOURNON, in describing some specimens from that island, in the collection of Mr ALLAN. Compact dolomite is found also in GlenElg, and in Kintail, on the coast opposite to the island of Skye.

serpentine in one or two places only. Almost all these compounds effervesce, more or less, with muriatic acid; and of course carbonate of lime, or carbonate of magnesia, perhaps both, are among their ingredients. Small specks of iron pyrites are frequent in them. One of the most common of them consists of quartz and compact dolomite. The quartz is in a large proportion; but the dolomite renders the mass extremely tough under the hammer, and imparts a little of its characters to the fracture, which sometimes approaches to that of compact felspar. It effervesces with muriatic acid, though often feebly. Its colour is whitish-brown, where it is not decomposed. By decomposition, it assumes a rusty-brown colour near the surface, indicating perhaps that manganese is an ingredient. Specimens from different spots present many varieties in these general characters. In some of the rocks consisting of the compounds above mentioned, neither the form of a bed, nor a stratified structure, is discernible; but we were led to consider them as parts of the strata, from the imperfect crystallisation of their ingredients, and their analogy to others that are distinctly stratified.

37. The rocks of sienite do not indicate stratification, either by the form of a bed, or by the structure of the stone. Of the sienite there are two varieties, one grey, and the other red; the colour of each depending on that of the felspar, which in both is the principal ingredient. Both contain quartz, and agree in most other circumstances. We paid little attention to the grey, as we no where found it in contact with the strata; but the characters, which distinguish it from the grey sienite graduating into sienitic greenstone, on the northern side of the valley, have already been mentioned. In the red sienite, the felspar presents different shades of red, graduating into grey; the hornblende varies much in its proportion, and in the size of its grains,

grains ; and so likewise does the quartz *. Minute crystals of iron pyrites are sometimes interspersed ; and our specimens effervesce slightly with muriatic acid. At a point marked C in the plan of the river, there is a variety of sienite, or rather sienitic greenstone, which I shall have occasion to describe hereafter.

38. In the bed of the river there are four spots, that draw peculiar attention by affording a good exhibition of the rock, and three of them contain the most distinct phenomena of the junctions of the strata with the sienite. They are marked in the plan of the river by the letters A, B, C, and D, and I shall employ these as names in describing them.

39. The spot marked A is the first place where the sienite is met with, in ascending the course of the Tilt. It may be readily found by a singularity in the channel of the river. Some large irregular masses of rock project on either side from the banks, and through them the water has cut a deep and narrow chasm. Above these rocks, the river holds a course nearly coinciding with that of the valley ; on meeting with them, it makes a sharp turn to the southward, and, after falling a few feet, in its passage through the chasm, expands into a pool at the outlet, and resumes its former direction. The direction of the channel through the chasm is nearly N. and S.

S s 2

40. At

* This red sienite bears a close resemblance to that of the Malvern Hills in Worcestershire. I do not remember to have seen one like it any where else.

40. At the lower end of the chasm, the rock is a large bed of granular quartz, stretching across the river in the direction of about N. 56 E. Its stretch is indicated in the plan* of A (Plate XV I.) by a line expressing the water-line upon its southerly face, and referred to by the figure 1. Its dip is to the south-east, at an angle of about 45°. Its structure is not stratified.

41. Immediately below this, (at 2 in the plan) there appears a rock consisting of quartz, penetrated by compact dolomite. Little of it is visible on the southern bank, but what there is, agrees in its general character with the larger masses on the northern, and there is probably a bed of it, conformable to that of granular quartz. The masses of it on the northern bank, are intermixed with others of granular quartz, but we could not pronounce them to be in alternating beds.

42. The rock on the north side is then concealed by the soil for a considerable distance down the river, except that, about forty yards below the quartz rock, (at 3 in the plan), there may be seen in the channel some small masses of gneiss, interstratified with, and graduating into, mica-slate. The stretch of these strata is N. 63° E., and their dip to the south-east, at an angle of about 45°. Some of the gneiss is so highly crystallised, as to have entirely lost its stratified structure, and is here and there in the form of veins, one or two inches in breadth, and cutting the strata of mica-slate at a very small angle. Figure I, in Plate XIX, is a sketch of a horizontal section of a forked vein of this kind.

43. On the southern bank, the strata are much covered by soil for a short distance below the quartz rock, but granular
limestone

* In this plan the rocks were merely sketched in by the eye. So likewise in the plan of D.

limestone begins to appear about ten yards from it. Soon after, the bank of the river turns to the westward, and exhibits the outgoings of the limestone strata for about a hundred yards. Their average stretch may be considered as about N. 30' E., within 20° of the direction of the bank, and their dip is to the south-east., at an angle varying from 26° to 56°. On a close examination, some rapid inflexions may be observed, and among them, a small one is remarkable for setting the planes of the strata at right angles to one another. This is in the slope of the bank, near to where the limestone is first seen below the quartz rock.

44. Returning now to the large bed of quartz, we find next to it, on the eastward, a rock composed of quartz and brown felspar; and this is succeeded by an aggregate of quartz, brown felspar, and hornblende. The granular quartz is not bounded on this side by any tabular face, or strata-seam; and we had reason to think, that the granular quartz passes into the quartz and felspar, and that again into the compound of quartz, felspar, and hornblende. Though the constituent parts of this last rock are those of a sienite, the mass differs from sienite in having them less perfectly crystallised, especially the hornblende. It may be considered as bearing the same relation to sienite, which the gneiss, already described as having an unstratified structure, does to granite, and perhaps may with propriety be called Sienitic Gneiss. Some specimens of it effervesce with muriatic acid.

45. In the rock of quartz and felspar, and in that of quartz, felspar, and hornblende, there are no marks of a stratified structure, nor are there any of a formation in beds; except that, on the south side of the stream, the sienitic gneiss lies very distinctly over, and conformable to, some strata that follow to the eastward of it, and dip to the south. This circumstance, as well

as the oryctognostic characters of these rocks, and the appearances of gradation from the granular quartz, induce me to regard them as parts of the primary strata. Yet, if they are so, they have been subjected to some peculiar cause of irregularity; for the stretch of the face of the granular quartz, which forms their western boundary, is about N. 56° E., and that of the strata, which bound them on the eastward, is at least 80° E. of N., while the distance between these different lines of stretch, measured along the middle of the channel, is less than thirty yards.

46. The strata to the eastward are a few feet in thickness, and consist, partly of hornblende-slate, and partly of granular limestone, varying to a compact dolomite penetrated with quartz. They appear on both sides of the river. Owing to some disorder in them, and a good deal of inflexion in one place, their position could not be well determined; but their average stretch was judged to lie between 80° and 90° E. of N., and their dip is southerly about 40° . It must be remarked, that these strata stretch from 17° to 27° more to the eastward than the gneiss and mica-slate lower down, and from 50° to 60° more to the eastward than the granular limestone on the southern bank.

47. Above this, the channel of the river opens into a broad and straight reach for a hundred and forty yards. On either side the bank is high, and exhibits the bare rock along its foot.

48. Beneath the northern bank, the strata last mentioned are succeeded by the red sienite, which may be seen on that side with few interruptions, through the whole of the reach, and for between forty and fifty yards beyond it. Low rocks of this substance often project into the water.

49. On the southern bank, nothing worth notice appears for near twenty yards, and then (at 4 in the plan) we meet with

strata

strata lying over the red sienite, of which a large mass follows to the eastward. The strata have somewhat of the characters of compact dolomite, and probably consist of this substance intimately blended with a large proportion of others. Their stretch is about N. 75° E., and their dip southerly at a considerable angle. Near the junction there are portions of the strata included in the sienite, so as to produce some resemblance to an interstratification of the two substances.

50. From hence rocks of the sienite appear on this side also, near the water and beneath it, through the rest of the reach, and forty or fifty yards farther. For about sixty yards, and to a point marked 5 in the plan, a junction of the strata and the sienite may be accurately traced in the bank; beyond that, the whole rock is of sienite. The strata, at the junction, lie over the sienite, dipping into the bank at an angle of from 30° to 50° . In some places they are bent. No veins of the sienite are here seen passing into the strata, and in the body of the strata I observed only one small obscure vein of sienite, or perhaps red felspar.

51. But in some of the rocks along the northern bank, there are junctions of strata and the sienite, attended with different circumstances. We found there no continuous strata of great extent, but various detached masses, consisting of granular limestone, gneiss, mica-slate, and hornblende-slate. These masses lie very irregularly; some of them are imbedded in the sienite, and others intersected by veins of it. Two instances deserve to be more particularly described.

52. One occurs in some strata of mica-slate, near the edge of the water, in a spot referred to by the figure 6. In stretch and dip, these were judged to agree nearly with the strata lying under the sienitic gneiss lower down; and this makes their stretch to be N. from 80° to 90° E., and their dip southerly about

about 40° . These strata rest on the main rock of sienite; and from it small veins of the sienite proceed upwards into the strata, and intersect them, running sometimes parallel, and sometimes transverse, to their planes. I think that the veins do not any where exceed an inch in breadth, and that they cannot be traced for more than a foot, owing to a covering of soil; but, though the fact is exhibited on so small a scale, it is very distinct.

53. The other instance is in a mass of gneiss and mica-slate, which stands detached, at a little distance from the bank, and close to the water, at 7 in the plan. Its form is somewhat pyramidal, and its greatest dimensions may be about twelve or fourteen feet long, six or eight broad, and between four and five high. The stratification is very distinct. This mass is cut by veins of the sienite, from half an inch to fourteen inches in thickness, which branch in various directions, and run in some places parallel, in others transverse, to the planes of the strata. The veins may be traced all round the mass, and, on the south-east side, to their connexion with the main body of sienite below. Figure II, in Plate XIX, is a sketch of this rock taken from the south-east, on the opposite bank of the river. There is no attempt at perspective in it, and it must be understood to represent an orthographic projection of the surface of the rock upon an inclined plane, dipping towards the spectator; but care has been taken to convey a faithful idea of the relative position of the strata and the sienite, as actually exhibited upon the surface. The strata on the west side, which rise to the top of the rock, have their stretch N. 168° E., and their dip westerly 51° . At the foot of the rock on the east side, the strata dip nearly at right angles to those on the western. The lines of junction between the strata and the sienite are marked with great precision. Near the top of the rock, the strata that dip to the west, where in contact with the sienite, are a gneiss containing

containing red felspar, but this extends only a few inches, and graduates rapidly into mica-slate.

54. Farther to the eastward, within about twenty yards of the end of this reach of the river, and at 8 in the plan, a cut lately made in carrying the road along the top of the bank, displays a junction of the sienite with some strata of hornblende-slate. The strata lie to the eastward of the sienite, and are visible for about seventeen yards. The cut into the rock is of small depth, and, where the substances are in contact, there is not much to be learnt. But the strata stretch about N. 130° E., and dip to the north-east at an angle of about 60°, thus indicating a position very different from that of most of the strata already described. In these strata there are some veins of sienite.

55. Beyond this, we observed nothing remarkable on either bank of the river for a considerable distance.

56. The spot marked B lies about eight hundred yards, in a straight line, above A. Here we found considerable masses of rock, consisting of gneiss, granular quartz, hornblende-slate, granular limestone, and some of the compound substances already described, in strata variously interposed. Some of the limestone is a fine white marble. Rocks of the red sienite also appear in many parts of the bed of the river, and the face of the southern bank affords a clear view of a body of strata lying over the sienite.

57. To shew the irregularity that prevails among the strata at this place, and how much their positions differ from that of the great body of strata along the south side of the valley, I shall mention the stretch and dip of several masses, within

small distances of each other. It must be recollected, that the ordinary stretch through the valley is between 30° and 60° E. of N., while the dip is uniformly to the south-east. Near the southern bank, a little below the face of rock in which the strata are seen lying over the sienite, are some strata standing up in the river, which stretch N. 97° E., and are almost vertical. Within twenty feet of these, there is, on the north side of the stream, a large mass of strata of gneiss and limestone, which stretch N. 78° E., and dip to the north at an angle of 40° . Not thirty yards from thence, the road, which runs along the edge of the northern bank, has been cut into some strata, composed of actinolite, felspar, mica, and a little carbonate of lime; and these stretch N. 165° E., with an easterly dip of 24° . Within a hundred yards, on the southern bank, and farther up the stream, are some strata, which stretch N. from 78° to 83° E., and dip to the north at a large angle.

58. Where the strata in the southern bank lie over the main rock of sienite, we did not observe that they were cut by any veins of sienite. But such veins abound in most of the neighbouring masses of strata, of which the irregularities have been just described, and they traverse them in every direction. Many of the veins contain little besides red felspar, but the felspar resembles that of the sienite; and I remarked, where the main rock of sienite was in contact with some strata of compact dolomite, penetrated by other minerals, that the substance of the rock was sometimes merely felspar for two or three inches from the line of junction.

59. In the pure white granular limestone, there are veins consisting of reddish felspar, minutely mixed with quartz. The limestone has yielded more readily to the action of the water, and left the veins very distinctly exhibited in their projections on the surface; but the fracture of the rock shews the substance of the vein to be intimately blended with
the

the limestone on its sides. The outlines of these veins are often extremely irregular, and their breadth is rapidly varied.

60. The strata on the northern bank, described above as consisting of actinolite, felspar, mica, and carbonate of lime, are cut by small veins, partly of red felspar, and partly of the red sienite. The outlines of these veins are very regular and distinct.

61. To the eastward of these strata is a greenstone porphyry, similar to that of the dyke below Gow's Bridge. I conceive it to lie here in a bed conformable to the strata.

62. In many of the veins at B, shifts, or slips, are very conspicuous, and they occur even in veins of large dimensions.

63. Within a hundred yards above B, the strata appear in the bed of the river in several places, and are much intersected by veins of sienite, or sometimes perhaps felspar.

64. The spot pointed out by the letter C lies about 260 yards above Forest Lodge. There is here a fall in the river, and large masses of rock appear on both sides of it, but they do not afford much information to the geologist.

65. Those on the southern bank are strata, which seem to consist either of granular quartz, or of compound quartzose substances, similar to those at A. There is a mass of granular quartz standing up in the middle of the stream.

66. On the northern bank, the rocks are an aggregate of red and grey felspar, intermixed with black or greenish hornblende. The hornblende is in a large proportion, and the stone may be considered as one of those gradations between sienite and sienitic greenstone, which have been already described. The

rocks of this substance have not a stratified structure, nor do they appear to form a bed.

67. From the nature of the place, the junction of the sienitic greenstone with the adjoining strata cannot easily be traced, and the rocks of either kind present few remarkable circumstances. We found, however, some pieces of granular quartz and gneiss imbedded in the sienitic greenstone, and there is an appearance of veins of sienite cutting the large mass of granular quartz in the middle of the river.

68. At the foot of the fall, there is a vein of granular limestone approaching to compact dolomite, which cuts the rock of sienitic greenstone for three or four feet, and varies in thickness from an inch to less than half an inch. Its fracture is splintery, but passing to the foliated. Its hardness is considerable; and from this, as well as from the appearance of its decomposed surface, it is evidently penetrated by some siliceous substance. Its colour is a dark greenish-grey; its greenish hue being probably derived from chlorite. It does not graduate into the sienitic greenstone that forms its walls, and there are small pieces of the sienitic greenstone imbedded in it. Its termination could not be seen in either direction.

69. The bridge over the Tilt, rather more than half a mile above Forest Lodge, stands at the place referred to by D. Twenty yards above the bridge, there is a fall of the river, and the arch is thrown between two of the nearest points of the precipices, which extend for a short distance on both sides of the deep pool below the fall. This is by far the most interesting scene in the whole valley for its geological phenomena. For a plan of it, see Plate XVIII.

70. On the southern bank, seventy or eighty yards below the bridge, is the eastern extremity of a high scar, extending from

from this point for about a hundred yards down the river, and forming at once the bank of the river and the foot of the mountain, which rises about eight hundred feet above it, with more than usual steepness. The rocks, that appear in this scar, are strata, consisting for the most part, either of mica-slate, or, more frequently, of granular limestone penetrated with mica. On a close view, they were found to be much bent: however, their general stretch may be estimated at about N. 33° E., and is nearly parallel to the course of the channel at this place. Their dip is to the south-east, commonly at an angle of from 30° to 40° , but in one place they are almost vertical.

71. Over against a point fifty yards below the east end of the scar, (at 1 in the plan) the rock begins to shew itself under the northern bank. The substance of it has here a base of grey compact dolomite, which serves as a cement to angular pieces of quartz, sometimes so numerous as to constitute the largest portion of the stone. The pieces vary in size, from that of a nut to the smallest grains. This stone lies in a bed stretching about N. 48° E.; and nearly in that direction from hence, there is on the other side (at 1) a rock of a similar character, near the east end of the scar, close to the water's edge; so that the bed probably extends across the river*. The dip of the bed we could not determine.

72. On the northern bank, a little farther up the stream, (at 2 in the plan) the rocks present characters that are various and complex. From near the same spot, I have one specimen consisting of quartz, penetrated with talc, chlorite, and carbonate

* In the plan, the bearings of the corresponding substances on the southern bank have in every instance been laid down more to the east of north, than is stated in the description. Perhaps, in sketching the channel of the river, it was made too broad, which would account for the error.

nate of lime or magnesia, and another composed of red felspar, hornblende, and carbonate of lime. The latter is very similar to the red sienite; but the substances are not so highly crystallised, as is commonly the case in the sienite. Within twenty yards, these are succeeded (at 3) by a compound of brown felspar, quartz, and a small proportion of compact dolomite. The decomposed state of these rocks, and their complex characters, make it difficult to ascertain their ingredients with much precision. The rock last described bears some slight marks of stratification, but the stretch indicated by them is in a direction nearly at right angles to that of the strata on the opposite side of the river, for it is nearly at right angles to the course of the channel: it must be observed, however, that the planes of these strata, if such they are, are much bent. Farther up, on the southern bank, (at 3 in the plan) and in the direction of between 43° and 53° E. of N. from this point, there are rocks of substances similar in character to some of those hereabouts; and these are in distinct strata, dipping south-east, conformably with the neighbouring strata of limestone.

73. About the place where these strata appear on the northern bank, the line of the bank turns to the northward; and from hence to the bridge, a distance of near a hundred yards, there is a straight reach of the river. On both sides of it, the banks are high and steep, and the margin of the water is indented by many projections of the rock.

74. In proceeding towards the bridge about twenty yards along the foot of the northern bank, the rock is found to vary considerably. It is often a mixture of compact felspar and quartz, and sometimes a pure granular quartz, but the felspar is generally the predominant substance; sometimes also it approaches to a gneiss. Specks of pyrites occur among these substances. Their structure is not stratified, and, if they are
are

are in beds, the small quantity of the rock, that appears, would hardly discover them to be so.

75. These compound substances are succeeded (at 4 in the plan) by a red sienite, very similar to that at A. Owing to a covering of lichens, which makes every thing obscure, and a reddish tinge, which pervades the rocks last described as well as the sienite, the one appeared on a first view to pass into the other; but, on a closer examination, the lines of junction between the sienite and the granular quartz, even where the latter approaches to a gneiss, were distinctly traced, and veins of the sienite observed to run through the granular quartz. The sienite first shews itself on the southern bank a little farther up, (at 4 in the plan) in the direction of N. between 48° and 53° E. from where it first occurs on the northern.

76. From these points, to the fall above the bridge, a distance of about sixty yards on the southern bank, and between eighty and ninety on the northern, the prevailing rock is the red sienite; but it is interspersed, especially on the north side of the river, with masses of gneiss, granular limestone, hornblende-slate, and granular quartz, from the size of a hazelnut, to that of several feet in every dimension. The stratification of these masses is for the most part distinct. Many of them are imbedded in the sienite; and others are intersected by veins of it, varying in their breadth from a small fraction of an inch to more than a foot.

77. The appearances throughout these rocks are closely analogous; but it may be more satisfactory to give a separate description of the most remarkable of them, subjoining at the same time, that in such spots as are not particularly dwelt upon, we observed no facts materially different from what I have to state. Every thing that I have to notice below the bridge is on the north side of the river.

78. The

78. Fifty yards below the bridge, measuring along the northern bank, there is a large rock, (at 5) which projects farther into the water than any in its neighbourhood. A little to the west of this, is another rock, containing a mass of limestone imbedded in the sienite. The junction of the two substances takes place without any gradation, or, if there is any, it is a very rapid one.

79. The most prominent part of the large projecting rock has its base formed by the main body of the sienite, and its top by several considerable masses of strata, consisting chiefly of hornblende-slate, with some interstratified felspar. These masses appear to rest entirely on the sienite, as the sienite can be traced below all round them. Some of them dip to the north-east, and others at large angles to the south-west. They are intersected by veins of the sienite, several of which were observed to be continuous with the main body of sienite below. One of these, which, though small, was very distinctly traced, is near the top of the rock, on the side towards the bank. Three others may be discovered by a close inspection in the steep face of the rock towards the river. The mass of strata intersected by them consists of hornblende, felspar, and quartz, and may perhaps with propriety be called Sienitic Gneiss. One of the veins is from six to eight inches broad, another about four inches, and the third about an inch and a half. The substance of the two larger veins has precisely the same characters with the sienite of the continuous main rock beneath them. Where the small vein joins the main body of the sienite, the substance of the main body, along the edge of the stratified mass, is chiefly felspar, and so is that of the vein for three or four inches; but above this, the vein is of a sienite similar to the common sienite of the rock below. The stratified masses separated by the larger veins exhibit no remarkable

remarkable correspondence in their outlines on the opposite sides of these veins; but there is a distinct correspondence in those divided by the small vein.

80. Adjoining to this projecting rock, but nearer to the bank, are several masses of gneiss. Along the lines of junction between the gneiss and the sienite, there is sometimes an appearance of gradation, owing to a large proportion of felspar in the gneiss, and an irregular structure in its stratification. We believe there are instances, in which they may be justly said to graduate into each other, but more commonly the line of junction is pretty well defined.

81. Among the rocks immediately to the eastward, we observed an imbedded mass, which, from its general aspect, might be considered as a granular limestone, but exhibited in some parts a singular siliceous character, especially near its junction with the sienite, though the line of junction is precisely marked. This character consists in a fracture that is rather splintery than foliated, a hardness so considerable as to yield with difficulty to the knife, and a greasy lustre. Yet its decomposed surface shows that it contains carbonate of lime, and it effervesces when pulverised. Its colour is a light greenish-grey.

82. Not far from this, there is a vein of limestone, less than an inch in thickness, running between two masses of sienite, and connected with an imbedded mass of limestone. The imbedded mass and the vein have similar oryctognostic characters. The fracture approaches more to the foliated, than that of the substance last described, and the hardness is less. The lustre is greasy, and the stone appears to contain a good deal of magnesia, but not a large proportion of silex.

83. A part of these rocks (near 6 in the plan) is formed by some large masses of granular quartz, containing numerous

specks of a reddish felspar, which give their tinge to the whole substance. The coincidence of colour, and that want of a stratified structure which is common in granular quartz, made it easily mistaken, on a superficial view, for a gradation from the sienite; but a more minute examination proved the line of junction to be well defined.

84. Nearer to the bridge, and from fifteen to twenty yards below it, the northern bank is formed by a rock, that rises almost perpendicularly from the edge of the water. This rock consists principally of the red sienite; but, near the level of the water, there appears in the face of it (at 7 in the plan) a mass of granular quartz, interstratified with hornblende-slate, about ten feet long, and four thick. The stretch of these strata is nearly the same as that of the strata of limestone in the scar lower down on the other side of the river, about N. 33° E. But their dip is at an angle of 47° to the north-west, and therefore in a direction opposite to that of those strata, and opposite to the common dip of the strata on the south side of the valley. They dip immediately into the face of the rock, and, as the sienite may be traced completely round them, they are evidently imbedded in it. These strata are traversed by two veins, composed of quartz and red felspar, in large grains, and not intimately mixed. The veins run almost vertically. They are visible for some feet in the sienite beneath the strata, and under the surface of the water, and one of them is continued in the sienite above the strata. There is a shift in one of these veins.

85. At the bridge the breadth of the river is much contracted, and the rocks on both sides rise many feet above it, with precipitous and irregular faces. Those immediately under the bridge, and to the east of it, are free from lichens, and their surfaces either retain that coarse polish, which they received, when

when formerly cut through and rounded off by the stream, or have had it occasionally refreshed by the action of the water in time of floods. They are thus well prepared to show the contrasted colours of the red sienite, and the blackish hornblende-slate, and to render the phenomena attending the junction of these substances extremely distinct.

86. From the east side of the bridge, there may be seen lying under the road that leads to it from the northern bank, a large body of granular limestone, mixed with felspar, and penetrated by chlorite, and often approaching in its characters to a serpentine. The position of the mass, with respect to its stratification, cannot, I think, be well determined; but I was inclined to consider its stretch as parallel to that of some strata, which rise, a few feet to the eastward of it, on the other side of an intervening chasm, and the stretch of these is N. 115° E. Veins of the sienite appear in the limestone. A part of the main body of sienite also lies over it, and the plane of their junction dips towards the southern bank.

87. The rock, in which the sienite thus lies over the limestone from the south, terminates towards the river (at 8) in a bold projection, affording a distinct section on its top, and on the three sides of its perpendicular face. Its substance is a part of the main body of the sienite, but it contains many imbedded masses, of various sizes, particularly about its summit. Two of them are of granular quartz, and the lines of junction between these and the sienite are perfectly distinct. In the rest we observed nothing but hornblende-slate, consisting entirely of pure black hornblende, except that it is here and there interstratified with a small quantity of red felspar. The angles of the imbedded masses are always quite sharp, even when the mass does not exceed half an inch in its largest dimension. In these masses there are numerous veins, of which the sub-

stance is often quite the same with that of the main body of the sienite, but they also exhibit a variety of gradations from it. The crystallised grains are often larger in the veins; in some of them there is a little mica; and sometimes, though rarely, they contain white felspar mixed with the red. There are small veins of red felspar and hornblende, running parallel to the stratification of the hornblende-slate, and veins of the same sort occasionally traverse the strata. The lines of junction between the veins and the hornblende-slate are generally well defined, and often extremely so; sometimes, however, the substances are blended. I imagine that, by a nice selection of small specimens, a series of gradations might be made out between the hornblende-slate and the sienite; but in the general aspect of the two substances, even where in close contact, there is a marked distinction of character. The veins are sometimes reticulated, and display the greatest irregularity in their outlines and direction. I believe that those parts of the veins, which are near the main body of the sienite, may in almost every instance be traced to be continuous with it.

88. The southern abutment of the bridge rests on a rock, which projects considerably under the arch. The larger part of it is formed of the main body of the sienite, but it contains many masses of hornblende-slate, in which felspar is sometimes interstratified. A large one, on the east side of the rock, contains much felspar, and is penetrated with carbonate of lime. Of the larger masses, some cannot be traced to rest entirely upon the sienite; but others can, and some of the smaller, which dip into the face of the rock, are manifestly imbedded in the sienite. There are adjacent masses, in which a correspondence of form argues that they were once connected. The position of the masses, with regard to their stratification, is very irregular; and as instances of it I may mention, that on
the

the top of the rock, in an area of about ten feet by eight, there is one having its stretch N. 143° E., and its dip south-west at an angle of 40° ; a second having its stretch N. 36° E., and its dip north-west at an angle of 55° ; a third, which appears to have been once united to the last, stretching N. 13° E., and dipping west at an angle of 58° ; and a fourth, stretching N. 108° E., and dipping north at an angle of 55° . The observations were taken in an order from the west to the east side of the rock. Of the four masses, the first and last extend to an unknown depth into the rock below; but the second and third rest entirely upon the sienite. Veins of sienite and felspar, and of gradations between them, are frequent in most of the larger masses, and many of them may be traced to be continuous with the main rock of sienite below. In the small veins there is generally white felspar intermixed with the red. As in the rock last described, there are small veins of felspar mixed with hornblende, running sometimes parallel, and sometimes transverse, to the planes of the strata. The veins also resemble those in the rock at 8 in their reticulation, in the great irregularities of their direction and outlines, and in having their lines of junction commonly distinct. There is here too the same marked difference in the general aspect of the substances of the sienite and the hornblende-slate. Sometimes perhaps the parts in contact are blended, but this is rare. They are usually bounded by lines that are well defined, and some of the imbedded pieces are remarkable for the sharpness of the angles, which they exhibit in the common section of them and the sienite, afforded by the surface of the rock.

89. At the bridge, the channel of the river makes a sharp turn to the eastward. The rocks about the bridge are chiefly formed of the main body of the sienite; but, near the fall, this is
succeeded

succeeded by a body of strata, stretching obliquely across the stream, which is cutting its passage through them. They extend about twenty yards along the northern bank, and twenty-five along the southern, and consist for the most part of gneiss, hornblende-slate, and granular limestone. Some of the hornblende-slate on the north side of the river, is a pure schistose hornblende, similar to that which forms the pieces imbedded in the rock at 8. The average stretch of these strata is about N. 115° E., and their dip to the south-west at an angle so large as 64°; but there are some irregularities from shifts and bendings. In the strata of limestone there are many small undulations, which are clearly marked on the horizontal surface, by parallel curved ridges of interstratified hornblende and felspar: these harder substances have been left projecting, while the softer limestone has been worn away by the water. Fig. III, in Plate XIX, shews the curved line of one of these undulations on the north side of the river. I think that the breadth from *x* to *y* may be about a foot.

90. The strata along the northern bank are not much intersected by veins of the sienite; but those along the southern are so in the most striking manner, especially in the neighbourhood of the main rock of sienite. A blast in the rock on the southern bank procured us specimens of these veins, where they run through some strata of a dark colour, consisting of hornblende, intimately mixed with talc and carbonate of lime, and shewed us that a section of this part of the rock, in any direction, would exhibit a complete net-work of them. The spot, where the blast was made, is near 9 in the plan. From hence I traced the veins in an oblique direction towards the bridge, irregularly branching, and again uniting, among the separated masses of the strata, to their junction with the main
body

body of the sienite, which forms the lower part of the perpendicular face of rock over the river, a few yards below the fall. In tracing the veins, an interruption, even of a few inches, in their actual substance, was scarcely any where passed over; and where any interruption was passed over, it was owing to a fissure, or some other circumstance in the surface of the rock, of such a nature as to cause no doubt of a perfect continuity of the substance of the vein in its original structure. I believe that the various and complex ramifications of the veins, between the spot in which the blast was made, and the main body of the sienite, would allow the connexion to be traced in more lines than one. As the veins approach the main body of the sienite, they grow larger, but contain many masses of the strata imbedded in them. Some of the imbedded masses are of granular limestone. One of these stands nearly vertical, and has its stretch about north-east; while the stretch of another, which is within a foot of it, and also nearly vertical, is at right angles to this.

91. The main body of the sienite appears on the horizontal surface of the rock for some yards to the east of the bridge; and it has been already mentioned that, where the adjoining part of the rock exhibits, on its upper surface, strata intersected by veins of the sienite; the main body of the sienite is to be seen in the lower part of its perpendicular face over the river. These strata, where thus intersected, appear therefore to rest upon the main body of the sienite.

92. The substance of the veins varies much in our specimens. Sometimes it is a sienite consisting of felspar, hornblende, and a considerable proportion of quartz, and precisely similar to some specimens of the main body of sienite, taken from a rock only eight yards nearer to the bridge. This forms the larger veins; but some of the smaller contain only felspar and quartz; and felspar occurs here and there alone in the
smallest.

smallest. There is an evident gradation in these aggregates.

93. In the spot where the blast was made, the veins are so numerous, as to destroy in a great measure the character of stratification in the rock, and in our specimens some of the pieces cut by the veins do not exhibit a stratified structure; but, about six yards farther to the eastward, there are some distinct, and almost undivided, strata, from which we obtained specimens precisely similar, in their oryctognostic characters, to many of the pieces in our specimens of the veins. Among the pieces of strata intersected by these veins, we found also some specimens of serpentine.

94. The lines of junction between the veins and the strata are in general clearly defined, even in some veins of which the breadth does not exceed the sixteenth of an inch, and the angles of the pieces of strata are often sharp. However, there are partial instances of gradation, by an intermixture of substances, along the sides of some of the veins; and, in the smallest of them, the felspar of the vein occasionally assumes the appearance of a streak of red spots, upon the dark ground of the stratified mass. The connexion of such streaks with the compact veins was traced in a number of cases, without any exception.

95. On the south side of the river, the limestone among the more entire strata is intersected by veins of felspar, which are sometimes interrupted, so that pieces of the felspar appear insulated in the surface of the rock. Both the veins and pieces of felspar are very irregular in their form and outlines. These appearances of the felspar resemble those of the veins in the limestone strata at B.

96. The strata of gneiss, on the same side, are in some places traversed by thin veins of white calcareous spar. One of these,
about

about half an inch broad, at length meets a thick stratum of grey limestone, in which, after penetrating for an inch or two, it branches out into small tapering veins, and is lost altogether.

97. On the northern bank (at 10) a large vein of white felspar crosses a part of the strata. Its thickness varies, but is in some places between one and two feet.

98. In the veins of sienite, or its gradations, throughout the rocks I have been describing, shifts are not uncommon.

99. Beyond the body of strata last described, the rock on the southern bank is lost beneath the soil for a considerable distance, and on the northern it appears only in some small portions of strata, in which we did not observe any thing of importance.

100. It will be seen, from the plan of the river, that both the strata and the sienite occur frequently in the intervals between the spots upon which I have dwelt so long; but there is no other considerable section of the rock, nor are the strata and the sienite often seen in contact. In every other rock, in which we observed a junction of them, the appearances are quite analogous to those that have been described.

101. Above the bridge also, for about a mile and a half, and considerably beyond the junction of the Crochie, the rock, where it appears in the bed of the river, consists either of sienite, or of granular quartz, gneiss, and other stratified substances, of the same characters with those already mentioned. The strata are occasionally intersected by veins of the red sienite.

102. The preceding details afford grounds for the following general statement of some of the most important facts.

103. The mountain along the southern side of Glen Tilt consists almost wholly of primary strata; that on the northern is formed chiefly of sienitic greenstone, with gradations into sienite. Through the bottom of the valley the rocks consist partly of stratified substances, and partly of crystallised aggregates, which are commonly sienite, but in one or two places sienitic greenstone. (Paragraphs, 11 to 34, 66.)

104. The great body of strata on the southern side of the valley, forming the base of Ben y Gloe, dips towards the south-east; and some stratified rocks on the southern bank of the river, which, from their situation and general conformity, appear to be connected with this great body, have main rocks of the sienite lying under them. (Parag. 49, 50, 56.) The sienite may therefore be considered as occupying a position lower than that of these strata*.

105. The lines of junction between these stratified rocks and the sienite are in most places distinct. (Parag. 49, 50, 56, 58.)

106. The strata, which occur, with scarcely any visible interruption, through the lower part of the Glen towards Blair, and

* It must be observed, that the fact of the sienite lying under these primary, or primitive, strata, is inconsistent with the geognosy of WERNER, who assigns to sienite a place in his *Overlying Primitive Formation*, that is, in a class of strata, which, according to him, always rest on the primitive strata, when those of the two classes are found in contact.

along the slope of the mountain on the south side of the valley, preserve a considerable degree of uniformity in their stretch and dip, and vary their position slowly by large undulations; on the other hand, some stratified rocks on the southern bank of the river, which appear to be connected with this great body of strata, but are adjacent to the main rocks of sienite, exhibit great varieties of position within a small distance, and rapid changes in the curvature of their planes. (Parag. 9, and from 19 to 27, compared with 43, 45, 46, 50, 70, 89.)

107. Adjacent to some spots, where the main rocks of sienite, and the great body of strata, appear either in contact, or near to each other, there are numerous masses composed of substances which are stratified, and closely resemble those that form the great body. These masses do not touch each other at the present surface, and sections of the rocks in every direction prove that many of them must be completely separate. The smallest of these masses are not larger than a hazel-nut; the larger would measure several feet every way. From the numerous sections of these masses, afforded by the indented and water-worn surfaces of the rocks, we farther learn, that the concealed surfaces of the masses are rough; that their shapes are irregular, and that they often terminate in sharp angles. In position, as to stretch and dip, they often differ extremely from the great body of strata, as well as from each other; and this difference prevails even among such as lie nearest together. (Parag. from 51 to 53, 76, from 78 to 81, 84, 87, 88, 90.)

108. Some of these masses rest upon the main rocks of sienite. (Parag. 52, 53, 79, 87, 88, 91.)

109. The intervals between such of these masses as appear adjacent on the present surface, vary in breadth, from the small

X x 2

fraction

fraction of an inch, to many feet. The intervals exhibit great diversity and irregularity in their forms; but an approach to parallelism between the surfaces of the masses, that lie nearest together, often gives to the interval the form of a vein. In all the various sections of the rock formed by the present surface, the intervals, whether above, below, or on the sides of the masses, appear, with few exceptions, to be completely filled, either with a sienite similar to that of the main rocks, or with felspar, either pure, or mixed with other substances, so as to present evident gradations from that sienite. (Parag. from 51 to 54, from 58 to 60, 76, from 78 to 81, 84, 87, 88, 90, 92.)

110. This appearance of separate masses of strata, with the intervals filled with sienite, was observed only where the great body of strata, and the main rocks of sienite, were each of them at no great distance. (Parag. 3, 6, 7, 21, 22, from 24 to 28.) One or two exceptions, however, occurred farther up the Glen*.

111. In several instances, the sienite and its gradations, which appear, in the form of veins, among the stratified masses resting upon the main rocks of sienite, were observed to join to the main rocks of sienite, without any abrupt change of character between the substance of the vein, and that of the main rock below. (Parag. 52, 53, 79, 87, 88, 90.)

112. The lines of junction between the stratified masses, and the sienite, or its gradations, are in most cases definitely marked. (Parag. 50, 53, 59, 60, 75, from 78 to 81, 83, 87, 88, 90,

* See the latter part of parag. 135, and note D at the end of the paper.

90, 94.) The principal exceptions are, where the strata are of gneiss, or of granular limestone*.

113. Shifts occur in the veins of sienite, and its gradations. (Parag. 62, 84, 98.)

114. These appearances afford grounds for some inferences respecting what took place in the formation of these rocks.

115. A crystalline character in a rock is generally admitted to prove, that it was once in a state to a certain degree fluid. An opposite notion has indeed been advanced by DOLOMIEU, and Mr. GREGORY WATT; but, both the mineralogical appearances, which suggested this opinion to DOLOMIEU, and the experimental results, which led Mr. WATT to it, admit of an easy explanation, from what Sir JAMES HALL ascertained by experiment, with regard to the varieties of structure produced by cooling under different circumstances; and no sound argument against the common opinion can be drawn from what either of them has described. We may therefore infer, that the main rocks of sienite have been in a state to a certain degree fluid; whether by aqueous solution, or igneous fusion, I do not now consider. We may make a similar inference for the sienite, and the gradations from it, which now occupy the intervals between the separate masses of the strata, often assuming the form of veins. I would farther infer, that the fluid from which the main rocks of sienite crystallised, and the fluid from which the veins of sienite, or its gradations, crystallised, were one and the same;

* The least real gradation may in appearance be a considerable one, where the surface of the rock cuts the plane of the junction at a small angle. This is a source of deception to be guarded against in observation.

same; and that so far the main rocks and the veins are of the same formation, or have a common origin. This inference will probably be disputed by some, and I shall therefore offer a few remarks in support of it.

116. All would admit, that every part of one of the main rocks of sienite in the bed of the Tilt was of the same formation. As in any other case of a rock of a crystallised aggregate, such as granite, greenstone, &c. the continuity of the mass, and its uniform, or gently varied, characters, are the grounds for the inference. It is the most simple hypothesis, and the rule that "No more causes are to be admitted than are sufficient to account for the phenomena," authorises us to conclude in favour of one productive process, where no reasons can be shown for inferring more than one. We have seen, however, that among the veins there are some consisting of a sienite precisely similar in its characters to that of the main rocks; and that some such veins are connected with the main rocks, without any abrupt transition. Now, where both these circumstances occur in the vein, the vein and the main rock are parts of one continuous and homogeneous mass; and every reason for admitting that any two parts of the main rock are of one formation, goes equally to prove that the main rock and the vein are of one formation, except in what regards the difference of form in the vein. But this difference affords no just ground of distinction. In several places, especially about the bridge, there are separate masses of the strata imbedded in the sienite. When such masses lie far asunder, the sienite between them is regarded as a part of the main rock: if we look out for such as have still smaller and smaller intervals between them, we shall by and by find the intervening sienite to obtain the form, and the name of a vein; and the sooner, according as we select larger masses

masses of the strata, and such as have their adjacent sides more nearly parallel. The gradation thus pointed out removes any objection from the extreme case of difference in the form of the vein, where the vein is in substance precisely similar to the main rock, and visibly continuous without any abrupt transition.

117. In the next place, we have the evidence of close analogy, for extending the inference of a common origin to those veins in the neighbourhood of the main rocks, which consist of a sienite similar to that of the main rocks, but between which and the main rocks no connection can be traced on the present surface. These veins are in substance similar to the former, and they intersect similar strata in a similar manner. Their proximity to the main rocks therefore argues that they either are, or have been, connected with the main rocks, though that connection does not now appear.

118. With respect to all the veins which consist of a sienite similar to that in the main rocks, an argument may be derived from stating the question in a dilemma, between a common origin and a different one; and the probability of the former must be proportionate to the improbability of the latter. Now a sienite of these precise characters is by no means common, either among minerals in general, or among those of the adjacent district; and it must appear highly improbable, that two separate processes should have agreed in producing it, with such perfect similarity of substance, in these contiguous spots, and there only.

119. Among the hypotheses, by which a separate formation has been ascribed to veins analogous in character, and situation, to those in Glen Tilt, is that of a cotemporaneous formation by secretion from the strata, while yet in a soft state. Should this hypothesis be applied to those veins of sienite in Glen Tilt, which are of a substance precisely similar to that of
the

the main rocks, it may be opposed with peculiar force by the last argument. The formation of the main rocks of sienite cannot be explained by secretion; and the improbability that two separate processes should have agreed in producing in contiguous spots a substance so rare elsewhere, as this sienite, is materially increased, when those two processes are supposed to have differed *in kind*.

120. From the veins, which consist of a sienite similar to that of the main rocks, it is easy to extend the argument to almost all those veins, which consist of various aggregates of felspar and quartz, or felspar and hornblende, with a little mica in one or two places, and those which contain felspar only. These present a great diversity in their ingredients, and in the size of the grains; but their continuity with the veins of sienite, and the gradation of the substances composing them into the sienite, were traced in many instances, even where the veins were most minute and obscure, and their ramifications most complex. These circumstances furnish sufficient grounds for referring their origin to the same fluid mass. Above the bridge, where the specimens were procured by a blast, we found many of the most minute and complex veins of this kind to graduate into veins of sienite, which again were joined to the main rock of sienite, and grew larger as they approached it; while we observed that in a stratified rock, of precisely similar characters, a little farther to the eastward, there were no such veins, or scarcely any. (Parag. 90, 93.) This, while it seems to exclude every explanation of those veins from circumstances attending the formation of the strata, points strongly to the once fluid substance of the main rock of sienite for their undoubted origin.*.

121. We

* See note B at the end of the paper.

121. We may likewise recur to the argument, that veins of these substances appear rarely in the strata of the adjacent country, where no main rock of a crystallised aggregate is near.

122. It may farther be remarked, that the substance of the main rocks of sienite is not quite uniform in its characters, for in them the proportion of hornblende and quartz varies a good deal, and is in some places small. A main rock at B was mentioned, (parag. 58.) which, where in contact with strata of compact dolomite with other ingredients, consists sometimes of felspar only, for two or three inches from the line of junction. A similar fact occurs at D. (Parag. 79.) While, however, we admit, that the substances thus graduating into one another were formed by crystallisation from the same fluid, the similarity in the other circumstances of these veins forces us to conclude, that some very trifling difference of cause was sufficient to determine, whether hornblende and quartz should enter into the composition of the vein, and in what proportions.

123. It must at the same time be allowed, that there are certain veins of similar substances among these strata, of which it may be doubted if they have been formed from the same fluid mass with the main rocks of sienite; such especially are the large vein of white felspar on the east side of the bridge, and the veins of red felspar with quartz, intersecting a rock of sienite, and an imbedded mass of strata, at the foot of the northern bank, below the bridge.

124. The similarity of the stratified substances, lying in separate masses among the sienite, to those which compose the base of Ben y Gloe, and occupy a wide district to the south and west, leaves no room for doubting that both are of the same formation. But if this be admitted, and likewise the common origin of the sienite in the main rocks, and in the

veins, the intermixture of the stratified masses with the sienite remains to be accounted for, and the case seems to admit of only two hypotheses. One is, That after the sienite had assumed its present form, the stratified substances were deposited in the cavities of its surface. But the forms of these supposed cavities,—the absence of crystals on their sides, as now exhibited in the lines of junction,—the structure also of the veins, dividing upon the sharp angles of the interjacent masses, branching with every variety of size and direction, and crossing each other among the stratified masses,—all these circumstances make it difficult to imagine, that the sienite would have received its present appearance, if at perfect liberty to assume that which crystallisation tended to give; and the diversities in position among the stratified masses, relatively both to the sienite and to one another, furnish the strongest objections to the idea that these positions were original. This hypothesis therefore, though it has been adopted by some to explain analogous appearances elsewhere, may be rejected without hesitation. The hypothesis which remains, is, That the masses of gneiss, limestone, &c. are portions of strata once continuous, and lying conformably with those which form the base of Ben y Gloc; that these strata were bent and broken by some violent shock; and that the fluid, from which the sienite crystallised, was introduced among the fragments.

125. Admitting this as the most probable, the nature of that fluid comes immediately into question. It may be supposed to have been some unknown aqueous fluid, which held the substance of the sienite in solution, and deposited it by crystallisation. To account for the various positions of the fragments, and the height at which they are now found resting upon the main rocks of sienite, it may be supposed, that the fragments fell at various times from the sides of a chasm, in which the deposition

tion of the sienite was going on. But an objection to any idea of a crystalline deposition from an aqueous fluid, arises from the compactness of the rocks consisting of stratified masses intersected by the sienite. This circumstance has been urged by Mr PLAYFAIR, as an objection to the aqueous origin of granite rocks *; and the argument may be applied with greater force to the veins of sienite in Glen Tilt. Had an aqueous solvent deposited this substance among fragments, in some places so confusedly heaped, in others so closely fitted, together, we might have expected to have found occasionally what so frequently occurs in a calc-tuf, a porous structure, and cavities, perhaps lined with crystals. On the contrary, in all these veins, however complex and minute, scarcely a single interstice appears unfilled.

126. It is to heat then that we may with more probability ascribe the fluidity of the sienite. This hypothesis is farther supported by the affinity of sienite to trap, or whinstone; and, in proportion to that affinity, we may extend to sienite the inference of an igneous origin, which, with respect to trap, seems established by so many convincing arguments. The crystallised aggregates of Glen Tilt afford excellent examples of gradations between these substances, and the first steps of the series from trap may be traced in the sienitic greenstones of the northern side of the valley; some of which very nearly resemble the most highly crystallised specimens of common greenstone from Salisbury Craigs. The sienite in the bed of the river may be considered again as a step in a continuation of the series to granite †. But, though these

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gradations

* See *Huttonian Theory*, art. 81.

† This series of gradations, in composition as well as structure, between trap and granite, does not appear to have been duly noticed; and I would particularly recommend it to the attention of those geologists, who, while they admit

the

gradations afford a presumption in favour of the igneous origin of all granitic substances, that presumption must always diminish, as the affinity of the substance grows more remote; and we have yet to wait for proofs, as strong as those which the valuable experiments of Sir JAMES HALL have furnished for trap, to be drawn from direct experiments, in the same way, upon sienite and granite.

127. Even admitting the sienite to have been introduced in a state of fluidity from heat, it is still difficult to account for its having completely filled up every interstice among the fragments; and it seems necessary to infer, that it flowed with a strong impulse, while at the same time a pressure of superincumbent matter, confining the adjacent bodies, opposed its introduction. Indeed, if the sienite be inferred to have been fluid from heat, compression must be admitted to have modified the action of that heat, since the carbonate of lime in the sienite appears to have suffered no decomposition. If we ascribe the fracture of the strata, the disorder in which the fragments now lie among the sienite, and the wide separation of some of them, to the force of the impelled fluid, we shall adopt a simple, and connected, explanation of the phenomena. In favour of this it may be further urged, that the fracture and dislocation of the strata are confined to the neighbourhood of the sienite, while those at a distance are comparatively undisturbed.

128. Even close to the junctions, the large masses of strata on the southern bank, similar in their component substances to

the igneous origin of trap, cannot bear the idea of violating by any such hypothesis the dignity of a rock, which has been so long venerated as the first foundation of the earth. The specimens, required to exhibit the whole series, would not be numerous, though the shades of difference, in every step, were slight. Perhaps our island does not afford a better field for illustrating the subject than Glen Tilt; and the mountains to the north-west of it.

to those among the sienite, but more nearly conforming in their position to that of the stratified base of Ben y Gloe, do not contain veins of sienite, or its gradations; (parag. 40, 43, 50, 56, 70.) and this fact, while it goes to exclude the idea of secretion, or any other tranquil process, attending the formation of the strata, as the source of the veins, argues that the force, which disturbed the strata, was one which impelled the fluid against, and among, them.

129. Some of the main rocks of sienite were observed to lie underneath large masses of strata, which are comparatively undisturbed, exhibit no veins of sienite, and are probably connected with the great body to the south. This circumstance favours the inference, that the direction of the impelled fluid was either lateral, or from below.—That the direction of the impulse was in some degree from below, may be inferred from another circumstance. On the top of some of the rocks of sienite, near the bridge, there are imbedded masses of hornblende-slate. Among these rocks, that referred to in the plan by the figure 8 is one which exhibits such masses with peculiar distinctness; and, in its perpendicular face, the sienite of the main rock is seen to the depth of several feet below them. The sienite of this rock was found to have its specific gravity 2.67; and of two masses of hornblende-slate resting upon it, one has the specific gravity of 3.00, the other that of 3.01. Hence it appears, that the specific gravity of the hornblende-slate is to that of the sienite, as 100 to 89. Now the specific gravity of the sienite, when fluid, whatever may have been the cause of its fluidity, was probably less than it is at present, and supposing it to have been somewhat greater, it must have been considerably less than that of the hornblende-slate: it can hardly therefore be imagined, that, if the fluid sienite came from above among the fragments, it would have
buoyed

buoyed them up, and raised them to their actual position *. Nor can this be explained even by supposing, that these masses of hornblende-slate were attached to others of gneiss, and limestone; for, while the heaviest specimen of sienite has the specific gravity of 2.67, the lightest of two specimens of gneiss was found to have that of 2.68, and the lightest of three specimens of limestone that of 2.70. A fluid impelled from below, though of a less specific gravity, might raise them; and that they should have remained suspended till the sienite crystallised round them, may be accounted for by the supposition, that the sienite was in that state of viscid fluidity, which earthy bodies, when melted, are known to assume at a temperature not far above their point of crystallisation; joined to the consideration, that the difference of specific gravity could not be great.

130. Any theory ascribing the origin of the sienite to an introduced fluid, of whatsoever kind, would lead us to account for the gradations, which in some places appear upon the lines of junction, by supposing that the stratified masses were acted upon by the fluid, so as to admit of their being penetrated by it, or at least by some of its ingredients. After having ascribed the fluidity of the sienite to heat, it is obvious to refer the mutual action of the substances to the same agent, and to suppose that it softened the strata, so as to enable certain ingredients of the fluid to penetrate them to a small depth. Some instances of gradation, where carbonate of lime is one of the substances,

* The large projecting rock below the bridge furnishes a similar argument. The specific gravity of one specimen of hornblende-slate, from the top of this rock, was found to be 2.95, that of another 2.97: the specific gravity of its sienite was not tried, but the mean specific gravity of two specimens of sienite from D is 2.66.

substances, agree very well with an igneous theory. Sir JAMES HALL found carbonate of lime, when fused under compression, to be a powerful solvent of silex. This offers an explanation of the gradation on the sides of the veins of felspar in the strata of limestone at B, and of some similar appearances at D, (parag. 95.); also of the siliceous character in the mass of limestone adjacent to the sienite at D, below the bridge, (Parag. 81.). In these cases, it may be supposed that the carbonate of lime, when fused by heat, derived a portion of silex from the felspar in contact. The siliceous character of the vein of granular limestone, or compact dolomite, at C, may be accounted for in the same manner. On the other hand, it may be supposed that the sienite of the main rocks derived its small portion of carbonate of lime by solution from the adjacent strata. But, whether we adopt an igneous theory, or any other, there are facts that it will appear difficult to explain; for we have seen that there are masses of hornblende-slate, and other substances, which, though penetrated by the felspar of the sienite, have still preserved a stratified texture, and the sharpness of their angles; and it is not easy to conceive a state of aggregation soft, or porous, enough for the former circumstance, and at the same time firm enough for the latter, or what agent could have produced it*. I have heard

Sir

* A remarkable example of fragments of a rock penetrated by an extraneous substance is mentioned by M. DAUBUISON, as having fallen under his own observation. In a note to § 72. of his translation of WERNER'S *Theory of Veins*, he tells us, that near Freyberg a metalliferous vein was found divided into several small veins, containing between them fragments of gneiss, through which galena was disseminated in so large a quantity, that the quintal of gneiss contained thirty pounds of lead. He adds, as something more remarkable, that it was only the gneiss of the vein, which contained galena; for the gneiss of the rock, even where adjacent to the vein, contained none at all.

Sir JAMES HALL mention the aggregation of curd and jelly, as an analogy to obviate similar difficulties. Perhaps the penetration of a fragment still retaining its shape may in some cases have been effected, by one more soluble component having been withdrawn by the action of a fluid in contact, and having thus left the pores it occupied to be filled with a new substance, derived from the fluid. It is clear, however, that where there is gradation on the lines of junction, it has been owing in some cases to a trifling difference of circumstance; for, among the specimens procured by a blast at D, the lines of junction are precisely marked in some, while in others the red felspar appears disseminated through the same blackish stratified compound for an inch, or more, from the side of the vein.

131. An igneous theory for the sienite might lead us also to ascribe the high state of crystallisation, in some of the stratified masses, to a slow reconsolidation after having been softened by the heat. Sir JAMES HALL's experiments upon the fusion of carbonate of lime under compression, make it more easy to admit this hypothesis with respect to the limestone, than in the case of the gneiss, and some other substances. The important information respecting the properties of carbonate of lime, which we have derived from Sir JAMES HALL's experiments, may assist us in explaining some other appearances in these rocks. Thus we may conceive that the vein of limestone proceeding from an imbedded mass, (par. 82.) was produced from a part of the mass which had been completely melted, and remained so, after the sienite had grown so hard as to admit of a crevice being formed. A similar origin may be ascribed to the vein of hard granular limestone at C, (par. 60.). The veins of calcareous spar, cutting strata of gneiss, (par. 96.) may have been formed, either from fused portions

tions of the adjacent limestone, introduced into crevices in the gneiss, or by secretion from the strata themselves, in which carbonate of lime is frequently an ingredient, and limestone an interstratified substance. Sir JAMES HALL observed that carbonate of lime, when fused under compression, is a viscid substance. In some limestone strata not far from the sienite, on the eastern side of the bridge, (par. 89.) there are rapid incurvations, without fracture, which strongly suggest the idea of a viscid state in the rock, at the time of their formation.

132. It has been mentioned, that there are shifts in some of the veins *. Whatever theory be adopted respecting the manner in which the sienite, and the stratified masses, assumed their present relative position; the shifts prove that, after they had done so, there was a dislocation among these rocks, in which the stratified masses, and the sienite between them, were moved together. But the fact that these rocks are now compact, and without cavities, can hardly be reconciled with the idea of such dislocation; unless by admitting that, when it took place, a certain degree of softness pervaded their whole substance, both the strata, and the sienite. It accords with an igneous theory to explain these circumstances by supposing, that the dislocation occurred not long after the introduction of the sienite, while the heat continued sufficient to maintain a softness through the whole. But the difficulty of conceiving such a soft-

VOL. VII. P. II.

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ness

* In a rock near the bridge, there was noticed (par. 84.) a shift in one of two veins, which are apparently of posterior formation to the sienite. It may be worth while to observe, that these veins dividing both the sienite, and the imbedded mass of strata, can hardly be ascribed to secretion; for it is highly improbable, that the strata, which are of granular quartz and hornblende-slate, should have agreed with the sienite in secreting the same substance; nor is it likely, that a secretion from the one, should have filled that part of the crevice, which had been formed in the other.

ness in all these substances recurs upon us ; and the advocates of an igneous theory cannot pretend to furnish a satisfactory explanation of these, as well as many other more common phenomena of a primary country, till they have acquired a farther knowledge of the properties of bodies composed of felspar, quartz, hornblende, carbonate of lime, mica, &c. when subjected to high temperatures, and regulated cooling,—with compression also, where there is any volatile ingredient. It is to an igneous theory, however, that we may look with the most confident hope, and it seems in vain to expect assistance from any hypothesis of aqueous solution and deposition.

133. The various inferences, for which I have been contending, have been separately considered, both for the sake of keeping the argument distinct, and because I conceive the evidence to be much stronger in favour of some, than of others. The whole hypothesis may be briefly expressed thus : That the sienite, in a state of igneous fusion, was impelled from below, by a violent force, against the strata ; that it bent them, broke them, dispersed them, and filled up the intervals, which it now occupies ; that the fragments of the strata were in some degree softened by the heated sienite, so as to admit of a mutual action ; that, while the whole intermixed mass was still soft, some farther dislocation took place in it, and that all this occurred under a great confining pressure of incumbent matter. This hypothesis coincides, in the main, with that by which Dr HUTTON explained the phenomena in Glen Tilt, in a paper read before this Society in the year 1790, and published in its Transactions. If it differs widely from the speculative views concerning these phenomena, which have been more recently given by Professor JAMESON and Dr MACKNIGHT, I must leave it to the candid inquirer to decide between us ; but, if I have
differed

differed also in my description of the facts, Professor JAMESON'S extensive experience, and acuteness in the discrimination of minerals, might seem to claim submission on my part; did I not consider that his observations must have been liable to a certain degree of bias from a favourite theory,—a bias, from which I can scarcely flatter myself that my own were kept entirely free*.

134. I proceed now to describe the rocks in the bed of the Tilt, as far as the junction of the Tarff, and shall subjoin some observations on the mountains to the north-west of the Glen.

135. For about a mile below the junction of the Chlochan, the channel of the Tilt is perfectly straight, and the bottom of the Glen is contracted to its breadth. The channel is here formed on both sides by rocks of a singular aggregate, approaching in its characters to sienitic greenstone. The colour of the mass is a dark greenish-grey, but varying according to the proportion of its ingredients, which are intimately mixed, and seem to be hornblende, felspar, sometimes compact, and sometimes crystallised, steatite or chlorite, and a good deal of carbonate of lime, or magnesia, perhaps both. The presence of these last ingredients is inferred from its effervescing briskly with muriatic acid. It may not be improper to consider the mass as a compound of hornblende, felspar, and compact dolomite, with a little chlorite, or steatite. Its hardness is not great, but it is very tough under the hammer. It is neither

Z z 2

stratified,

* See note C.

stratified, nor in beds. Sometimes it presents an appearance of veins, a few inches in breadth, and consisting of a brownish-red compact felspar, hornblende in large distinct grains, with a curved foliated fracture, and a little quartz. These veins, if they deserve that name, differ from those of the red sienite already described, in the irregularity of their outlines, and in a slow gradation on their sides into the contiguous mass. In the bed of the Tilt, not far from where it receives the Crochie, there is a rock of a similar character as to its constituents, which is in some places cut by veins of the red sienite, and the lines of junction are there precisely marked.

136. On the east side of the foot of the fall of the Crochie, and nearly on a level with the grey sienite, or sienitic greenstone on the opposite bank, (par. 29.) the rock is of a character similar to that last described, though its structure approaches to slaty. It effervesces slightly with muriatic acid, and seems to be a compact dolomite, penetrated by talc and chlorite. It is probable that a great part of the base of the mountain, between the Crochie and the head of the Glen, consists of similar aggregates; for a cut made by a brook farther up, shewed the rocks, to the height of two or three hundred feet, to have a strong resemblance to those that form the channel of the Tilt*. I consider all these congenerous rocks as belonging to the primary strata. In their constituents they resemble some of the compounds that I enumerated in the general sketch of the strata through the valley; and one of them, which has been mentioned as occurring in the bed of the Tilt, not far from the
junction:

* Some of these substances bear a considerable resemblance to those which appear on the Malvern Hills, in the cut made by the road to Ledbury. The latter are in a state of decomposition, which makes it difficult to discern their ingredients.

junction of the Crochie, and as cut by veins of the red sienite, has in some parts a stratified structure. Their imperfect crystallisation, the small proportion of the felspar, and their brisker effervescence with acids, are the characters which distinguish them from the sienitic greenstones, that form so considerable a part of the northern bank of the valley. Along the foot of this bank, between the Crochie and the junction of the Clochan, angular blocks of sienitic greenstone, with similar varieties, still occur at intervals in great numbers, and shew that the higher parts of the ridge contain a large quantity of this substance.

137. On the south side of the long straight reach of the river, granular limestone appears in two or three places. Near the lower end, there are some beds of it extending about sixty yards close upon the edge of the water, in a face of rock that is nearly vertical, and a few feet in height. The lines, that mark the stratification of the beds, run horizontally in the face of the rock; but at each end the beds are curved upwards in a remarkable manner, so that at the eastern, the lines of stratification make an angle of between 30° and 40° with the horizon, and at the western, an angle of from 40° to 50° . At the west end, the rock immediately in contact with the limestone is concealed, but the dark-grey aggregate appears within a few yards of it on the brink of the river. At the east end, this aggregate lies immediately under the curved extremities of the beds of limestone. See Fig. iv, in Plate XIX.

138. On the same side, not a hundred and fifty yards below the junction of the Chlochan, and within fifty feet above the river, there are beds of limestone, dipping, as usual, into the face of the declivity, but perhaps rather more to the eastward.

139. At:

139. At the junction of the Chlochan, the channel of the Tilt turns at a large angle to the northward, and preserves nearly the same direction to the junction of the Tarff, a distance of about half a mile. The Tarff flows from the west, and, immediately before it joins the Tilt, issues from a deep chasm formed by precipices on either side of the channel, which, near the outlet, is broken by two considerable falls. This spot is called Pull Tarff. Between the junction of the Chlochan and Pull Tarff, there appear at intervals various stratified rocks, consisting of granular limestone, mica-slate, the dark-grey aggregate, and granular quartz. Out of five positions observed with the clinometer, if we omit one that appeared to be owing to a convolution, the rest argue that these strata lie round, and lean against, the base of the mountain, which separates the valleys of the Tarff and the Tilt. Four observations gave successively N. 150° E., 128° E., 133° E., and 101° E.; while the dip was getting round by the east to the north*. Our observations in this quarter terminated at Pull Tarff. Dr HUTTON mentions †, that to the eastward of this, near Falar, he found both granite and primary strata between Glen More and Glen Beg. In following up the course of the Tarff to the westward, he observed that the strata disclosed by that river, while they stretched about east and west, dipped to the northward. In Glen Tarff he met with many tumbled stones, which he describes as "composed of broken schistus, including white granite," by which he of course means fragments of primary strata, intersected by veins of white granite. He also ascended to the summit of the ridge that separates the Tarff from the Tilt, where he found almost every thing to be granite, and but little of the primary strata.

140. The

* See note D.

† In the manuscript, of which some account is given in note C.

140. The extent of the granite, and other crystallised aggregates on this side of Glen Tilt, was farther confirmed by our observations on the mountains more to the south, about Glen Criny and Glen Merk. These mountains do not shew much of the fixed rock, and we were therefore left in most cases to infer the materials of which they consist, from the loose blocks that are scattered on their sides, and along the higher parts of the courses of the streams. In order to make the account as faithful as possible, I shall enumerate the substances that we met with, in the different lines of our walks, beginning with what lies nearest to Forest Lodge.

141. For a considerable distance from the Griurnon towards Cairn y Chlannan, along the broad ridge that joins them, I found fragments of granular quartz, and a few of granite. The summit of Cairn y Chlannan is granular quartz. To the south-west of it, the ground falls into the head of Glen Criny, which opens into Glen Tilt.

142. In ascending from the lower part of Glen Criny towards the Griurnon, but winding to the north, so as to gain the ridge that joins it to Cairn y Chlannan, I saw loose blocks of granular quartz, and sienitic greenstone graduating into grey sienite. This sienitic greenstone, and its gradations, resemble those so abundant on the northern side of Glen Tilt, and in the following pages the terms always denote similar substances. Through the lower part of Glen Criny, the stream runs with a very rapid fall, for between one and two miles, over red granite; which, as far as I could judge from where the observation was made, extends to a point within two or three hundred feet in perpendicular height, and perhaps a furlong in horizontal distance, from the granular quartz that appears at the bridge over the Criny in Glen Tilt. We regret that we had no opportunity of examining the junction of the strata and the granite, in the
channel

channel of the Criny. Dr HUTTON relates, that he with some difficulty climbed up the precipitous bed of a rivulet, which comes from the mountain on the north side of Glen Tilt, and which, from his description, I can only conceive to have been the Criny. He there obtained a view of a section of the rock. "We first," he says, "found the alpine schistus inclined as on the south side, and heading, or rising up, to this north hill; immediately after which, we found the granite under the alpine schistus, seemingly bedded in like manner, and running parallel to the alpine strata." It did not strike me that the granite in Glen Criny was in parallel beds; though I own that my examination was hasty, and I had only a distant view of the lowest part of the Glen.

143. In following up the course of the Criny, many blocks of grey sienite, or rather sienitic greenstone, are seen, and many also of granular quartz. The granular quartz on Cairn y Chlannan and in Glen Criny, contains almost always either specks of felspar, or minute cavities similar to those already mentioned in the granular quartz of the Griurnon. Round the head of the Glen, the blocks of sienitic greenstone lie in great numbers, both on the side of Cairn y Chlannan to the east, and on that of Cairn Vardenoch to the west.

144. I ascended Cairn Vardenoch from the lower part of Glen Criny, and kept along the broad ridge of it, winding round to the westward, till I nearly reached that part which overlooks Glen Merk; and returning by a different track, on the west side of the same ridge, crossed over it, and came down into Glen Criny at a point higher up. In the whole of this walk, except on that part of the ridge which lies towards Glen Merk, I met with scarcely any thing but a small-grained granite, having its felspar red. It appears in loose blocks, and
the

the soil is formed of the decomposed ingredients. On that part of the ridge which lies towards Glen Merk, the loose stones are partly mica-slate, and partly grey porphyry.—On Cairn y Chlannan and Cairn Vardenoch, there occurred also a very few loose fragments of gneiss.

145. Glen Merk merits more attention, as it exhibits a junction between the strata and the granite. The name of Glen Merk is confined to a part of the valley along that stream, extending about two miles above its opening into Glen Tilt. At some distance from the opening, the Merk receives the Deery from the west; and, above this, the Glen has the direction of due north, being formed on the east by the declivity of Cairn Vardenoch, and on the west by that of Ben y Venny. The head of the Glen is closed in by a rocky steep, about three hundred feet high, connecting the two mountains, but deeply cut by the stream, which falls through the dark chasms of a narrow channel.

146. At the falls of the Merk, where the Glen opens into Glen Tilt, there are strata of mica-slate, stretching N. 48° E., and dipping to the south at an angle of 27°. They agree therefore with the general position of the strata through the adjoining part of Glen Tilt. In following up the side of the Merk, to where it receives the Deery, the channel affords a transverse section of the strata. They seem to be either mica-slate, or gneiss; and, as far as could be judged from a distance, they conform in stretch and dip with those at the falls.

147. Before I proceed up the channel of the river, I shall mention what we noticed respecting the mountains on each side. Along the declivity of Cairn Vardenoch, at the distance of more than a mile from the head of Glen Merk, the loose fragments shew the higher part of the mountain to consist either of gneiss, or of a granite, nearly the same in character

with that which occurred so abundantly upon it towards Glen Criny. Farther up, the fragments are chiefly of gneiss; and near the head of the Glen, they are either gneiss, or sienitic greenstone, with its gradations.—On the side of Ben y Venny, near the head of the Glen, we met with sienitic greenstone in the fixed rock, and the loose blocks that had fallen from this mountain were either gneiss, or sienitic greenstone; the latter afforded gradations into grey sienite, for quartz occurred in it frequently. Some of the blocks consisted of felspar, hornblende, and mica.—In ascending the steep at the head of the Glen, on the western side of the stream, we found the rocks, that projected in different places from the surface, to be alternately gneiss, and sienitic greenstone, but could not determine their relative positions.

148. I now resume the description of the bed of the river. On the eastern side of the Merk, a furlong above its junction with the Deery, the strata are of gneiss, stretching N. 85° E., with a southerly dip at an angle of 22° . Fifty yards farther up, the gneiss stretches N. 105° E., and dips to the south at an angle some degrees larger. An hundred and fifty yards farther, there is gneiss interstratified with beds of felspar porphyry; the stretch is here N. 46° E. and the dip southerly at an angle of 28° . Three hundred yards higher up the stream, the strata are again gneiss, stretching N. 63° E., and dipping southerly at an angle of 27° . There are here some appearances of bendings in the strata. We saw nothing but gneiss, with its dip southerly, in the fixed rocks, till we came within a quarter of a mile, or somewhat more, of the head of the Glen; when we met with a junction of gneiss and granite, close to the eastern side of the stream. The strata of gneiss are much disordered; and are cut by veins,—some of granite, that is to say, a compound of felspar, quartz, and mica, and some of felspar and mica.

mica only. We saw here no hornblende. The granite is small grained, and its felspar reddish, of the same colour with that of the gneiss. The veins in some places run parallel to the planes of the strata, and in others cross them. Sometimes there is a gradation between the vein and the gneiss, but the line of junction is often quite distinct. Detached pieces of gneiss, much bent, are imbedded in the granite rock.

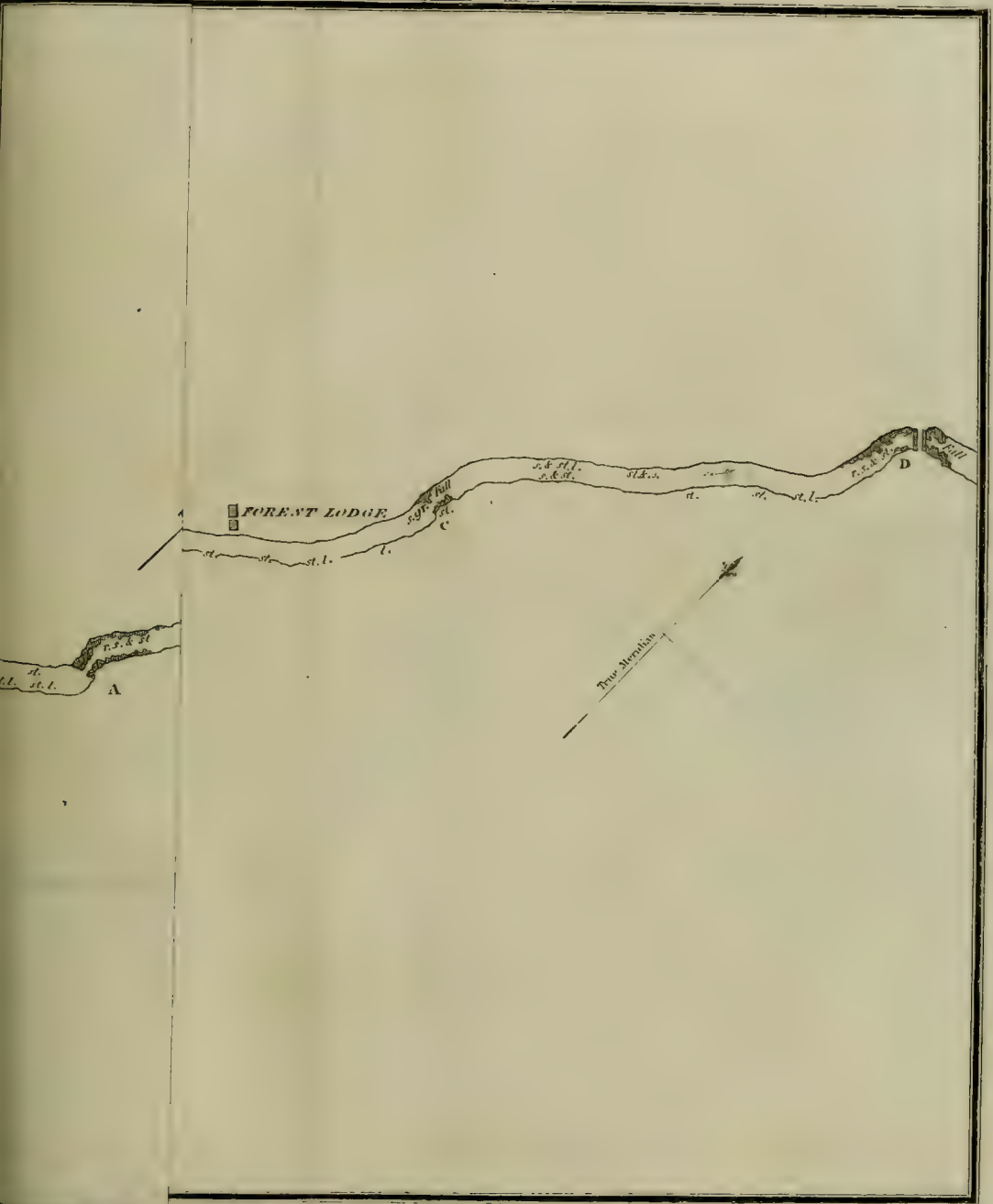
149. The junction is exhibited with remarkable distinctness in a rock, which lies on the brink of the stream, and has received a polish from it. The surface here exposed measures a few feet each way, and is nearly in the same plane, sloping rather obliquely to the water, at an angle of about 20° . (See Plate XIX, Fig. v.) The part next the water is granite; that next the shore is gneiss, of which the strata stretch N. 83° E., dipping northerly at an angle of 42° . It will be observed, that the dip of these strata is in an opposite direction to that of all the strata through the lower part of the Glen. The line of junction runs for about four feet, nearly straight, in a northerly direction, and thus crosses the lines of stratification. It is marked with the greatest precision. About the middle, the line is interrupted by a small vein of granite, which projects from the principal mass, intersecting the gneiss parallel to the lines of stratification, and tapering away, till it is lost in the course of a few inches. Near the line of junction, is a small fragment of gneiss imbedded in the granite.—In the loose blocks along the declivity of Cairn Vardenoch, we met with many junctions of granite and gneiss, exhibiting facts analogous to those now described.

150. Eighty yards below the fall of the Merk at the head of the Glen, we found gneiss, dipping about north-east, or perhaps still more to the north, at an angle of between 40° and 45° . Near this there are large masses of granite rock, and

also veins of granite; but the circumstances of the ground render it difficult to trace the junctions of the granite with the gneiss. At the foot of the fall there is gneiss, lying nearly as that last mentioned.

151. There is an obvious analogy in the appearances, at this junction of granite and gneiss, to those at the junctions of the sienite and strata in Glen Tilt, and they afford strong grounds for adopting a similar hypothesis with respect to their origin.

ADDITIONAL



SECTION
OF THE
COURSE OF THE RIVER
IN
SOME TIME

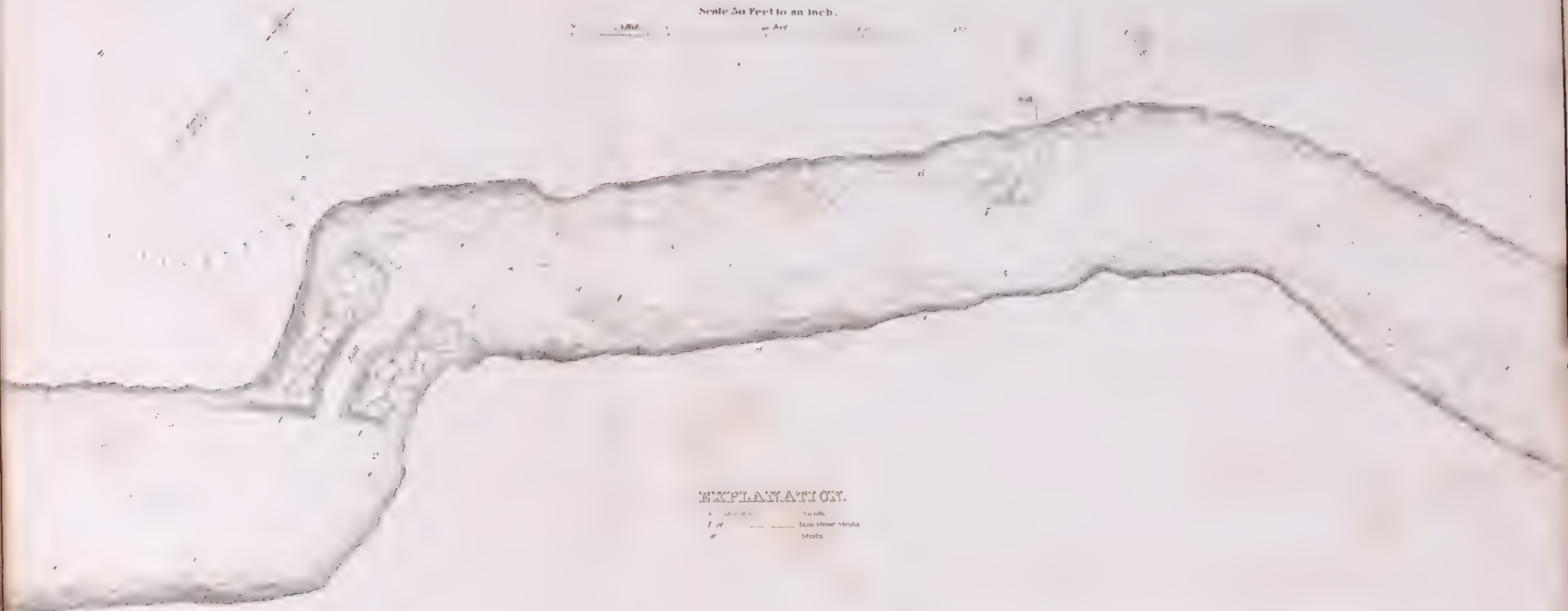
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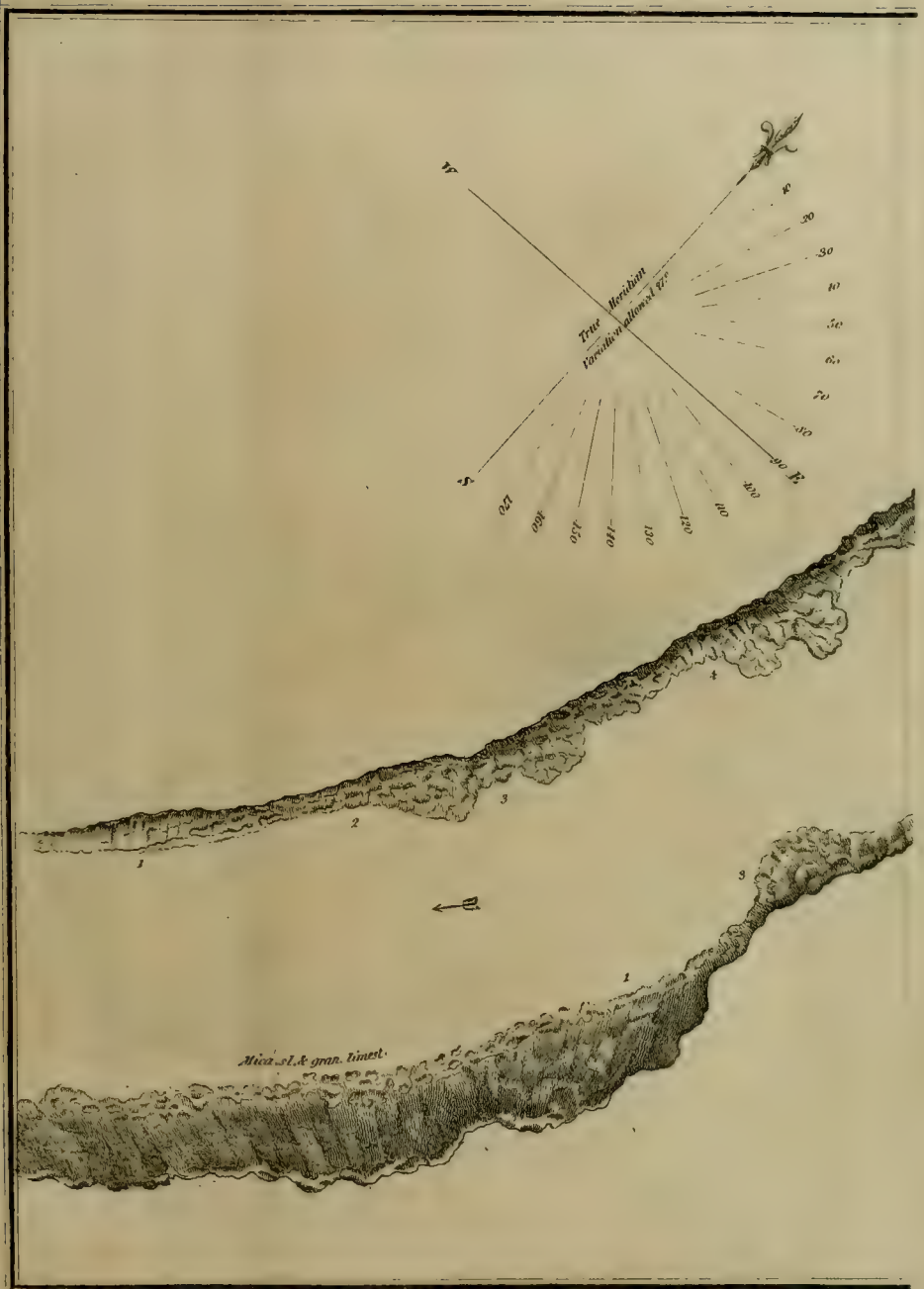
PLAN
OF THE
BED OF THE TILT.
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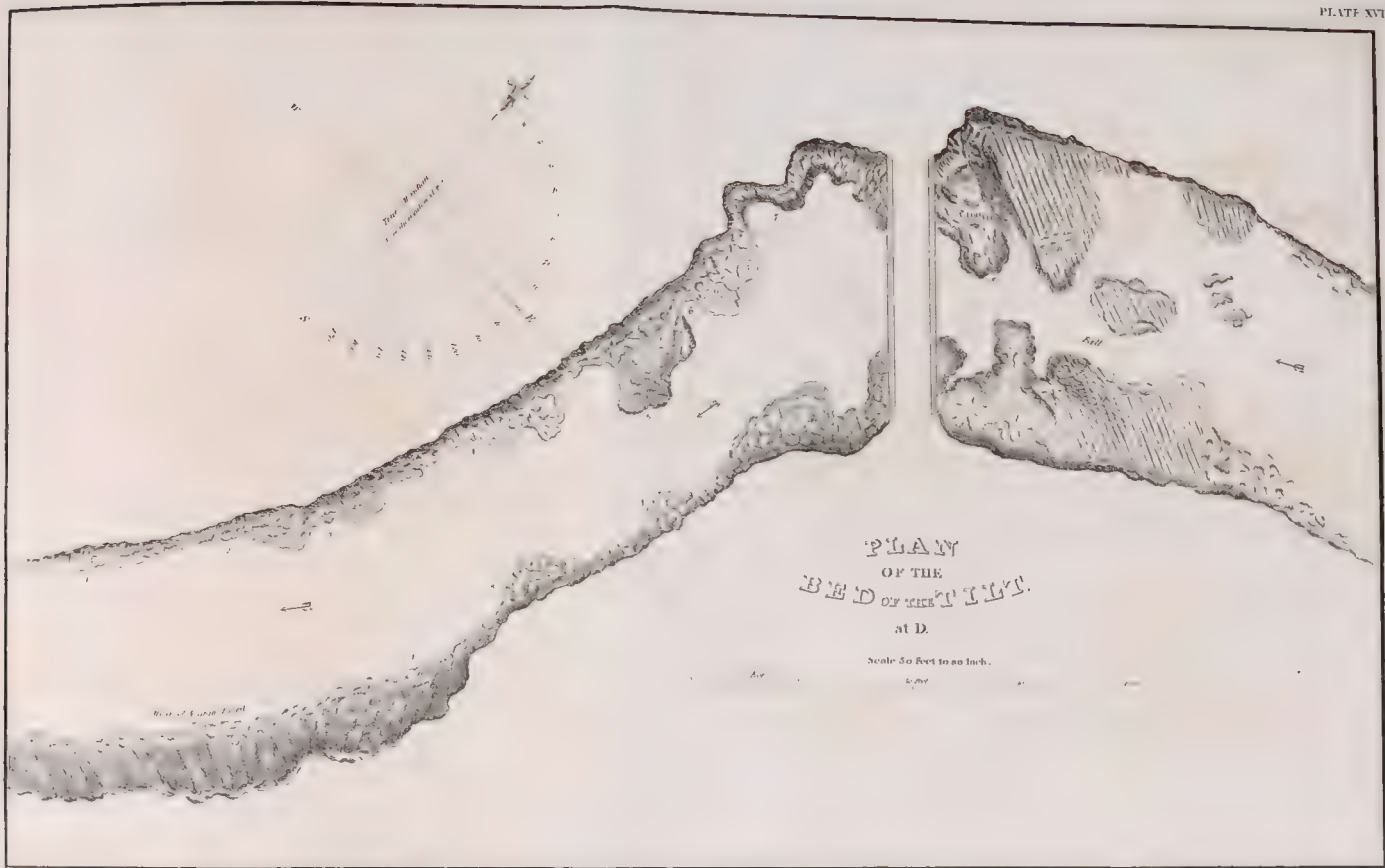
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EXPLANATION.

- Shale
- Sandstone
- Limestone Strata
- Strata





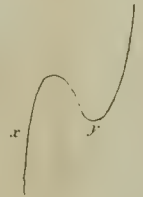
PLAN
OF THE
BED OF THE TILT.

at D.

Scale 50 Feet to an Inch.

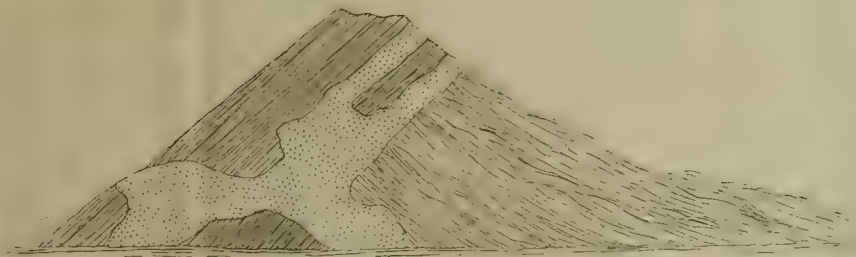
417

Fig. 1.



Gneiss

Mica Slate



Mica Slate or Gneiss

Granite

Fig. 1



Mass Slates

Fig. 2

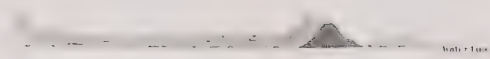


Fig. 3

- Granitic Limestone
- Old Sandstone
- Gneiss



Mass Slates or Gneiss
Sediments

Fig. 4

Northerly

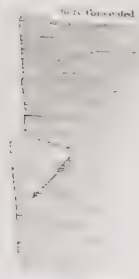


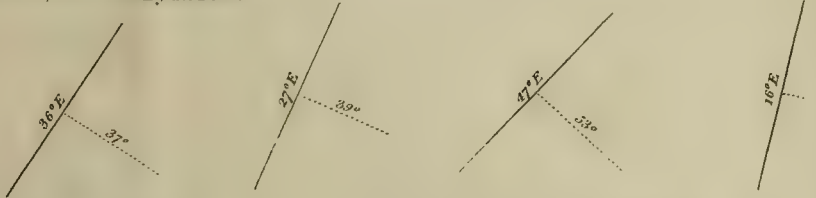
Fig. 5

West

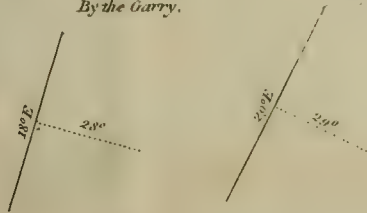
East

- Gneiss
 - Granite
- ↙ Direction of the slope of the surface

By the Bruar.



By the Garry.



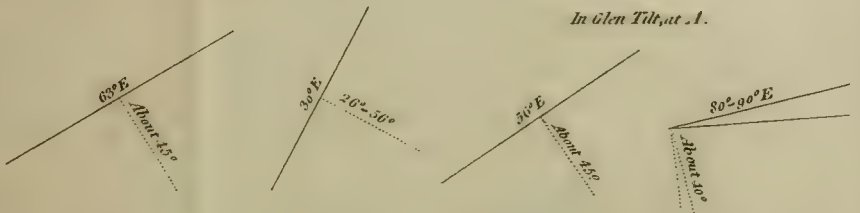
Near Blair.

By the Tilt.

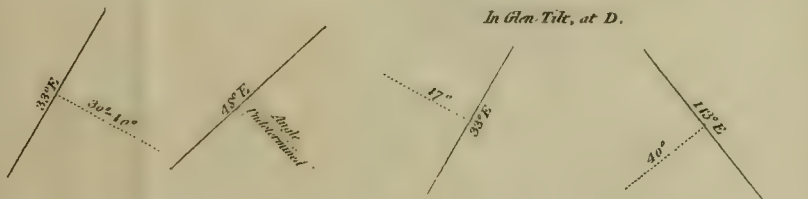
By the B.



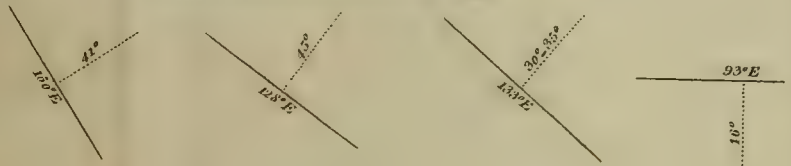
In Glen Tilt, at A.



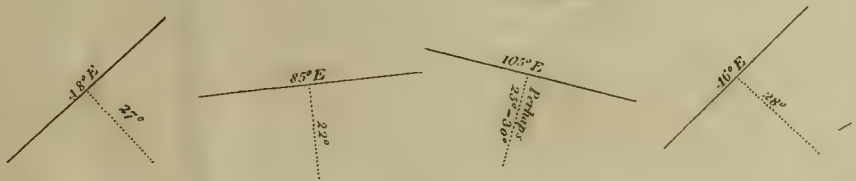
In Glen Tilt, at D.



By the Tilt, between the Chlochan & the Tarff.



In Glen Mirk.



By the Plumb



COMPARATIVE TABLE
— of the —
POSITIONS of STRATA.

N.B. The continued line indicates the stretch, & the dotted line the dip.

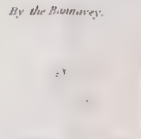
By the survey



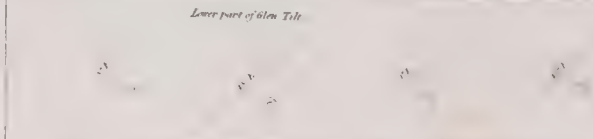
By the Tilt



By the Blunt

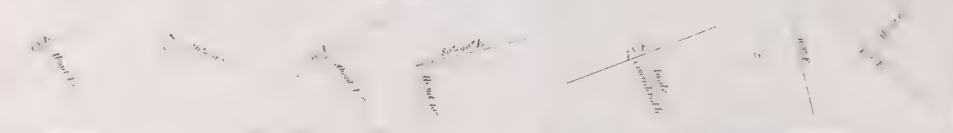


By the Boundary



Lower part of Glen Tilt

In Glen Tilt, at A



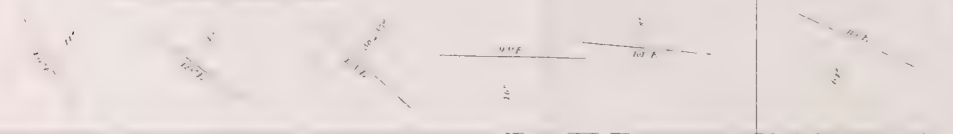
In Glen Tilt, at B



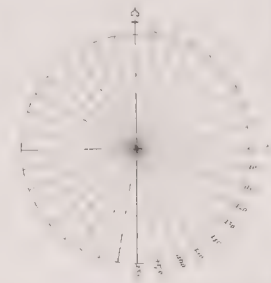
In Glen Tilt, at D



As the Tilt between the chert and the Earth



In Glen Mark



30' 29' 28' 27' 26' 25' 24' 23' P 22'

57'

E N S H I R E

56'

Each Tilt

55'



Meridians West of Edinburgh



PART OF
PERTHSHIRE
 Shewing
GLEN THOL
 and the
 ADJACENT COUNTRY

Scale of British Standard Feet



ADDITIONAL NOTES.

Note A, parag. 2.

In tracing up the course of the Bruar, we made the following observations.

About a hundred yards north from the road to Dalnacardoch, the rock by the side of the river is mica-slate, stretching N. 36° E., and dipping south-east at an angle of 37° . Immediately above this, the river, in its descent through a long succession of falls, crosses the strata, and displays a deep section of them at the bottom of a ravine. The prevailing rock is mica-slate.

At the first bridge in the walks made by the Duke of Atholl, there is a bed of grey porphyry interposed between the strata of mica-slate. Its chief ingredient is a brownish-white felspar, in crystals that are crowded together, and not very distinct; and these are imbedded in a base of compact felspar, penetrated by mica and hornblende. Near the second bridge, the mica-slate stretches N. 27° E., and dips S. E. 39° .

About a mile and a half from the road, and not far from the river, there appears another bed of grey porphyry interposed between the strata of mica-slate. The base of this porphyry seems to be compact felspar, minutely penetrated by hornblende; and there are imperfect crystals of hornblende interspersed. The stretch is here N. 47° E., and the dip S. E. 53° .

A quarter of a mile farther up, the rock is of mica-slate and gneiss, stretching N. 16° E., and dipping to the eastward 23° .

In proceeding by the side of the river, we saw mica-slate in several places, to all appearance conformable, in its average dip and stretch, with that last observed.

About

About four miles from the road, we found mica-slate, stretching N. 8° E., and dipping eastward 23° .

About two miles to the north-east of this, lies Ben y Chat, the nearest of that group of mountains, which spreads between the Bruar, the Tarff, and the Tilt; but we found no opportunity of extending our examination farther towards it. Among the rolled stones in, and near, the bed of the Bruar, we saw a few blocks of sienite, both grey and red, and one large block of granite.

The course of the river Garry, above its junction with the Bruar, lies a good deal farther to the west of Blair. In following it up, along the road to Dalnacardoch, for between six and seven miles from Blair, we had frequent opportunities of seeing a fine section of the rock in the channel. The banks are abrupt, and the stream crosses the strata almost at right angles to their stretch. Our examination of them was very cursory; but they seemed to be chiefly mica-slate, and pretty uniform in their stretch and dip. About five miles from Blair, the stretch was N. 18° E., and the dip easterly 28° . Six miles and a half from Blair, the stretch was N. 29° E., and the dip easterly 29° .

From a comparison of the observations in the beds of the Tilt, the Banavie, the Bruar, and the Garry, it appears that the strata, which would be crossed by a line passing through Blair a little to the north of west, and extending about seven miles in that direction from the Tilt, are conformable in dipping towards the south-east; but vary in the extreme differences of their stretch about forty degrees.

The strata in the channel of the Garry are intersected by veins, or dykes, of porphyry. About five miles from Blair, there is a remarkable one crossing the river, and between twenty and thirty yards broad. The base of this porphyry is a reddish brown compact felspar, in which there are imbedded small crystals of white felspar, and very imperfect crystals of hornblende. Where the porphyry joins to the mica-slate, the colour of the base changes to black by a gradation in the course of a few inches. Of this circumstance I can offer no explanation.

Six miles and a half from Blair, there is a bed of porphyry perfectly parallel to the strata, and uniform in its thickness, which is several feet.

feet. This porphyry has for its base a brownish-red compact felspar, which is well characterised, and contains crystals of quartz thinly interspersed. Its red colour makes it very conspicuous on the banks of the river, and the outgoing of the bed may be traced on both sides in the slopes of the mountains.

Note B, Parag. 120.

The minuteness, and intricate reticulation, of the smaller veins, may be urged in support of the hypothesis of secretion. In these points indeed they resemble those veins of quartz, and of calcareous spar, which are often found running in all directions through various stratified substances, and which it is difficult to conceive any other process, but secretion, to have produced. However, after having rejected the hypothesis of secretion for the larger veins of sienite, it would be absurd to have recourse to it for the origin of small veins, which are evident ramifications, and gradations, from them.

The gradation on the sides of the veins in some places may also be insisted on, as favouring the hypothesis of cotemporaneous formation by secretion. But distinctness, or gradation, on the sides of veins, are equivocal circumstances in a question about their formation. That distinctness will not prove posterior formation, is evinced by its occurring in veins, which there is every other reason to suppose secreted; as in the minutely ramified veins of quartz running through clay-slate; in the small veins of calcareous spar in limestone; in the veins of calcareous spar, which cross dykes of whinstone, and do not penetrate their walls*; also in the lenticular veins of brown spar, or
spathose

* A good example of this occurs in the dyke that crosses the Water of Leith, above St Bernard's Well.

spathose iron-ore, in nodules of argillaceous iron-stone. On the other hand, that gradation is no decisive mark of secretion, is proved by instances of it on the sides of veins, of which the posterior formation is undoubted. WERNER mentions examples of it* ; and Mr JAMESON observes in his Geognosy, where treating of such veins, that there are "cases in which the substance of the vein is intimately mixed with its walls †." A fact of this kind was met with in Cornwall in the year 1799, when Mr PLAYFAIR and myself were in that country. A mine near Redruth, called Toll Carne, was then working in a vein containing tinstone. The walls were of granite, and crystals of tinstone were found disseminated through the granite, for a few inches from the sides of the vein. It was regarded by some experienced miners of that district, as quite a singular instance; but WERNER mentions similar facts concerning tinstone ‡.

An analogous example of gradation was described to me by Sir HUMPHRY DAVY, as having been seen by Mr GREENOUGH and himself, on the shore of Loch Erne in Ireland. A projecting rock of considerable height, so as to render it a conspicuous object from a distance, was found to consist of a large vein of trap, or dyke of whinstone, traversing strata of limestone nearly horizontal. At the distance of some yards from the vein, the limestone presented the usual characters of that substance; but, on approaching the vein, it grew more indurated, and to all appearance siliceous, and was highly so, where in contact. The softer parts of the strata were much worn away, and had left the harder parts on the sides of the vein, to form with it a projecting mass.

At Beadnel Bay, on the coast of Northumberland, there is a whinstone dyke, cutting strata, some of which are of limestone, and exhibit near the dyke a change of character very analogous to that just described.

* See *Theory of Veins*, § 58, 72, 75.

† P. 232. See also p. 239.

‡ See *Theory of Veins*, § 72.

described. An account of this dyke was laid before the Geological Society of London, in a paper by Mr BENNETT, read in March 1812. A notice of it appears in *Nicholson's Journal*, for April 1812, which is the source of my information.

Note C, Parag. 133.

When Dr HUTTON communicated his views respecting Glen Tilt to the Society, he signified his intention of giving at some future time a full description of the phenomena he had observed; and, in the first volume of his *Theory of the Earth*, he promised to introduce it in the course of the work. At his death he left a third volume of this publication nearly ready for the press. Mr PLAYFAIR has favoured me with a sight of the manuscript, which contains a more particular account of the author's observations upon the junctions of granite and primary strata, in Glen Tilt, in Arran, and in Galloway. His descriptions are not minute, but were to have been illustrated by engravings, from drawings made on the spot; partly by the late Mr CLERK of Eldin, who accompanied him in two or three of his mineralogical excursions. When describing the appearances in the bed of the Tilt, he says, "The granite is here found breaking, and displacing the strata, in every conceivable manner, including the fragments of the broken strata, and interjected in every possible direction among the strata, which appear. This is to be seen, not in one place only of the valley, but in many places, where the rocks appear, or where the river has laid bare the strata." If Dr HUTTON has called the sienite *granite*, this can hardly be considered as a mistake in nomenclature at the time when he wrote; and, though the Wernerian School have insisted much upon the distinction of those substances, it appears of little consequence in forming a theory for the origin of that great tribe of crystallised aggregates, to which they

both belong. Dr HUTTON's chief error in describing Glen Tilt is, that of stating the mountain on the northern side of the valley to consist of red granite. He was probably led to this false inference, by what he saw in the deep cut made by the Criny, and by loose blocks near its entrance into Glen Tilt.

Note D, Parag. 139.

About sixty yards above the junction of the Chlochan, there are seen, in the bed of the Tilt, strata of a white granular limestone, highly crystallised, interstratified with, and penetrated by, mica and felspar, and containing pyrites. The mica gives it a slaty fracture in the large. The greater facility with which the carbonate of lime is worn away by the water, has caused the felspar to stand up in some places, in a rough and porous crust, on the surface of the rock. The position of these strata differs much from that which is most common in Glen Tilt; for their stretch is N. 150° E., and their dip north-east at an angle of 41°. Those on the eastern bank of the river are cut by red veins, either of felspar or sienite*.

A hundred yards farther up the stream, I again fell in with the dark-grey aggregate.

A hundred and fifty yards higher, there are strata in the face of the hill above the southern bank, which seem to be limestone. They dip to the eastward into the face of the hill, but are much bent.

In the course of another hundred yards, the dark-grey aggregate again occurs in the bed of the river.

A hundred and fifty yards farther up, there is mica-slate, stretching N. 128° E., and dipping to the north-east at an angle of 45°.

The stretch and dip of the strata, where distinct, continues much the same for a hundred and fifty yards, when there appears mica-slate,

* This is one of the exceptions alluded to in parag. 110.

slate, stretching N. 133° E., and dipping to the north-east from 30° to 35°. About a hundred and fifty yards above this, is the junction of the Tarff and the Tilt.

On the southern bank of the Tarff, fifty yards below the fall nearest the junction of the two rivers, the rock is of granular quartz, containing some felspar, and steatite in the small fissures. It effervesces slightly, and seems to be penetrated with carbonate of lime. The stratification of this rock is indistinct, but, as far as I could determine it, its stretch is N. 93° E., and its dip southerly 46°. On the opposite bank the rock is of the same character. Its stratification is very indistinct, but appears to be horizontal, or rather dipping to the north. On the southern bank, nearer to the fall, there are strata of mica-slate, very distinct, stretching N. 101° E. and dipping northerly 38°. Some of these strata are only thirty yards from the granular quartz, which has nearly the same stretch, and dips at nearly the same angle, but in an opposite direction. As these strata were, no doubt, once conformable, there is reason to imagine that they have undergone an extraordinary convolution, and some traces of it may be discovered in the intervening face of the bank, though it does not afford so clear a section of the rock, as could be wished.



XII. *On certain Appearances observed in the Dissection of the Eyes of Fishes.* By JAMES L. DRUMMOND, M. D. of Belfast. Communicated by Dr THOMAS BROWN, Professor of Moral Philosophy in the University of Edinburgh.

(Read 2d May 1814.)

SOME months ago, in investigating the anatomy of the eyes of fishes, I washed off the back silvery part of the choroid coat of the haddock, with a hair pencil, and about half a teaspoonful of water. The latter became of a milky colour, and on examining a drop of it, with an ELLIS's aquatic microscope, I found the milkiness to be owing to innumerable slender, flat, silvery spicula, which had composed the *substance* of the choroid. They seemed to be in constant motion, apparently rolling upon their axes, but having no degree of progressive movement. The light reflected from their surface was very brilliant, like that from polished silver, and often disappeared, and again returned, with alternations so rapid, as to produce a twinkling, very like that of a fixed star.

Sometimes, on examining an individual specimen, it would disappear altogether, but in a few minutes return, renew its twinkling, and apparent revolution on its axis, and again disappear, to return as before.

Frequently

Frequently also, some were observed to be in the fluid, or on its surface, for a long time motionless. but very brilliant; then they would give a few slight twinkles, seem to turn round, and almost disappear; then resume their original situation for a moment, appear more brilliant than at first, partly disappear again, and again return, and so on, for a number of times, till at length they would disappear entirely; but, after a time, perhaps five or ten minutes, shew themselves in the same spot as before. These observations could be made only on the larger spicula; the minute ones being in incessant motion.

On the first examination of this appearance, it seemed probable, that the motion might be communicated by animalcula swimming through the fluid, and that these had been propagated after the death of the fish, as the eye was not quite fresh. Soon afterwards, however, I examined the choroid in the same way, from a flounder before it was quite dead. The same appearance presented itself, if possible still more lively; and I found also, that it continued in many of the spicula, after exposure to the heat of boiling water.

Since that period, I have observed the same phænomena in the choroid of all the fishes which I have examined, in the cartilaginous as well as the bony.

The motion continues in a great many of the spicula even after the fluid containing them has become putrid; but it is then more slow. The addition of ardent spirit deadens, but does not destroy the motion. After exposure to a heat of boiling water, the number of spicula seems much diminished, and those which remain move less rapidly than before. The addition of vinegar, in a quantity equal to the fluid containing the spicula, suddenly causes a great diminution of the number of moving ones, probably from coagulating the albuminous matter, which had been washed from the eye along with the spicula,

cula, and entangling them in it. Many, however, continue their motion as before.

Whether these bodies really turn upon their axes, or whether they have the property of producing a rapid renewal and cessation of reflection of the rays of light, from their surfaces, so as to produce only the appearance of rotation, I cannot positively determine. In the larger spicula of the choroid of the herring, I have certainly, sometimes, been able to trace the opaque outline of an individual spiculum, after it had ceased its twinkling reflection; but, in general, the appearance of rotation seems so distinct, as almost to preclude the supposition of its not really taking place; and these bodies are not so minute as to admit of much optical deception; for in those from some fishes, the herring in particular, the reflection, and apparent motion, are quite evident, in the sunshine, to the naked eye.

The spicula of the eye of the herring are jointed, being generally thus divided into three distinct portions, of which that which forms the centre is much larger than the two others. In common day-light, the entire spiculum is silvery; but if it be observed in the sunshine, it will be found to reflect different rays, from the different jointed portions; the end-joints being generally of a light straw colour, while the central one is steel-blue, like the main-spring of a watch, or of a red or light rose colour, sometimes silvery, green, or purple; but never of the same colour as the extremities of the spiculum. The colours of the different joints do not shade into each other, but terminate abruptly by a well-defined line.

To account for the motion of these spicula is difficult. There seems no reason to suppose it to depend, either on electrical or galvanic influence. The motion of each spiculum is proper to itself, nothing like attraction, or repulsion, subsisting
between

between it and any other, or between it and either the surface or bottom of the fluid.

Evaporation, causing an intestine motion in the particles of the fluid, cannot be the cause; for the addition of ardent spirit, which increases the evaporation, deadens the motion, and the motion continues when the liquid is covered over with almond oil, or inclosed in a glass tube.

The spicula are a little heavier than water; for, on standing, they slowly settle at the bottom; but the slow manner of their subsiding, and consequent turning, from side to side, is not the cause of the motion mentioned; for when a stratum is spread on a piece of glass with a hair-pencil, as thin as possible, the motion continues till the whole water is evaporated; and if a drop of oil is let fall on such a stratum of fluid containing the spicula, the motion continues under the oil without interruption for many hours.

The transmission of caloric will in no way account for it. The motion is totally different from that of particles of dust, &c. caused by the currents formed during the cooling or heating of alcohol or other fluids.

Phosphorescence has no connection with it; the reflection being apparent only when light falls on the surface of the fluid; and the stronger the light the more brilliant, as in other cases, is the reflection.

To suppose that these spicula are animated, would, for many reasons, seem a wild and improbable idea.

First, They resist a heat of 212° , which no animated being is known to do.

This alone might at first seem conclusive, yet, perhaps, it is not entirely so. The experiments of naturalists prove, that the ova of animalcula resist a boiling heat, as, in various vegetable decoctions, which, although poured hot into vessels,
and

and immediately corked up or sealed over, yet are found, after some hours or days, to be replete with myriads of animalcules; but I believe no experiment goes to shew, that any animalcule could itself withstand such a degree of heat. In these obscure departments of nature, however, there is no reasoning with certainty from mere analogy. Before TREMBLEY'S discovery of the nature of the polypus, it could scarcely have been credited, that there existed any animal which might be multiplied almost *ad infinitum*, by cutting it into pieces; and there are many other phænomena in the animal kingdom, which stand as it were insulated and independent of the laws which govern the chain of animation to which they belong. Thus vinegar proves an instantaneous poison to all animalcules found in other fluids, yet the *vibrio aceti* is found in vinegar, and in it alone. Some animalcules, after being dried up for years, as the eels in blighted wheat, revive upon the application of moisture; and the insect named the *puceron*, or vine-fretter (as discovered by BONNET), contrary to all analogy, propagates without sexual intercourse down to the ninth generation. From these, and many other similar occurrences, it must be evident, that analogy, in our researches into the lowest classes of animal life, must be a fallacious guide. Perhaps, therefore, without too far transgressing the bounds of probability, we may admit the possibility of the existence of a being possessed of animation, and yet capable of resisting a heat as high as 212°.

Second, It appears very absurd to suppose, that any organ of a living body should be composed of a congeries of other minute animated beings or animalcules.

But, at one time, it would perhaps have seemed nearly as great an absurdity, to suppose that such beings existed in any of the secretions; yet the discovery of LIEUWENHOECK, and the

observations of succeeding philosophers have proved, that in the seminal fluid animalcules do exist in the greatest abundance.

Third, These spicula, contrary to the nature of animalcules, have no power of progressive motion.

This is no argument whatever against their vitality; for how many animals are fixed to one spot from the first moment of their existence till that of their dissolution. I need only mention some of the *testacea*, as the *lepas*, *anomia*, and *serpula*; and the numerous zoophytes, the inhabitants of which remain for ever fixed, each to the cell which gave it birth. Even in the human body, we find the various species of hydatids under the same circumstances.

Fourth, Animalcules increase in number by long keeping and putrefaction; on the contrary, these spicula, by the same means, become greatly diminished in number.

Here it may be observed, that when animalcula thus increase, they are in their natural and proper element; but the spicula, when removed from the eye, are in the same circumstances as intestinal worms or hydatids, which, when removed from the body, soon die. Besides, the life of the animal which they infest, seems necessary to the continuance of their being; for after the death of the animal, (as in the case of removal,) they soon die also. Hence, allowing these spicula to be animated, the wonder may rather be, how they retain their animation so long after being removed from their natural situation. It would, however, only be another instance of the extraordinary power by which some animals retain vitality, as is strongly evinced in the frog or turtle, which are said to leap or move about even twenty-four hours after the head has been taken off, or the heart cut out.

With

With regard to the little effect which ardent spirit produces on the spicula, it may be remarked, that scarabæi, and other coleopterous insects, often remain a considerable time immersed in spirits before they become motionless, and, when taken out after a lapse of four or five hours, frequently recover.

Fifth, Are not these spicula formed and nourished by blood-vessels as the other parts of the fish are; and, if so, how can they be considered as distinct animals? This question, it must be confessed, is almost unanswerable. To prove that vessels cannot be traced into them, would be to prove nothing; for in many parts of the body, as the cartilages of the joints, vessels cannot, even by the finest injections, be detected. The spicula are chiefly abundant in the cellular substance which connects the outer layer of the choroid to the sclerotica, and there they seem to have little or no adhesion, at least not more than would seem to arise from their confinement among cellular substance. Hence, in merely separating the choroid from the sclerotica, they are obtained in thousands. When the choroid also is dissected off, and gently shaken in a phial with a little water, the liquid very soon becomes milky, from their dispersion through it.

This looseness, or want of connection, is, however, of little consequence with respect to the present argument, since the brain or the pulpy retina may be diffused through water with almost as much ease, yet both are perfectly vascular. The particles, however, into which the brain or retina is broken are quite irregular, and of different sizes or clusters, and of pulpy consistence, and exhibit no appearance of motion. The spicula, on the other hand, seem to be rigid, are always distinct, and of a constant determinate form, and keep up a perpetual motion and reflection of light.

Perhaps many other objections may be opposed to the supposition of animalcular life in these bodies; and yet the strong *expression* of animation, if I may so term it, and air of seeming design, with which the varying motions, sometimes slow, and sometimes rapid, are performed, and the difficulty of otherwise accounting for their motion, whether real or apparent, lead, upon the whole, I think, to this supposition, not as one which we can admit with confidence, but as the *least improbable* conjecture, which, in the present limited state of our knowledge, we can venture to form.

In managing the microscope, the reflector must not be used; for when a strong light is transmitted through the fluid, the spicula become invisible, or are only imperfectly seen. The fluid, therefore, containing them, should be placed upon something of a dark colour, as a piece of glass, with its underside blackened; and, the stronger the light which can be thrown upon the surface, the better; hence they appear uncommonly brilliant and distinct when placed in the sunshine; and, in this situation, with a high magnifier, they present one of the most brilliant and lively microscopic objects that can be imagined; each spiculum seems like a long bar of light, constantly twinkling, with a reflection almost too strong for the eye to sustain. They may be also well seen in a strong candle light, so managed, that it may fall upon the surface.

Perhaps this phænomenon may have been already pointed out; but it is not noticed by ADAM, CUVIER, BLUMENBACH, or any author with whom I am acquainted.

Since

Since these observations were originally made by me on the spicula of the eye, I have been led to extend my inquiries to other parts of the fish ; and though I have not yet had time to vary my observations as fully and widely as with more leisure I would have done, I may state even now, that I have ascertained beyond a doubt, that the brilliant colours of the surface of this splendid tribe of animals are not owing, as CUVIER supposes, to a *mucous** substance, but to bodies in every respect the same as the spicula of the choroid coat of the eye. The scales of fishes are pellucid ; and their brilliant appearance is owing to a thin film, which covers the underside of each scale, and is itself entirely formed of spicula, as is easily proved by scraping off a quantity of scales, and agitating them in water with a stick or other body, so as to detach the films. The water will then be found to contain thousands of moving spicula, which, in the sunshine, may be discerned with the utmost ease by the naked eye. The scales of the salmon answer best for this purpose, as they are large and easily detached.

* “ Of all vertebral animals,” says this illustrious physiologist, “ fishes are the most remarkable for the brilliant and metallic colours which their rete mucosum exhibits. We find in them gold, silver, and copper, tin, lead, and even all the tints which these metals assume in different degrees of oxydation. But as the description of these colours is the province of Natural History, properly so called, we wish merely to point out in this place, that they are produced by the mucous substance which adheres closely to the internal surface of the scales, and with which it is frequently renewed.” Sect. XIV.



XIII. *Observations on the Theory of Language.* By HENRY DEWAR, M. D. *Fellow of the Royal College of Physicians of Edinburgh.*

(*Read 2d January 1815.*)

THOSE philological studies which have been cultivated among polished nations, for their subserviency to literary taste, and their influence on the formation of correct habits of speaking and writing, have gradually suggested the more profound science of Universal Grammar. The business of this science is, not merely to dictate the best manner of connecting words with one another, but to investigate the origin of the various distinctions existing among them, and to discover in what instances these distinctions are dictated by occasional convenience or partial caprice, and in what instances they are essential to the purposes of language. By the aid of these investigations, philosophers have even hoped to trace some of the most important laws of human thought, and to obtain a solution of the most difficult problems of metaphysics. Some of the attempts made to realise these expectations have displayed great ingenuity; but, even with this qualification to recommend them, they have often failed to establish principles on a foundation sufficiently sure to preclude controversy.

Amidst

Amidst the equilibrium of sentiment on such subjects, and the consequent indifference to the investigations themselves which previously prevailed, Mr HORNE TOOKE published his *Diversions of Purley*,—a work which must be acknowledged not only to exhibit a brilliant display of the genius of the author, but to afford a luminous explanation of some points in the science that were formerly obscure.

This author has refuted some of the errors of his predecessors with so much clearness of argument, and exposed their incongruities with such forcible effect, that it might naturally have been expected that these errors would not have been repeated. But his lucubrations have laboured under some disadvantages. The circumstance of his declining to deliver his opinion on some important points has prevented his readers from appreciating the merits of his doctrines, or even from perceiving whether or not he was in possession of a complete system on the subject: while the boldness of his asseverations, the acrimony of his polemics, and the delight which he betrays in confounding the self-complacency of those whom he regards as less enlightened, have probably cherished, in many of his readers, a prejudiced attachment to errors which might otherwise have been considered as exploded.

It is on all hands allowed, that Mr TOOKE had the merit of giving this department of inquiry a more interesting character than it formerly possessed; and those who are disposed to read his work with a view to information, and to pass over all that is dark or uncertain in it, cannot fail to be much instructed. Even where he appears to aim at the subversion of sound and highly valued principles, the facts on which he proceeds are deserving of careful attention; and though we may for a time fail in endeavouring to account for them, they are worthy of
being

being recorded for more complete investigation, as promising to lead us to new and unexceptionable conclusions.

The design of the following paper is to suggest a few considerations for the purpose of solving some difficulties in which the Theory of Language is involved.

It will be requisite, in the first place, to consider minutely the OBJECT OR PURPOSE of Language,—a subject which the greater part of writers, and Mr TOOKE himself, seem to have thought liable to no diversity of opinion, and which is introduced at the beginning of every treatise on universal grammar, rather for the sake of form than with a view to establish any peculiar doctrine.

No grammarian seems to have disputed the justness of this account of the object of language, that it is “to communicate our thoughts to one another.”

We communicate our thoughts, sometimes by presenting the objects of them to the senses of another; sometimes by suggesting them through the medium of signs. When we hold out an apple or a stone, when we point to a river or to a mountain, we communicate to another a thought which exists in our own mind. This act has been reckoned a specimen of natural language. It is, however, with *signs*, and only with that species of signs called *words*, that universal grammar is conversant; and, in our inquiries into this subject, we are to consider the use that is made of these signs, as completely separate from any immediate exhibition of the objects of our thoughts themselves.

Words are, indeed, objects of thought, and are perceived, like other objects, by the senses. They are not, however, presented on their own account, but only for exciting different ideas, for which they conventionally stand as signs. The ori-

ginal objects must have been at some time presented, in order that the words may be understood ; but, when once understood, they perform their office without that aid.

All thoughts which are communicated, must either be affections received immediately from the perceptions of *external* objects, or affections which become communicable in consequence of certain *relations* originally existing betwixt them and external objects.

In communicating thought, we sometimes merely recall thoughts which had on some former occasion existed in the mind of the person addressed ; at other times we convey new ones. This last is the case when we give a person new information.

It is not, however, possible, that any thought which is communicated by words should consist of *new materials* ; because every word, in order to serve the purpose of communication, must be previously understood. All *new* thoughts, therefore, communicated by language, consist of new *conjunctions* of ideas, produced by new arrangements among the words that are used. From differences of arrangement given to the same ideas, new feelings may arise. The varieties of sentiment, depending on the varieties of these arrangements, constitute, in a great measure, the varieties of character and of intellect that exist among mankind. By the use of language, as subservient to these ends, the great fabrics of science and literary taste are erected.

We must, therefore, understand by the communication of thought to which language is subservient, the act of communicating new arrangements of such ideas as were formerly possessed. The resulting feelings are subsequent consequences, which we have it in view to produce ; but the conveyance of them is not the immediate act of language itself.

Mr

Mr TOOKE does not explicitly say, that he considers all language as consisting in expressing the connections of ideas ; but he seems to make some approach to this doctrine. He acknowledges only two essential parts of speech, the Noun and the Verb. The Noun he defines to be “ the simple or complex, the particular or general sign or name of one or more ideas.” The Verb, according to him, is “ the word by which we make our communication.”

In the outset of his speculations, he denies that the mind has any farther concern with language than to receive impressions, that is, to have sensations or feelings. What are called its operations, he says, are merely operations of language. This distinction is perhaps rather frivolous. If language is a human contrivance, both the creation and the application of it must be operations of the mind. He must therefore mean, that it implies no farther operation of the mind than what consists in the contrivance and employment of the means of communication.

“ The consideration of ideas, or of the mind, or things, relative to the parts of speech, will,” he says, “ lead us no farther than to Nouns, that is, the signs of these impressions, or names of ideas : The other part of speech, the Verb, must be accounted for from the necessary use of it in communication. The Verb is, *quod loquimur* ; the Noun, *de quo*.”

This author, however, has not given a full account of the doctrine which he entertained respecting the nature of the Verb. In the end of his second volume, he seems to concur with other grammarians in believing that the idea expressed by most verbs may be expressed also by means of a noun. He says, “ the verb always contains a noun, but, besides this, contains something more ;” and he proposes it as an enigma for exercising the acuteness of the metaphysical world, “ what

“ is that peculiar differential circumstance which, added to the “ definition of a Noun, constitutes the Verb ?” In another part of his work, he refuses to the infinitive mood of the verb the character of a noun, although it appears to be the mere name of the idea contained in the verb. The infinitive mood, indeed, is connected with some of the words in a sentence in a manner differing a little from other nouns ; but in more important points of syntax it agrees with them. Mr TOOKE, however, appears to have considered the radical use of the infinitive mood as the same with that of the verb as distinguished from the noun, and not as, in this respect, analogous to the noun itself. It is difficult to reconcile this doctrine, not only with other doctrines of Mr TOOKE, but with one obvious fact in language itself,—that, wherever the infinitive of a verb occurs, an additional Verb will serve the purpose of completing a sentence, but the addition of a Noun will not. If we take the infinitive of the Verb “ to deceive,” we never can complete a sentence by adding the Noun “ man,” “ friend,” or “ stranger :” but we can complete one by adding the Verb “ dishonours.” “ To deceive dishonours,” is a complete sentence, though not an elegant one. It is not easy to form even a probable conjecture what views Mr TOOKE could entertain of the Verb, in strict consistency with other doctrines which he advances.

The theory adopted by some grammarians who have availed themselves very fully of the improvements of Mr TOOKE is, that the proper business of language is AFFIRMATION OR ASSERTION ; that all sentences, when analysed, are resolved into assertions ; that assertion is the connecting of one idea with another ; and that the office of the verb is to serve as the sign of connection betwixt different ideas. The Substantive Verb is, in this theory, considered as the only pure verb, and all other verbs are supposed to be made up of the Substantive Verb and a Noun.

In fact, the substantive verb is considered as the simple expression for "that peculiar differential circumstance which, added to the definition of a Noun, constitutes the Verb." This is the view adopted by the learned author of the article *Universal Grammar*, in the later editions of the *Encyclopædia Britannica*.

This doctrine does not seem, however, to be acquiesced in by all. The ingenious author of the corresponding article in REES'S *Cyclopædia*, though he does not directly oppose it, seems not to consider affirmation as essential to language. He casually speaks of the verb as losing its power of affirmation in the Imperative Mood. He has not, however, substituted any other theory of the general object of language, nor laid down any general doctrine on the nature of sentences. He does not, indeed, shew himself to be aware, that since, in this instance, he denied a doctrine reckoned by others fundamental, it was incumbent on him either to substitute another, or to prove that none could be obtained.

The theory which considers the business of language as consisting in affirmation, and which considers all sentences as assertions, is certainly recommended by some share of plausibility. We feel, for the most part, that this is our object. We use the noun as the sign of an idea, and the verb as a sign by which we communicate a new connection betwixt one idea and another. The substantive verb, placed betwixt two nouns, signifies that the ideas expressed by these nouns have a certain order of succession in our thoughts,—an order which we endeavour, by means of this sign, to produce in the thoughts of the person to whom we speak. "This cloth is green," when addressed to any person, signifies merely that we connect the idea expressed by "this cloth" with that which is expressed by the term "green." The assertion made is, that a
green:

green colour is an object which has a definite connection with that congeries of other qualities to which we attach the name or description of "this cloth."

From sentences expressing the simpler combinations of ideas we ascend to the formation of others of a more complicated kind. Of this we have an instance in describing the *local situation* of any object. We still only use nouns and verbs for expressing a certain order of ideas. We mention one or more objects to which the chief object of our interest bears some local relation. It makes no difference in the general nature of the act of language, whether these others are previously known, or are now first brought into view for the sake of specifying the relations sustained by that which we intend to describe. The series is still more complicated, when we give an account of a *change of place* which any object has undergone. We now describe a series of ideas composed of two or more simpler series. We assign to the object a particular situation at one moment, and describe its situation as different at a succeeding moment. This is implied in all our ideas of *motion*, on which the greater part of Active Verbs are founded,—simple as they appear, in consequence of the instantaneous resulting sentiments which habit has led us to attach to them. The verbs "to go,"—"to run,"—"to strike,"—"to kill,"—derive their difference of meaning from varieties of arrangement which the same original ideas are intended to assume.

These doctrines concerning the Noun and the Verb, and the universality of the character of Assertion in the composition of sentences, cannot, however, be maintained, unless it can be shewn that all the different kinds of sentences are actually resolvable into assertions, and are originally possessed of that character. Its application to such sentences as I have now alluded to is sufficiently obvious. It might also be shewn to be applicable

applicable to Negations. But how will it apply to Imperatives and Interrogatives? This is a question which deserves particular attention.

The author who writes in the *Encyclopædia Britannica*, reduces Imperative sentences to Assertions, by placing before them the affirming words, "I command you," or, "I request you." The imperative "veni," he says, is exactly equivalent to the assertion "jubeo te venire." If all imperative sentences are assertions, Interrogatives will easily fall under the same denomination, as they are only imperatives in a different form. By interrogatives, we request the person whom we address to give us information. "What is your name?" and "Tell me your name," are equivalent; and both are resolved by this theory into the affirmative sentence, "I beg that you will tell me your name." The Imperative mood is thus considered as an abbreviation.

This doctrine, as applied to Imperatives, however, is unsatisfactory. The brevity of Imperatives has every appearance of being part of their original character. The words prefixed by the author now mentioned do not seem natural to such sentences, and tend rather to encumber than explain them. The sentences into which they are then reduced are, strictly speaking, mere *assertions* of a command or request, and no longer the giving of a command itself. But a command must exist before it can properly be asserted, and no command exists before the Imperative is used.

With a view to substitute a different doctrine on this subject, I shall premise a few general considerations which are necessary to give us right notions respecting the mutual influence of thought and language. I begin with remarking, that we should avoid the error of supposing that language is merely a simple and spontaneous expression of our thoughts. A
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notion has of late obtained general currency among metaphysicians, that, when we speak, we merely think aloud, and that, when we think, we merely speak in secret. The closeness of the connection betwixt thought and language has thus been overrated. The origin of this error may be traced without much difficulty. It is to Language that we owe the greater part of our information. This circumstance creates an attachment to Language which sometimes leads us astray. Another circumstance possessed of the same tendency is, that when we are engaged in private thought, we very generally have some nearer or more distant view of expressing our ideas to others. It is also to be remarked that, independently of any direct intention, we are prone to indulge the pleasure of associating our mental exercises with the imagery of conversation. Thoughts which are thus associated are most apt to be attended to and revolved in the mind. But it ought to be recollected that many thoughts pass through the mind which are never expressed, and have no necessary connection with language whatever.—We sometimes hear it maintained, that we cannot think except through the medium of language. This is to suppose that we never think of an object, before the name of that object occurs to us. According to this theory, the name and the thing represented by it are not even allowed to be strictly collateral. The precedence in point of time is given to the name. A little reflection on the varieties which take place in the usual current of thought will soon lead us to regard the opinion now mentioned as immature. It will probably be found that, though the occurrence of a word sometimes suggests the object which it signifies, the occurrence of the object to the mind as frequently suggests the word which is employed as its name. Whether we think of words or of things, both the one and the other are to be equally considered as Objects of thought, and in
neither

neither case can we be said to think through the medium of Language in any other sense than that which is implied in the familiar fact, that one kind of thought readily suggests another with which it has been associated. The cause of this error seems to be, that our thoughts about words and things are intimately mixed, and the transitions from the one to the other too rapid to be perceptible. Another cause is, that our most considerate speculations are generally subsequent to some looser movements of the mental faculties, in which many words are revolved, for the aid which they afford by suggesting a variety of views. Men, to be sure, very often imagine themselves to be speaking when they are thinking; but, in proportion as this is the case, their thoughts are bewildered and imperfect. Hence persons who are detected speaking aloud in private, are ashamed of having employed language where it could not be subservient to its appropriate uses. When we observe a person's lips moving during his private thoughts, we conclude him to be deficient in presence of mind, and subjected for the moment to a certain degree of hallucination. Reverie is a common failing of human nature. Even the strongest minds are subject to it. That species of it which is called verbal reverie is incident to those whose relish for conversation is accompanied with vanity. It is also extremely prevalent among literary men, in consequence of a peculiar share of their attention being directed to the meaning and choice of words. We find a great difference betwixt the value which an illiterate man of natural good sense and the man of letters attach to words. The former thinks of things without words, and merely employs words from acquired habit when he has occasion to address others: he is satisfied in this case when he knows that he is fully understood, and despises the direct study of words as an idle occupation. But, with litera-

ry men, thinking of things, and thinking of the signs of things, are peculiarly apt to be co-existent. This co-existence, generating confusion, has probably biassed the estimate which metaphysicians have formed of the universal and necessary influence of language.

It has been said in proof of the necessity of language even to the act of thought itself, that there are many mental processes which cannot be conducted without the intervention of signs similar to those of language. Examples of this kind in the mathematical sciences are considered as occurring in great abundance. The signs in such examples become indeed prominent and useful objects of thought to the student of science, but they are perhaps in no greater degree the instruments of thought than land-marks are the instruments of navigation, or milestones the instruments of travelling. When we have completed a mathematical calculation, resulting in a precise and comprehensive theorem, this result is expressed by a more or less complicated sign. The sign in its various uses employs our active thoughts; but we have numerous thoughts about this sign itself, for which we do not in our private meditations use any signs. The leading sign, which we in this instance employ, enables us to refer with facility to our past operations, so as to secure precision in our further studies; but such thoughts as merely serve the purpose of the moment are not even accompanied with the silent use of speech or of signs. We may perhaps find the same fact illustrated in a different way, by attending to the state of our thoughts, in so far as language is concerned, when we exercise the understanding on a moral subject. We shall probably be conscious that, when we obtain a new idea, this often exists for some time in the mind before we can find an appropriate expression for it, and, after all, our best expressions are rather regarded as hints for eliciting

ing a similar idea in others, than signs fully fitted to convey our meaning. It is in this case that we are most certain of having performed an act of understanding. This fact may convince us that language is not the medium of private thought.

Another mistaken view seems in some degree to influence the speculations of metaphysical writers on language, though it is not expressly maintained:—That man is originally *prone* to *communicate all* his thoughts to others, and that language is nothing else than the spontaneous catenations of our thoughts put in words. No analytical inquiry is necessary to demonstrate the fallacy of such an opinion; but it may be useful to keep in mind some facts on the subject, which will serve to put us on our guard against any unconscious leaning to it, in estimating the justness of particular theories of language. A person whose conversation consists of mere thinking aloud is always considered as betraying weakness by an imprudent deviation from the original purposes with which language is employed. We expect a speaker to have some other object in view, than merely to gratify a proneness to communicate his thoughts. Many thoughts which it is reckoned allowable and laudable for him to entertain cannot be expressed to others, without subjecting him to the charge of impertinence.

There is one fact in the history of man; which is certainly worthy of more attention than it generally receives, being too often overlooked in its relation to some other views with which we are occasionally occupied,—that thought is of much earlier date in the life of the individual than language. Inattention to this fact appears to be the reason, that those who have expected to find in etymology a developement of the first prin-

ciples and the earliest history of human thought have not met with adequate success. The origin of thought and the origin of language form distinct subjects of inquiry, the difference betwixt the eras that belong to them not being accidental but essential. There is nothing in the general nature of the thinking faculty of man that implies a proneness to communicate his thoughts to others. His inclination to speak to his neighbour is not the consequence of the simple existence of thought, but arises from a particular kind of thoughts which occupy his mind. His motives for employing any sort of language are to be found in the mutual relations subsisting among the species. The ideas received from inanimate nature might exist in full vigour, and the sensations of pleasure and pain might be varied and lively, and yet never lead to the employment of language. Before language can have existence, a man must perceive that he is surrounded by other sentient beings ; he must know that some analogy subsists betwixt their perceptions and his own ; he must be acquainted with the principles of mutual attention and imitation, which characterise human nature ; and, he must know that it is practicable to influence the thoughts of others by means of signs. After possessing all this knowledge, he must, before he can use language, feel the influence of some particular motive. These circumstances not only enter into the first contrivance of language, but perpetually accompany the employment of it.

In speaking to our neighbour, we employ a certain power of which we find ourselves possessed to rouse his thoughts from a state of inaction, or to direct them to objects different from those which have previously engaged them. The exercise of this power is properly an IMPERATIVE ACT, and the original and most natural expression of it, is in the Imperative mood of the verb.

verb. I may here observe, by the way, that I do not use the the word Imperative as meaning the exercise of a power implying positive superiority in one individual to another, but as expressive of the general characteristic circumstance of personal influence, and including all its forms,—solicitations, for example, as well as commands. An Imperative is implied both in the use of Nouns and of Verbs. A grammarian may call a noun a simple sign of an idea, implying no further movement of the mind ; but he will find, on closer consideration, that every person, in making use of a noun, expresses some intention, and that this intention gives to the simplest word the force of an animated sentence. When we pronounce the name of an object, we desire the person addressed to think of it, and this itself implies an Imperative act along with the particular idea pointed out.

The tendency of the preceding observations is to establish two positions in the theory of grammar, which are connected with each other, and which differ from the doctrines usually entertained. The first is, that the object of language instead of being described as consisting in the communication of our thoughts, should be made to consist in PRODUCING THOUGHT, OR IN INFLUENCING THE THOUGHTS OF OTHERS, BY THE MEDIUM OF WORDS. The other is that, instead of considering all LANGUAGE AS AFFIRMATIVE, we ought to consider it as originally IMPERATIVE.

Those who are averse to think favourably of this doctrine will, on consideration, find it at least to be less liable to some exceptions than that to which it is opposed. These exceptions they may indeed despise as founded in verbal quibbles ; but it ought to be recollected that, in the doctrine of words, the precluding of quibbles is a very important object. I hope to shew that this doctrine will, with the aid of a little illustration,
throw

throw some light on the general nature of Language, and prove materially subservient to the purposes of a rational etymology.

Imperatives seem to be deprived of their characteristic animation when we attempt to resolve them into affirmations. Affirmations, on the other hand, will preserve that interest which originally belongs to them by being considered as imperative directions for regulating the thoughts and volitions of others.

The etymology of verbs in every language illustrates the primary importance of Imperatives. The first purpose for which a man naturally employs language is to implore the assistance of his fellow-creatures. The first ideas, therefore, for which he wishes to have signs are those of locomotion. Words expressing locomotion are employed for a variety of purposes; but that purpose for which a man is likely soonest to call the attention of another to the idea by means of the oral sign, is to solicit him to perform the motion. For this reason, the Imperative is in all languages the shortest part of the Verb. In the English language, it consists in the mere use of the syllables which distinguish each verb from all others. It is in the Imperative that we say, "walk,"—"run,"—and "go." The other uses of the verb, such as that of describing our own motions or those of a third person, are subsequent to it in the order of etymology, as they are in the order of nature. In such cases, we find the Verb receiving a Nominative, a Termination, or some sort of additional sign. For example, "I go,"—"thou goest,"—"he goes." Even when the definitive idea expressed by any verb is introduced as an Object concerning which an assertion is to be made, an additional sign is employed besides that which is used for the imperative. The simple act is not called "walk,"—"run,"—or "go;" but, "to walk,"—"to run,"—"to go;" or "walk-ING,"—"run-

ING,

ING,—GO-ING. “He likes to walk,” or, “he likes walk-ING:” “to walk or walking is conducive to health.” This principle of etymology is equally conspicuous in the Latin language. In the imperatives “ama,” “doce,” “lege,” “audi,” we have merely the letters essential to each verb, together with the terminating vowel corresponding to their respective conjugations. In some imperatives even this vowel is wanting, as in “dic,” “duc,” “fac,” and “fer.” Grammarians call these imperatives irregular, and seem to suppose that, at some period, they have been deprived of the terminating vowel. Unless we had good proof of this, it might perhaps be with equal probability supposed that these words retain a simplicity originally belonging to all imperatives. At all events, they illustrate the tendency to brevity in this form of the verb. It is not necessary to recur to uncertain points in the antiquities of any language, in order to find out the principles of etymology. They are exemplified in every modern language. They are abundantly displayed in those cases in which a new language has been formed from materials surviving that destruction of pre-existing languages which is naturally occasioned by the competition of several of them for current use, in the mingling of different tribes. The same radical principles are even found to operate in the diversities of provincial dialects, and in the variations produced by the restless spirit of novelty. The imperative of the Latin verb *eo*, is as short as can be imagined, as it consists of the single letter *i*. When the definitive idea represented by any verb is considered as an object concerning which an assertion is to be made, intimation of this circumstance is given in the Latin language by the termination “re” added to the imperative. From the preceding imperatives we have “ama-RE,” “doce-RE,” “lege-RE,” “audi-RE,” and “i-RE.” This termination seems

seems to be nothing else than the noun "res" in an indeclinable form. *I* signifies "go;" *I-re*, "go-thing;" *go* considered as a *thing*, or Object of thought. *Vult i-re*, "he wills going, or the thing *to go*."

Those inarticulate sounds which express strong sensation, which are not originally addressed to any hearer, and are uttered even by the deaf and dumb, such as laughing, crying, and expressions of surprise, are the only sounds that precede Imperatives. They are in the first place involuntary, and afterwards uttered from design. A man desirous of impressing another with a particular passion first contrives to excite it in his own mind, and then utters the sound by which it is expressed. Some sounds bearing a resemblance to these are conveyed by articulate syllables, and ranked by grammarians under a head by themselves, to which they give the name of *Interjections*. Mr TOOKE refuses them any place in human language, because they are common to us with brutes, and are in some degree symptoms of the wildness of mental dereliction. Language, however, must include every sound addressed by one man to another, from the lowest to the highest state of mental cultivation. Interjections may be considered as a mixture of involuntary expression with social discourse. In the use of this part of speech, man is seen to rise from the character of an animal impelled by passion, to that of a reflecting being, who discovers intelligence and address in influencing his fellow-creatures. Interjections, in so far as they partake of the nature of social discourse, are real imperatives.

The imperative influence which mankind, by means of language, exert over the thoughts of one another, is subject to a variety of modifications, which depend not so much on the diversities of their thoughts themselves, as on the different forms of the relative situation in which the speaker stands to the per-

son addressed. It is by examining these modifications that we may expect to make the most successful advances in acquiring a philosophical knowledge of the parts of speech. They may be distributed in the following manner :

1. We desire another to perform some voluntary motion. This gives rise to Imperatives in their acknowledged form.

2. We direct his attention to objects that are independent of himself. This gives rise to words that designate External Objects, and to words which connect these with imperatives. Under this head we shall probably be obliged to rank not only substantive and adjective nouns, but participles, adverbs, prepositions, and some conjunctions.

3. We direct him to combine and arrange his ideas in a definite manner. This gives rise to those parts of speech which are subservient to Affirmation.

Lastly, We desire him to give us information. This gives rise to Interrogatives.

Perhaps it would be rash to assert that the different parts of speech would be most advantageously treated in the order now mentioned. It is probable that the analogy subsisting betwixt the inflections and uses of the imperative and the indicative of the verb, would render it advisable to consider them in conjunction. Similar considerations might perhaps justly induce us to give the other parts of speech an arrangement nearer to that which they receive in our common grammars. We shall find, however, that when we trace the prevalent etymologies among each of those kinds of words which are referred to different parts of speech, the radical importance of the imperative of the verb every where appears. This fact will be illustrated by a few observations on the progress of etymology.

As few objects of our knowledge have a natural connection with such sounds as the human voice can imitate, the choice of the respective oral signs appropriated to the expression of our different ideas, is for the most part entirely arbitrary. Hence the origin of the first invented words must have been regulated by such slight and momentary views, that their history was lost before they received an established application. Words strictly original, however, are probably few. The greater part are derived from others previously existing, which receive such modifications as are sufficient to distinguish the various shades of their application. In coining new words, mankind have always a wish to employ materials to which some sort of meaning was previously attached.

Under the operation of this law, there is one very extensive feature of etymology, which we are obliged to Mr TOOKE for pointing out more fully than was done by his predecessors, and which he seems inclined to regard as universal,—that nouns, or the names of objects, are derived from Verbs. To this we may subjoin, that the verbs from which they are derived signify some sort of action, and, as has been shown, exist in the simplest form in the Imperative.

This position is illustrated in the *Diversions of Purley*, by a very copious induction. Mr TOOKE shows that many English nouns, which we had been in the habit of considering as original words, are derived from verbs. A “passage” receives its name from the verb “pass;” a “gate” from the verb “go;” a “road” from the verb “ride.” “Spot,” “spout,” and “spite,” are from the verb “spit.” “Snout” and “snot” are things “snited.” “Head,” from the verb “heave,” means something “heaved” or elevated above the neighbouring objects to which it is referred. “Heaven,” from the same verb, means the same thing. A “flood” is something that has “flowed.” “Bread” receives its name from the verb to “bray” or
“bruise,”

“bruise,” because it is made of grain which, among other operations, has undergone that of being “brayed.” A “joint” is something “join’d.” The word “gift” is the same with “giv’d” or “given:” “thrift” is from “thrive,” “drift” from “drive,” “weft” from “weave.” The “haft” of a tool is the part “hav’d.” The “hilt” of a sword, the part “held.” A “clock” is an instrument that “clicks.” “Doom” means “deemed,” from the Saxon verb “dæman,” to “deem” or “judge.” “Brood,” “breed,” “brat,” mean something cherished, from “bredan” to “cherish.” “Hand” and “hint” are from “hentan,” to “take hold of.” “Fang” and “finger” from “fingan” to “take.” “Truth” comes from the verb “true,” because it is that which a man “tru-eth” or “believeth.” “Wealth” is that which “weal-eth;” “growth” that which “grow-eth;” “birth” that which “bear-eth;” “warmth,” that which “warm-eth;” “earth” that which a man “ear-eth” or “plougheth.” Nouns derived from Latin verbs are more familiar in their etymology, as, an “act,” “aliquid act-um,” a “debt,” “aliquid debit-um;” “rent,” “aliquid rendit-um;” “tribute,” “aliquid tribut-um;” “expense,” “aliquid expens-um;” “merit,” “aliquid merit-um;” “accident,” “aliquid accent-um;” “fruit,” “aliquid fruit-um;” “fate,” “aliquid fat-um.” Some words of French derivation are obviously formed on the same principle, as “alley,” from the verb “aller” to go; “view,” something seen,” from “vu,” the passive participle of “voir;” “destiny,” “quelque-chosé destinée.”

As Mr TOOKE seems inclined to maintain that *all* nouns are formed from verbs, and as the induction which he has brought forward is more extensive than his readers were prepared to anticipate, some have been struck with an apprehension that this doctrine would prove injurious to some of the most satisfacto-

ry conclusions that prevail among mankind. Its tendency has appeared to be to shake our belief in the existence of a material world. Verbs have been considered as mere expressions of certain temporary modifications to which matter is liable, and therefore a subordinate class of words compared to the names of substantial beings. It has been thought a degradation of the truths received by the testimony of our senses, to suppose that permanent objects should derive their names from words of so fugitive a character as verbs, which represent mere events, or phenomena that disappear the moment after they are presented. This apprehension will vanish if we attend to the distinction betwixt the early history of thought and the early history of language. Our first ideas are acquired, and extensively combined, long before we express them. The first words of which language consists represent, in the form of compendious signs, assemblages of ideas, or features of these assemblages, mutually understood among men. The etymology of each derivative word depends on the motives which direct us to select those definite assemblages to which it will be useful to assign names. Accordingly, we find that the voluntary motions of mankind, which have already been considered as the earliest objects that suggest the use of language, and which are first used in the form of Imperatives, have a predominant influence in regulating the interest which we take in directing the attention of those around us to other objects, and in contriving names to represent them. Let us take for an example the word "road." The surface of the earth presents us with one continued series of colours and tangibilities which we call the qualities of matter. We wish to direct the attention of another to a part of this scene, which has the form of a lengthened stripe, differing somewhat in appearance from the adjoining parts; and we know that the difference which we
observe

observe is produced by the action of travelling, or is made for promoting the convenience of this voluntary motion. The materials of which it is composed do not radically differ from those which compose the parts in its immediate vicinity, and the same sensations are communicated by the ultimate particles of each. But the characteristic mode of assemblage which they exhibit, suggests the idea of the cause which has produced it, or the purpose to which it is subservient. Hence we derive its name from the action of travelling or riding. This is the origin of the word "road." It signifies a place which has been "rode" upon. On the same principle, an uninhabited country is called a "desert," because it is "desert-um," a place forsaken or avoided by men. A cultivated spot is called a "field" or "felde," from the past participle of the verb "fell," intimating that the trees which are supposed to have once covered its surface are cut down. In short, the particular interest which an association with the motions of mankind gives to the assemblages which our words denote fully accounts for the facts adduced by Mr TOOKE in proof of the prevalence of this mode of derivation among Nouns, and at the same time illustrates the various positions which have been advanced in the present paper. This law, while it throws light on some of the operations of the human mind in imposing names on external objects, neither throws any dubiety on the existence of an external world, nor prevents us from admitting that the materials of which it is composed possess an importance independent both of human action and of human thought.

While Mr TOOKE has been blamed for leading his readers to the species of scepticism now alluded to, it has also been his fate to be accused of employing grammatical analysis in the support of a doctrine diametrically opposite, that of Materialism. He has asserted that every word expresses an "object," by
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which he seems to mean an *external* object ; and thus it has been supposed, that no room was left in his theory of Language for expressing the important phenomena of Mind. From this it has appeared to be an inference, though certainly a very illogical one, that mental operations are merely the result of the varied motions of matter. But it is probable that those facts in the formation of languages advanced by Mr Tooke which appear to have this last tendency would, if enquired into, throw a further light on the progress of the human mind as displayed in giving origin to various oral signs, without at all influencing the question of materialism. If a full view of the theory of language were exhibited, these facts would come under consideration, and it would be necessary to attempt an explanation of them. They are not, however, farther connected with the observations contained in this paper than as belonging in general to the theory of language, although now alluded to from being suggested in consequence of the contradictory sentiments which have been ascribed to the author now mentioned.

The author of the present memoir indulges a hope that some valuable instruction will ere long be derived either from the criticisms of the learned on the doctrines which he has here ventured to offer to their attention, or from exertions otherwise made to cultivate a subject which undoubtedly lies open to much improvement.

XIV. *On the Diffusion of Heat at the Surface of the Earth.*
By JOHN MURRAY, M. D. F. R. S. EDIN.

(*Read 2d May 1814.*)

AN argument which I had stated against the Huttonian Theory of the Earth, in so far as it relates to the operation of a central heat, was honoured some time ago with a reply by a distinguished Member of the Society. Respect for the opinion of Mr PLAYFAIR led me to consider attentively the reasoning he employed ; and still feeling some confidence in the grounds on which the original argument rests, I propose to offer a few observations with regard to it. The question farther involves the consideration of the mode in which heat is distributed at the surface of the globe. This, when minutely investigated, presents a very perfect arrangement, by which the escape of caloric is prevented, while its equal distribution is more effectually attained ; and the subject, under this point of view, may have some interest, independent of its relation to any controversial discussion.

The argument which I had advanced is, That if a heat exist in the interior part of the earth, operating for an indefinite period, as is assumed in the Huttonian Theory, it cannot for such a period remain locally accumulated. It must diffuse itself
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through the entire mass, and become at length equal, or nearly so, over the whole. An equilibrium of temperature must therefore be established, incompatible with that system of indefinitely renewed operations which is represented as the great excellence of the system. This has always appeared to me conclusive; and an argument such as this, derived *à priori*, and directed to the first principles of a geological theory, if successful, is of greater weight than arguments derived from its adaptation to natural phenomena, which, even when they appear to be just, amount only to probability, and, from our imperfect knowledge of the relations of the mineral kingdom, leave often some degree of uncertainty*.

To obviate this argument, the following reasoning has been employed by Mr PLAYFAIR. The diffusion of temperature, he remarks, is a consequence of the tendency of heat to pass from bodies where the temperature is higher to those where it is lower. It is not, therefore, a necessary result, but is only contingent,

* I ought to remark, that this view of the constant existence of a central heat is not considered by all the defenders of Dr HUTTON's system, as a necessary part of it, nor do they even regard it as a position which he himself maintained. They suppose the existence only of interior local heat, which may cease for a time, and be again renewed; and to this hypothesis the above argument does not so strictly apply. I must only add, that if even this view of the subject be adopted, much of the difficulty will still remain in its original force; it becomes liable, too, if I mistake not, to other objections peculiar to itself, equally important; and by adopting it, much of the beauty and unity of the system are lost. These seem to me to require the assumption of a *central heat*, or general reservoir of heat capable of extending its action to every part of the circumference, always existing, though not equally active in its *apparent* effects. It is to this view of the subject, consistent, I believe, with the original statement of the theory by Dr HUTTON, that the argument applies. It is this which Mr PLAYFAIR admits, and on the admission of which, indeed, his reasoning is founded; and, strictly speaking, it is to *his* reasoning only that the observations in this paper are directed.

tingent, requiring the presence of another condition, which may be wanting, and actually is wanting, in many instances;—this is, that the quantity of heat in the system should be given, and should not admit of continual increase from one quarter, nor diminution from another. When such increase and diminution take place, no such equilibrium can be attained. In proof of this, he mentions the fact, that a bar of iron thrust into the fire, though red-hot at one extremity, will not become so at the other in any length of time, but each part of it will have a fixed temperature, lower as it is farther from the fire, but remaining invariable while the condition of the fire, and of the surrounding medium, continues the same. He illustrates it also more fully by the following example: Let A, B, C, D, &c. be a series of contiguous bodies, or let them be parts of the same body; and let us suppose that A receives from some cause, into the nature of which we are not here to inquire, a constant and uniform supply of heat. It is plain that heat will flow continually from A to B, from B to C, &c.; and in order that this may take place, A must be hotter than B, B than C, and so on; so that no uniform distribution of heat can ever take place. The state to which the system will tend, and at which, after a certain time, it must arrive, is one in which the momentary increase of the heat of each body is just equal to its momentary decrease, so that the temperature of each individual body becomes fixed, all these temperatures together forming a series decreasing from A downwards. This is then applied to the argument in the following manner: “If heat be communicated to a solid mass, like the Earth, from some source or reservoir in its interior, it must go off from the centre on all sides towards the circumference. On arriving at the circumference, if it were hindered from proceeding farther, and if space or vacuity presented to heat an impenetrable bar-

rier, then an accumulation of it at the surface, and at last a uniform distribution of it through the whole mass, would inevitably be the consequence. But if heat may be lost and dissipated in the boundless fields of vacuity, or of ether, which surround the earth, no such equilibrium can be established. The temperature of the earth will then continue to augment, only till the heat which issues from it every moment into the surrounding medium, become equal to the increase which it receives every moment from the supposed central reservoir. When this happens, the temperature at the superficies can undergo no farther change, and a similar effect must take place with respect to every one of the spherical and concentric strata into which we may conceive the solid mass of the globe to be divided. Each of these must in time come to a temperature at which it will give out as much heat to the contiguous stratum on the outside, as it receives from the contiguous stratum on the inside, and when this happens, its temperature will remain invariable.*,"

The principle on which this reasoning rests will not be disputed. Admitting it therefore, my objects in attempting to support the original argument will be to shew, *first*, That such a discharge of heat from the surface of the earth as is here supposed does not take place; and, *secondly*, That if it did, this would be as subversive of the system, as if the heat were retained.

There are two modes in which caloric may be supposed to be conveyed from the surface of the earth; one is by radiation, the other by slow communication by the medium of the elastic fluid which surrounds it. Each of these may be briefly considered.

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* *Transactions of the Society*, vol. vi. p. 356.

The great expansibility and mobility of an elastic fluid, such as the atmosphere, give rise to a peculiarity with regard to the communication of temperature through it. Its proper conducting power appears to be inconsiderable, but, whatever it may be, it is principally by the motion of its parts that heat is diffused through its mass. When the temperature of a lower portion of such a fluid is raised it ascends rapidly; a fresh portion comes in contact with the surface communicating heat; and by this successive application and retrocession of the air, and this movement of the heated portion, rather than by the direct communication of caloric from one part to another, the temperature of the whole is raised. It is in this manner that heat is diffused from the earth into the atmosphere. The air incumbent on any part of the surface communicating heat, is, by the elevation of its temperature, rarified, whence an ascending current is formed; and the common opinion with regard to this is, that it carries the excess of heat to the higher regions of the atmosphere, and may allow it to be diffused into the interplanetary space. It is on the assumption of this that the heat must be supposed, in Mr PLAYFAIR's argument, to be discharged from the earth, so far at least as it is conveyed from the surface by the surrounding elastic medium. The just view of the operation of the atmosphere in diffusing heat, is however more complicated; other conditions connected with it are to be taken into account, and lead, I believe, to a very different result.

The air heated at any part of the earth's surface, may, as it ascends, impart, especially at first, a portion of heat to the air with which it comes in contact. But as it rises, becoming subject to less pressure, it expands; by this its capacity for heat is augmented, and its temperature, therefore, falls proportionally. As it recedes from the surface, with its temperature thus con-

stantly falling, from its increasing capacity, its tendency to part with heat is always becoming less; and as it must continue to rise in the atmosphere until it attain an equilibrium of temperature with the air around, any tendency to communicate heat to that air must at length cease. If it retained its high temperature, or if it were to lose this gradually only from the communication of its heat, it might be conceived to convey caloric onwards; but its capacity for caloric increasing from its rarefaction as it ascends, it is enabled to retain the excess of heat it had received, without having a corresponding elevation of temperature. It is only when it returns towards the surface, in consequence of that circulation which the constant ascent of portions of heated air establishes in the atmosphere, that this is evolved. As it descends, it becomes progressively subject to greater pressure, its capacity gradually diminishes; and continuing to do so as it falls, it gives out in the same gradual manner the excess of heat which it contains. Thus, for every portion of heat conveyed by the ascent of a stratum of heated air from any part of the circumference of the globe, a corresponding portion of heat is given out by a descending stratum at some other part; and as this communication of heat from the atmosphere will happen principally at the colder parts of the earth's surface, both as the descent of the air will be there greatest, and the disposition to receive heat also greatest, the whole forms an admirable arrangement to counteract local inequalities of temperature, to diffuse heat equally over the globe, and to prevent any dissipation of it beyond the sphere of the atmosphere.

It is thus, I conceive, demonstrated, that by the principal mode in which heat is propagated through the atmosphere,—that, by the motion of the heated portion of air, it can only be withdrawn to comparatively a short distance from the surface.

face of the earth, and that there is a constant return of it. By direct communication its conveyance must be equally limited; the conducting power of an aeriform body is extremely imperfect, and there is every reason to believe becomes less as the fluid becomes more rare; and in the higher regions of the atmosphere, the subtilty of the medium is so great, that beyond a certain height it cannot be supposed to be the vehicle of the conveyance of heat, far less that it can convey it into the boundless fields of vacuity.

But, farther, were the conducting power of the most perfect kind, no effect could arise from it in the conveyance of caloric beyond a certain height. The communication of heat from one part of a mass of matter to another, or from one body to another, depends, as Mr PLAYFAIR justly observes, on difference of temperature, and if there is no difference, there will be no such communication. Now, a little reflection will shew, that this completely limits the diffusion of heat through an elastic fluid, receiving it under such conditions as our atmosphere. The air heated at the surface expanding as it rises, but at the same time, from this expansion falling in temperature, must come at length to be in equilibrium, both in density and in temperature, with air at a certain height; its farther ascent will then cease, and its temperature being the same with that of the air around it, it will yield none of its heat. It is only in its descent, as a fresh portion of air rises, that it will evolve caloric, and it will continue uniformly to do so, as it returns to the surface of the earth.

We thus trace a curious provision to prevent any discharge of heat by the atmosphere into the interplanetary space; the excess at particular regions is only withdrawn, is more equally distributed over the surface, but is ultimately communicated to the earth; and there is no other apparent arrangement by which

which this could have been attained, than by an atmosphere constituted as ours is,—expanding as it is heated, and falling in temperature from augmentation of capacity as it expands. To admit of an unlimited conveyance of heat, a solid mass, however subtle, would have been required, or an atmosphere the capacity of which for caloric should not increase with its rarefaction.

The passage of heat is thus from the circumference towards the centre of the globe, instead of the opposite direction, and it is prevented from accumulating at the surface only by being conveyed into the mass of earth.

These conclusions may be applied to the present argument. If the view I have stated be just, we have demonstration that the atmosphere conveys no heat into unlimited space; our planet, in relation to the discharge of caloric from it, is bounded as it were by a wall of non-conducting matter, and no arrangement can be imagined more perfect, by which the heat belonging to it could be confined. If the heat from the centre, Mr PLAYFAIR observes, on arriving at the circumference, were prevented from proceeding farther, and if space or vacuity presented to heat an impenetrable barrier, then an accumulation of it at the surface, and at last a uniform distribution of it through the whole mass, would inevitably be the consequence. It appears that the atmosphere does present such a barrier, and the consequence, therefore, necessarily follows*.

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* If the sphere from the centre of which heat is supposed to be diffused, be not exposed to an unequal external source of heat, the heat will be communicated equally at its circumference to the incumbent atmosphere, and produce ascent and descent of the air, with little of that circulation of it from one part to another, which is the consequence of inequality of temperature at the surface of the solid. But still the same changes of capacity for heat will accompany the
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This is true, at least, so far as relates to any power of the atmosphere to convey heat. There is still to be considered, however, another mode in which caloric may be supposed to be discharged from our planet,—that by radiation. Rays possessed of heating power are thrown off from a body at a high temperature, and by the discharge of these its temperature is reduced. But various considerations shew, that this would be a very inadequate source of the escape of heat from the interior part of the earth.

Thus the radiation of caloric is inconsiderable, except from a body which is heated; and the quantity radiated increases as the temperature rises in a much higher ratio than the increase of temperature itself. At low temperatures, therefore, it must be extremely small. At 100° it is scarcely apparent, from experiment; and at 50° is not sensible. Not only, too, does the quantity diminish rapidly with the temperature, but the projectile force of the rays emitted becomes less, so that those which are discharged at low temperatures, are incapable of penetrating *media* such as glass, which those at high temperatures penetrate with facility. And as even the latter are, to a certain extent, intercepted by the atmospheric air, the former must be more completely arrested. At 50° , therefore, the medium temperature of the globe, and which prevails over so large a portion of its surface, we have no reason to believe that a discharge of caloric by radiation, into remote space, can take place to any extent.

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ascent and descent of the aerial mass, and the principal effect will be, to accumulate temperature at the surface. The condition is one, however, which need not be taken into account, as it is one which has never existed with regard to our planet; and the argument remains as is stated above, while there is inequality in the distribution of heat over the circumference of the globe.

At the parts of the surface of the earth which are at still lower temperatures, and at those where intense cold prevails, the supposition of any radiation is of course still more precluded, and the negative even may be proved. When a solid body, and the surrounding elastic medium, are at the same temperature, there seems to be no discharge of radiant heat from the former; and still more when the temperature of the solid is lower than that of the elastic medium, (and this is usually the case in colder regions), it is rather disposed to absorb than to emit caloric by radiation. The resistance, too, opposed by the atmosphere to the discharge of rays having the weak projectile force which those emitted at such a low temperature must have, would in a great measure prevent their escape, if the power to radiate did actually exist.

At the hotter parts of the earth's surface there may be some emission of caloric by radiation, but it is not difficult to shew, that the quantity of this cannot be equal to the quantity communicated by the solar rays; for of the heat derived from the latter source, a portion is absorbed by the earth, and conveyed to the interior, as is apparent from the decreasing temperature, as we recede from the surface to a certain depth; and another portion is carried off by the ascending current of heated air, and conveyed to colder regions, where it is also absorbed.

Thus, even from those parts of the surface of the earth where the circumstances are those most favourable to radiation, the quantity radiated cannot be equal to the quantity received by the solar rays. Over the whole surface, the difference must be still greater, and instead of the conclusion, that this planet discharges an excess of heat by radiation, there is every reason to draw the opposite conclusion,—that part of the heat which it receives from the sun is retained.

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The only reason, at least, that would lead to hesitation in admitting this last inference, is a hypothetical one,—that it is inconsistent with that perfect arrangement, which we are disposed to believe exists among all the heavenly bodies, whence the stability of the entire system is maintained, and whence, therefore, in relation to any communication of caloric among them, the quantity radiated by each will, on the whole, be equal to what it receives. This, whether we restrict the view to the different parts of our solar system, or extend it even to the universe, is no doubt the conclusion which the imagination is naturally disposed to embrace, though it may not be supported on any decisive evidence, or may even be in some measure apparently in opposition to the fact. But to suppose that this planet discharges more caloric than it receives, would be not only in opposition to evidence, but in opposition to the very principle which alone leads to the admission that the quantity discharged may be equal to the quantity received.

Concluding, then, as it appears to me, may be strictly inferred from the preceding reasoning, that our earth does not discharge caloric from its surface into the regions of space, or at least does not discharge more than it receives from the other heavenly bodies, if it discharge even this, the argument recurs, I believe, in its original form, as sufficiently established, That if an interior heat exist, it must be diffused through the substance of the earth, and an equilibrium of temperature be at length established, incompatible with those operations ascribed to its action in the Huttonian Theory.

But farther, though the heat were not retained,—though it were granted that it is propagated into the regions of space, this does not render the argument I have urged less conclusive. The strength of that argument lies in the circumstance, of the diffusion of heat from the interior, not in the accu-

mulation of that heat at the surface. The latter is a contingent event, which may or may not happen, without being of essential importance in the conclusion to be drawn. The former is a necessary result, which must prove subversive of the arrangement, the assumption of which constitutes the leading principle of Dr HUTTON's System.

Mr PLAYFAIR, accordingly, in the general case which he takes for the illustration of his argument, assumes *a constant supply of heat at the centre, as well as a constant escape of it from the circumference*. But in applying the argument, where is the proof of such a supply of heat at the centre of the earth? It can scarcely be necessary to remark, that none is attempted to be given. And were it allowable to make an assumption, without evidence, merely to obviate an argument, the force of which cannot otherwise be avoided, no hypothesis, even the most extravagant, could ever be overturned. Heat cannot originate in nothing; and if we assume its constant discharge from the circumference of this planet, what cause is it possible to assign for its constant supply from the interior? Admit even its existence in any degree of intensity, still it is obvious, that it must be in limited quantity. If we speak of a *spring* of heat, as conveying the idea of an unlimited supply, we deceive ourselves by the use of a term to which no definite signification can be affixed, but what is subversive of the reasoning it is designed to support. A source or spring means nothing more than a hidden reservoir, connected with external supplies. There can be no reservoir of heat in the centre of the earth, which, without being recruited by constant supply, shall continue to furnish it, to be discharged from the circumference through indefinite time into unlimited space. And it is not possible to imagine any circulation by which it shall be restored.

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It is, therefore, I conceive, of little importance in the discussion, whether the heat supposed to be conveyed from the centre to the circumference is accumulated there, or is discharged into the regions of space. Its propagation to the surface must be subversive of its accumulation in the interior. Either the diffusion alone, therefore, or the diffusion attended with the final result of equilibrium of temperature, is sufficient for the refutation of the hypothesis. Both I conceive are established. And I would still regard the argument in the light in which I first advanced it, as a demonstration of the fallacy of the leading principle of the Huttonian System, which assumes the existence of an internal heat operating at renewed periods for indefinite time.

There are some other points of view under which the subject may be considered, in which the difficulties attending the assumption of an internal heat, when connected with the law which Mr PLAYFAIR has illustrated, appear in a very strong light.

It has always been found difficult to give any account of the origin of the supposed internal heat, to the operation of which the consolidation of minerals, and the elevation of the habitable land from the bed of the ocean, have been ascribed. If the view be restricted to the production even of one world, such an intensity and continuance of heat are necessary for this single effect, that no adequate cause can be even hypothetically assigned for it. If it be extended to the successive production of three or four worlds, each embracing a period between its formation and destruction in which millions of years must elapse,—an event which, according to the theory, is not only possible, but has actually occurred,—the heat required is altogether beyond what the imagination can conceive. But if in addition to this, caloric is also diffused from the interior,

and discharged from the circumference of the globe into unlimited space, we are lost in the magnitude of the result, and are unable to acquire a just conception of the force of the argument, from the impossibility of contemplating clearly the difficulty in all its extent.

The difficulty, from the intensity of the heat which must be assumed to exist, is not less great than that from its continuance and waste. It is sufficiently apparent, when we consider that the highest mountains of the globe run in extensive chains, and are so connected, that they must have been formed at one time, and that they are composed of materials which a very intense heat does not fuse. But this is nothing compared with the statement which must be made, in consequence of the law, that the internal temperature is a *decreasing* one from the interior to the circumference of the globe.

If we can discover the rate of this decrease, by knowing the temperature which exists at two distant points, we may of course form some calculation of the intensity of the heat which exists at the commencement of the series. Now this we have the means of determining with considerable precision. At the bottom of the sea, or within a short distance from it, the heat from the interior must be at a degree of intensity sufficient to produce mineral fusion and consolidation from the disintegrated materials of a former land, which may be estimated from our knowledge of the fusibility of these bodies. It is propagated from this onwards, with such a decrease that at the surface, there is no sensible high temperature. Its diffusion from the central regions to the bottom of the sea must of course have been at a similar rate of diminution. If we were to calculate the rate of progression, and compare it with the distances in the two portions of space,—that from the central region to the bottom of the sea, and that from the bottom of the sea to the surface

surface of the land, we shall find an intensity of heat in the interior, compared with which the heat necessary to melt mountains of quartz, formerly supposed to present so great a difficulty, is a mere atom in the scale, scarcely affording even a point of comparison.

Some idea may be formed of this, by recurring to the illustration of the iron bar, with a decreasing temperature, making the most liberal allowance in favour of the Huttonian hypothesis, with regard to the respective portions of space. Thus the bar, being one thousand inches in length, if its temperature at the one extremity be 50° , and if within five inches of this it is at a white heat, then the heat increasing at the same rate, through every succeeding five inches, what must be its intensity at the other extremity? No effort of the imagination can form the most remote conception of it, nor can any argument be wanting to prove, that no such heat can exist in the interior of the earth.

If to avoid the difficulty, a less rapid decrement of temperature be supposed, then, from a heat of that intensity which must be assumed to exist at the bottom of the ocean, to produce the effects ascribed to it, the decrease in the short space between that and the surface cannot be such as to bring the temperature within that which is at all compatible with the established economy of nature. The difficulty is, therefore, insurmountable; it must occur on the one hand or on the other; and it is not merely connected with Mr PLAYFAIR'S argument, but as that argument is founded on a law perfectly just with regard to the diffusion of temperature, it is a difficulty which necessarily follows from the assumption of a central heat, or of any internal heat such as that which must be assumed in the Huttonian Theory,—a heat which is to operate around the whole circumference of the globe, continue its operations for such immense periods, and renew it for indefinite time.

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Leaving the consideration of this subject, in so far as it is connected with the argument on the Huttonian Theory, I may add a few observations of a more general nature, which the preceding reasoning suggests with regard to the temperature of the globe, and its relation to solar heat. From the nature of the agency of the atmosphere in diffusing heat, the conclusions seem to me almost necessarily to follow,—that there is a tendency to equalization of temperature over the whole surface of the earth,—that this continues to operate in such a manner, that in the progress of time the difference at different parts must become less than what existed at a preceding period; and that, ultimately, a temperature nearly uniform shall be established over the whole.

At the hotter parts of the earth's surface, the temperature cannot increase, or must increase very slowly, and to no great extent; for if it were to rise higher, the ascent of heated air from it, and the transfer of this to the colder parts of the surface, would only become more rapid. But the temperature at the colder regions may rise higher; for the direct ascent of heated air is there less abundant, and what recedes in a lateral direction, does so, deprived of caloric, which it has yielded to the earth. Whatever discharge of caloric, too, may take place by radiation, must be principally from the hotter parts of the surface; from the colder it must be much less considerable, for the quantity radiated is less as the temperature is low: it increases, too, at a higher ratio than the increase of temperature; and hence, if increased radiation from increased temperature, did take place at both, being greater at the former than
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at the latter, its effects would be more considerable in retarding a farther augmentation of temperature. Thus it appears, that the causes counteracting rise of temperature at the surface of the globe, act most powerfully at those parts where it is high, and any progressive rise, therefore, must be principally at those where it is low.

The effect of this arrangement may be most obvious, perhaps, from comparing the two extremes. At the equator, the ascending current of warm air, carrying off heat, is constant or nearly so; towards the poles, the descending current imparting heat, must be equally constant. The heat directly communicated at the former by the solar rays, and the heat communicated at the latter by the current of warm air, will both be in part conveyed through the solid mass towards the interior; but this will always be with a decreasing temperature as it proceeds,—that is to say, the first or exterior layer will be at a higher temperature than the second, the second than the third, and the accumulation, therefore, will be at the surface, to a certain extent. At the poles this may continue to proceed, because heat is there communicated without an equal abstraction. But at the equator it will remain stationary, or nearly so, as no increase can take place without the abstraction both by the atmosphere and by radiation becoming proportionally greater.

This is aided by another effect, that with regard to the diffusion of heat through the solid mass itself. This diffusion from the surface proceeds in part towards the centre, or each layer, as the surface is receded from, receives a portion of heat from the exterior one, and this continues onwards, though with a gradual diminution. But there must be at the same time a diffusion more nearly horizontal, proceeding through these
layers

layers from the equator towards the poles, while there can be none in the opposite direction to counteract this effect.

The ocean, too, serves to convey a considerable portion of heat from the warmer to the colder regions of the earth, by the movement of currents, somewhat similar to those which exist in the atmosphere, and the course of which it is even possible to trace.

Thus, by these various arrangements, whatever excess of heat may be received by this planet from the sun, and retained at the surface, will be permanently accumulated towards the poles, and the temperature there will increase. In all the portions of the intermediate space, between the poles and the equator, the same law will operate, though with decreasing force; and over the whole surface, there is a tendency to equality of temperature, which, however slow the progression to it may be, must, as the result of general causes constant in their operation, be finally established.

The speculations, then, on which some have dwelt,—that the northern parts of our globe have suffered a gradual refrigeration, and which BAILLY, in particular, applied to the fanciful system which he supported with so much ingenuity,—that civilization and science have descended from the elevated regions of the north of Asia,—have probably no foundation. It is always interesting to trace the succession of opinions which mark the progress of knowledge, and to observe how far what at one period is considered as established, is at another rendered doubtful, or proved to be false. The refrigeration of the globe from the loss of its interior heat, is a fact, says the author just referred to, of which there can be no doubt; and this refrigeration, he adds, must have been principally towards the poles, partly from the flattening of the sphere there, in consequence of which the heat from the centre must escape
sooner

sooner, but still more from the unequal action of the solar rays, which is more intense towards the equator. Hence the countries round the poles must have been the first that were habitable, and this gradual cooling has caused the same temperature to proceed successively over all the regions of the globe*. Not only are these conclusions unfounded, but the changes which have occurred are probably the very reverse. No heat can escape from this planet, but in consequence of a high temperature being kept up at its surface by the communication of heat from an external source. Until this be attained, it must retain the heat it receives from the sun; and this must accumulate principally towards the poles.

That the temperature at the surface has risen above the original temperature of the earth, may be inferred not only from the consideration, that solar heat has been communicated to it, which could not be discharged until a certain elevated temperature were attained, but also from this, that there is no natural operation actually generating cold; there is only the production of heat; and cold prevails where this is less powerful, or is counteracted. To account, therefore, for the low temperature at the colder regions of the earth, only two suppositions can be advanced. Either the original temperature must have been as low as this, or lower, and have been raised higher, where it is actually superior, by the reception of the sun's rays: Or it must be assumed, with BUFFON and BAILLY, that heat can escape from this planet to an indefinite extent, and that it is in a state of progressive refrigeration, the effects of which are prevented from being apparent at the equatorial regions by the direct communication of solar heat, but towards the polar circles are evident, as not being counteracted by the same

VOL. VII. P. II. 3 H

* *Lettres sur l'Origine des Sciences*, p. 329.

cause. This assumption, however, is precluded by the proof, that heat is not capable of being carried off by the atmosphere, but can escape only by radiation, which is dependent on an elevated temperature. The conclusions seem, therefore, necessarily to follow, that the original temperature of the earth must have been at least as low as the lowest natural temperature, and of course that the mean temperature has been raised.

While there is this rise, and this equalization of temperature at the surface, there must be a corresponding rise in the internal mass. It is obvious that no permanent rise can take place at the external layer, without a portion of heat being diffused to the internal matter at a lower temperature. It is obvious also, that a portion of the heat received necessarily must be conveyed to that matter. And as this diffusion is limited at the centre, the temperature must rise until it become equal or nearly so through the whole, and as high, or nearly as high, as that which the solar rays can excite.

Lastly, the rise of temperature must observe certain limits; it must continue until that of the surface is such, that as much caloric is discharged from it by radiation, as is received by the solar rays, and it cannot proceed beyond this. The first conclusion necessarily follows, when it is proved, that radiation is the only mode by which caloric can escape from this planet. The second conclusion is equally evident. Thus the tendency is to an equal and uniform temperature. How far this may be from that which at present exists, it is difficult to determine. If the increase of heat is accompanied with its more equal diffusion, so as to establish nearly the same temperature over the entire surface, it may not proceed much beyond that which now prevails at the hotter parts of the earth; for at this, when extended over the whole, the quantity of caloric radiated may

may be equal to that received. And even if it were to rise higher than this, still, from gradual changes in the laws of organised matter, or in the species of living beings, not greater than what seem to have occurred in the past revolutions of the globe, the existing temperature might be sufficiently compatible with the continuance of animated existence, and with an order not very different even from that which now prevails.

Different views have been presented of the relation of the temperature of the globe to solar heat. Some have imagined, that the earth is in a state of progressive refrigeration; and while it was believed that the atmosphere could convey heat onwards without limitation, this conclusion might be drawn. Others, from considering merely the constant communication of solar heat, have drawn the opposite conclusion, that its temperature must increase, and this indefinitely. While a more probable opinion than either, founded on the apparent uniformity of natural operations in those past periods to which any records reach, is that, the temperature has always been the same, or nearly the same, as that which now prevails. The view which follows from the preceding observations, is different from all these, and presents a more perfect arrangement. The temperature of the globe must, from the mode in which heat is communicated to it, rise, and at the same time, as it advances, must become more equal over the whole surface. And this rise has its limits; there cannot be either unlimited increase of heat, or indefinite refrigeration; but the final result will be a state of permanence and uniformity, the continuance of which is secured by the very circumstance, that if it is deviated from, the deviation must check itself.

No view has been presented in physical science of equal grandeur with that established from astronomical observations,

that amid all the revolutions of the heavenly bodies, an order exists whence the irregularities arising from their mutual actions do not increase, but are so adjusted, that when they reach certain limits they recede, and within this state of oscillation the stability of the entire system is for ever secured. It would gratify the mind, could it be shewn, that a similar system exists with regard to the subordinate parts; or if not, that a state of permanence in these parts will be ultimately established, compatible with the operation of that more general law under which the order of the universe is maintained. In the structure of the globe, however, and in the operations to which it is subject, there are evident causes of disintegration, which seem incompatible with a state of permanence, and from which, in the progress of time, those arrangements which constitute it a habitable world must apparently be subverted. For the Huttonian Theory has been claimed the praise, (with what justice need not here be inquired), of unfolding a system of renovation corresponding to this waste. In the opposite Theory, no similar attempt has been made; its object has been merely to trace the arrangements which exist in the mineral kingdom, and from these to infer the order and mode in which they have been framed; nor have any causes been pointed out, as, indeed, none seem to follow from the principles of the theory, by which that disintegration, the occurrence of which in past periods is so clearly marked, and the operation of which, even at present, is to be traced, may be repaired.

If the view, however, which has been given of the relation of the temperature of the earth to solar heat be just, this deficiency may perhaps be supplied. Inequality of temperature is the great source of change and of disintegration at the surface; the expansion and contraction from alternations of heat
and

and cold, the absorption and expulsion of humidity from the same causes, the distending force of congelation, and the rapid precipitation and flow of water, which are the principal, or rather the sole disintegrating processes of general or uniform operation, being the results of it. When inequality of temperature, therefore, shall cease, or be restrained within much narrower limits, an order may be established of less vicissitude, and less waste, than that which now prevails, and the stability of which may even remain unimpaired for indefinite time.

This view, if it is not carrying the speculation too far, may even be extended to all the parts of our solar system, and the condition of each planet may be connected in permanence with that law which appears to regulate the constitution of the universe. Considering this Earth as passing through a series of revolutions from its chaotic state to a more permanent and perfect form, the different planets may be regarded as in a similar progression. Astronomers have often traced the analogy which exists among them, not only in the laws of their motions, but in the figure of their masses; they have not failed to remark the flattening at the poles, which can be observed in some of them, similar to that of our globe, proving a similar state of fluidity from which this figure has originated; and they have endeavoured even to draw from the aspect which they exhibit, indications of the stage of progression in which they now are*. But they have presented no pleasing prospect of the final adjustment of this series of revolutions. Regarding the planets as extinct suns, or fragments of suns, or at least as masses which have been hot and luminous, they have supposed them to be in a state of gradual refrigeration, which will terminate

* *Histoire de l'Astronomie Moderne*, tom. ii. p. 726.

terminate in the total cessation of movement and animated existence. The assumption on which this gloomy hypothesis is built,—that of the unlimited escape of heat from each planetary mass, is fortunately as false, as the view to which it leads is unworthy of the order and magnificence which the system of nature displays; and instead of this termination, in what one of these philosophers emphatically calls the state of Ice and Death, of silence and repose, we may with more confidence look to the equal diffusion of heat through the mass of each planet, as the state of permanence under which it will exist, and to the equal interchange of heat among all, as the perfection of the system they form.

XV. *On a New Species of Coloured Fringes, produced by the Reflexion of Light between Two Plates of parallel Glass of equal thickness.* By DAVID BREWSTER, LL. D. F. R. S. EDIN. & F. A. S. E.

(Read February 20. 1815.)

DURING a series of experiments in which I was lately engaged, for the purpose of determining the law of the polarisation of light, by successive reflexions from plates of parallel glass, I observed that all the images of the luminous body which were formed by more than one reflexion, were crossed by parallel fringes of coloured light, when the two plates had a small inclination to each other; and that these fringes suffered considerable changes, by varying the position of the plate with regard to the incident ray.

These coloured fringes seemed at first to have the same origin as those of thick plates, which were discovered by NEWTON, and afterwards examined by the Duke de CHAULNES, Mr BROUGHAM, and Mr JORDAN; and I considered the second plate of glass as performing the part of the quicksilver in NEWTON's glass mirror, or of the metallic speculum in the experiments of the Duke De CHAULNES, and Mr BROUGHAM. A more attentive examination, however, convinced me that this

was

was a mistake, and that the coloured fringes constituted a new class of phenomena, having a different origin from those of thick plates, though explicable by the beautiful theory of Frits of easy reflexion and transmission, by which NEWTON was enabled to explain all the phenomena of the colours of thick and thin plates.

In order to observe the phenomenon to the greatest advantage, let the light of a circular image subtending an angle of 1° or 2° , be incident perpendicularly, or nearly so, upon two plates of parallel glass, placed at the distance of one-tenth of an inch, and let one of the plates be gently inclined to the other, till one or more of the reflected images be distinctly separated from the bright image formed by transmitted light, and received upon the eye, placed behind the plates. Under these circumstances, the reflected image will be crossed with about fifteen or sixteen beautiful parallel fringes: The three central fringes consist of blackish and whitish stripes, and the exterior ones of brilliant stripes of red and green light; and the central fringes have the same appearance in relation to the external fringes, as the internal have to the external rings, formed either by thin plates, or by the action of topaz upon polarised light. If the two plates of glass are turned round in a plane at right angles to the incident ray, the reflected images will move round the bright image, and the parallel fringes will always preserve a direction at right angles to a line joining the centres of the bright and reflected images. Hence it follows, *that the direction of the fringes is always parallel to the common section of the four reflecting surfaces, which exercise an action upon the incident light.*

The position of the plates remaining as before, let the inclination of the plates, or what is the same thing, the distance of the

the

the bright and the reflected image be varied by a gentle motion of one of the plates, the coloured fringes will be found to increase in breadth as the inclination of the plates is diminished, and to diminish as the inclination of the plates is increased.

In order to determine the law according to which the magnitude of the fringes varies, I employed two plates of parallel glass $\frac{1}{1000}$ ths of an inch thick, and obtained the following measures for the fringes which crossed the image that had suffered two reflexions between the plates. The pencil of light was incident nearly in a vertical direction upon the first plate.

Inclination of the Plates.	Angular breadth of each Fringe.
1° 11'	26' 50"
2 20	13 3
5 36	5 41

Now since $5^{\circ} 36' : 26' 50'' :: 1^{\circ} 11' : 5' 40''$, and since

$5 36 : 13 3 :: 2 20 : 5 27$, it follows, that

the breadth of the fringes is inversely as the inclination of the plates.

Owing to the rapid diminution of the fringes, by increasing the angle formed by the plates, I could not with any degree of accuracy determine their breadth at greater angles of inclination; and therefore it still remains to be ascertained whether it varies with the sine, tangent, or secant of the angles.

If the light of the circular object, instead of falling perpendicularly upon the plates, is incident at different obliquities, so that the plane of incidence is *at right angles to the common section of the plates*, no fringes are visible across any of the images. But if the plane of incidence is *parallel to the common section of the plates*, the reflected images increase in brightness with the obliquity of incidence, and the coloured fringes become more vivid. When the angle of incidence increases from 0° to 90° , the images that have suffered the greatest number

ber of reflexions, are crossed by other fringes, inclined to them at a small angle. At an angle of about 44° , the image formed by four reflexions, is covered with interfering fringes, but it is not till the angle of incidence is greater, that this irregularity is distinctly seen on the image formed by two reflexions.

Hitherto I had observed no fringes upon the first or bright image, which is obviously composed of light that has not suffered reflection from the second plate of glass. By concealing, however, the bright light of the first image, so as to perceive the image formed by a second reflection, within the first plate, and by viewing this image through a small aperture, which I found of the greatest service in giving distinctness to all the phenomena, I observed fringes across the first image, far surpassing in precision of outline, and in richness of colouring, every analogous phenomenon which I had seen. When these fringes were concealed, I also observed other fringes on the image immediately behind them, and formed by a third reflexion, from the interior of the first plate. I now concealed the second image, upon which the fringes were extremely bright, and very faint stripes were seen upon the one immediately behind it.

In examining these phenomena a little more attentively, I observed that the size of the fringes in the first image, varied with the distance of the eye from the plates, while those on the second and fourth image diminished with that distance.

The magnitude of this change will be understood from the following experiments:

Angles of Incidence.	Number of Fringes across the first Image.		Number of Fringes across second image.	
	Eye near.	Eye a few inches distant.	Eye near.	Eye a few inches distant.
0°			6	$6\frac{1}{3}$
47	$4\frac{1}{3}$	$3\frac{1}{2}$	5	$5\frac{1}{3}$
61	$3\frac{1}{2}$	3	4	$4\frac{1}{3}$
73	$2\frac{1}{3}$	$2\frac{1}{2}$	3	$3\frac{1}{3}$

When

When the fringes on the second image were inclined to the right, those on the first image were inclined to the left; so that both in point of position and magnitude, the two sets of fringes follow a different law.

The preceding measures of the magnitude of the fringes at different obliquities, were not taken with that accuracy which is necessary for determining the law of their variation. I have made numerous experiments for this purpose; but when the angle of incidence is considerable, there is always such a degree of distortion in the fringes, and such a perceptible variation in their magnitude, from the slightest change in the position of the eye, that I found it quite impracticable to take measures in which any confidence could be placed. This difficulty no doubt arises from the imperfect flatness of the surfaces of the plates of glass; and I fear that even our best artists are not capable of producing better plates than those which I used in the preceding experiments. The following measures may be considered as tolerably correct.

The inclination of the plates was not the same as in the preceding experiments:

Angles of Incidence.	Number of Fringes across the first Image.	Number of Fringes across the second Image.
0° 0'		$6\frac{1}{3}$
36 56	5	$5\frac{1}{4}$
58 48	$3\frac{1}{3}$	$4\frac{1}{6}$
62 52	$4\frac{1}{6}$	$3\frac{2}{3}$
71 30	$2\frac{1}{3}$	$2\frac{1}{2}$

If the two parallel plates are placed at *any distance whatever*, and the preceding experiments repeated, the fringes will be found to suffer no change either in their magnitude or direction.

I now took three plates of parallel glass, that gave the coloured fringes when any two of them were put into the proper position. When the third plate was placed either before the other two, or between them, or behind them, it did not in the least degree affect the fringes which they produced. When it was placed, however, in such a position as to form a new reflected image, this image was also crossed by the coloured fringes.

When the *third* piece of parallel glass was cemented with Canada balsam upon the face of the *first* plate, or upon the back of the *second* plate, the fringes disappeared. When the interval between the two plates was filled with water, or with Canada balsam, the fringes were very faint, though distinctly perceptible. Hence it follows, that *the production of the fringes depends upon the action of all the four surfaces of the two plates of parallel glass.*

All the preceding experiments were made with plates which were cut out of the same piece of glass, and had therefore the same thickness. I now tried plates of different thicknesses, both when ground parallel, and when cut from common plate glass; but I could never render the coloured fringes visible, unless when the glass was parallel, and exactly of the same thickness in both plates. I also tried plates of topaz, of equal thicknesses, and plates of sulphate of lime; but though I used pieces of various thicknesses, I have never succeeded in making them exhibit the coloured fringes, owing, perhaps, to the imperfect flatness of their surfaces.

In order to ascertain if the magnitude of the fringes depended on the thickness of the glass plates, I procured a piece of parallel crown glass $\frac{1}{1000}$ th of an inch thick, and compared the fringes which it produced, at an inclination of $2^{\circ} 20'$, and at a vertical incidence, with those produced by another piece of glass $\frac{1}{1000}$ th of an inch thick. In the first case, the circular

cular image was crossed by *five* fringes, and in the second case with *seven* fringes: But

$$\frac{121}{1000} : \frac{168}{1000} :: 5 : 7 \text{ nearly.}$$

In another experiment, I found, from a mean of five measurements, that the thickest of these pairs of plates produced fringes each of which had a breadth of 11' 10", when the inclination of the plates was 1° 58'. Now the other pair of plates gave fringes 13' 3" broad, at an inclination of 2° 20", which gives 15' 29" for their breadth at an angle of 1° 58', and

$$\frac{121}{1000} : \frac{168}{1000} :: 11' 10" : 15' 30".$$

Hence *the magnitudes of the fringes are inversely as the thicknesses of the plates which produce them, at a given inclination; and in general the magnitudes of the fringes are in the compound inverse ratio of the thickness of the plates, and of their angle of inclination.*

Hitherto we have supposed the glass plates to be placed between the eye and the luminous object, so that only the 2d, 4th, and 6th reflected images were seen. When the eye is placed between the plates and the luminous object, so as to perceive the 1st, 3d, and 5th, reflected images, the coloured fringes are also seen, having the same characters as those already noticed.

The phenomena which have been described are equally produced when the fringes are formed by polarised light, and they do not suffer the least change when examined by doubly refracting or doubly polarising crystals.

When the eye is placed at a considerable distance, either before or behind the glass plates, all the fringes have a very distorted.

distorted appearance, arising probably from the imperfect figure of the reflecting surfaces.

In order to explain the changes which the light undergoes in its passage through the plates of glass, let AB, CD , Plate XXII. fig. 1. be a section of two plates at right angles to the common section of their surfaces, and let RS be a ray of light incident nearly in a vertical direction. This ray after passing through the first plate AB , will suffer a small refraction at P and Q , and emerge in the direction QV parallel to RS . At the point P , in the second plate CD , the ray TP will be reflected to a , again reflected to b , and after suffering a refraction at b and c , will emerge in the direction cd , forming with RV an angle equal to twice the inclination of the plates. A portion of the reflected ray Pa , will enter the first plate at a , and having suffered reflexion and refraction at β , the reflected portion $\beta\gamma$ will reach the eye at θ . The ray $Pabc$ will likewise suffer a reflection at c and at e , and will reach the eye at g . In like manner, a part of the ray PQ will be reflected at Q , and move in the direction $Qrstuv$, and another part of it in the direction $swxyz$, and these rays will suffer several other reflexions; but the images which they form will be so faint, that the eye will not be capable of perceiving them. When the observer, therefore, looks at a luminous body, in the direction SR , through the glass plates, he will perceive two images, one of which is a bright image, seen by the transmitted light QV , and the other is a faint image, seen principally by the reflected light $Pabcd$, and composed of several images, formed by the pencils $cd, uv, \varepsilon\theta, \zeta\delta$, and eg . The bright image is not crossed by coloured fringes, but the fringes appear distinctly upon the other image; and the light by which these fringes are formed,

formed, has suffered two reflexions from the exterior surfaces, and two refractions at the interior surfaces of the plates.

When the ray RS is incident obliquely, so as to produce the coloured fringes, the plane of incidence is parallel to the common section of the plates. In this case, it is difficult to represent in a diagram the progress of the rays, as they are reflected in a plane at right angles to that in which they are refracted. The changes, however, which the light must undergo in the production of the fringes, may be understood from figs. 2, 3, 4, 5, 6, 7, and 8, where AB and CD are the two plates of glass, inclined at a small angle, and RS a ray of light incident obliquely, in a plane at right angles to the common section of the plate.

In fig. 2. the plates are so arranged, that the incident ray RS does not pass through the first plate AB. In this case, the *fringes are produced* in the same manner as if the ray had passed through AB.

In fig. 3. the rays reflected from the plate AD do not pass through the second plate CD. In this case, the *fringes are produced* as formerly.

In fig. 4. the reflection from the external surface *mn* of the plate AB, is destroyed by a layer of indurated Canada balsam. In this case *no fringes are produced*.

In fig. 5. the refraction and reflection at the interior surface *op* of the plate AB, is destroyed by a layer of Canada balsam. In this case *no fringes are produced*.

In fig. 6. the refraction of the interior surface of the plate CD is destroyed by a layer of Canada balsam, and in this case *no fringes are produced*.

In fig. 7. the reflexion from the external surfaces *mn, op*, of the two plates is destroyed, and *no fringes are produced*.

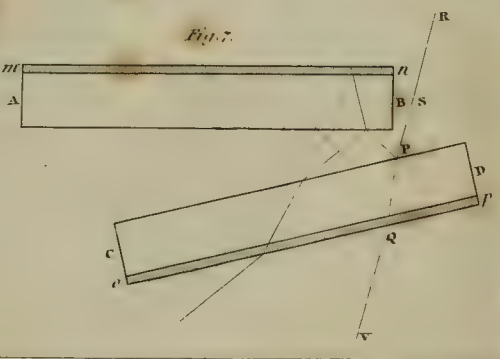
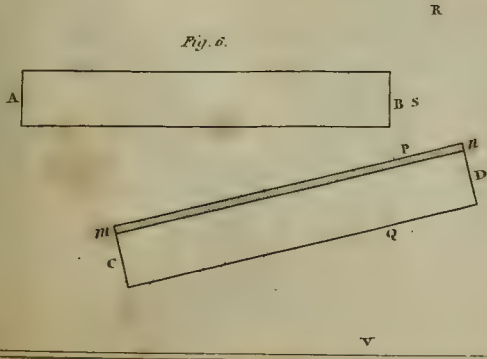
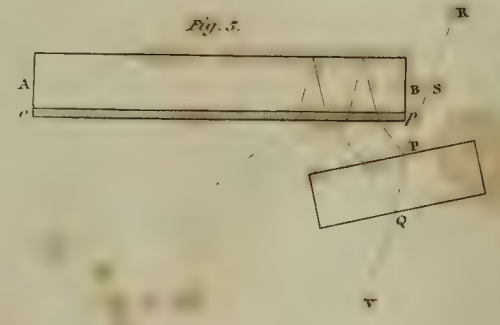
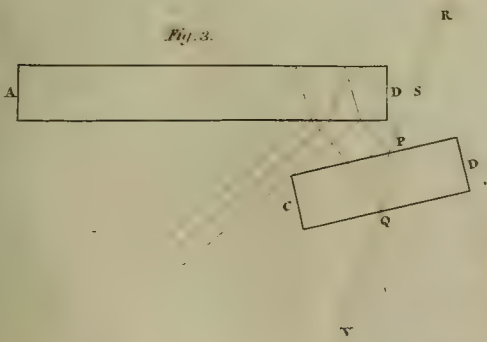
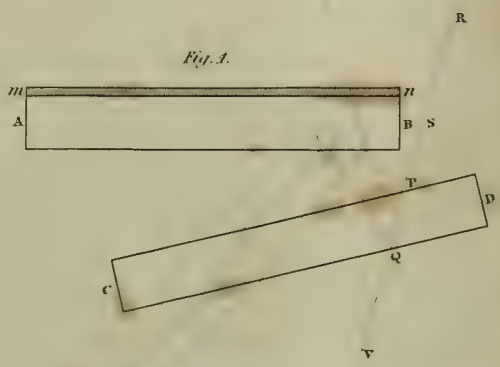
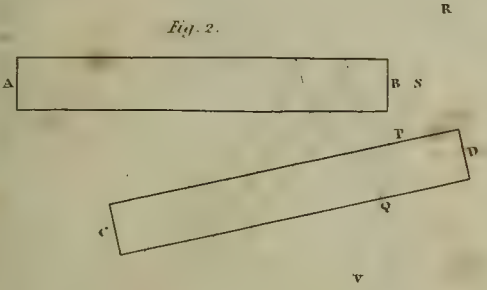
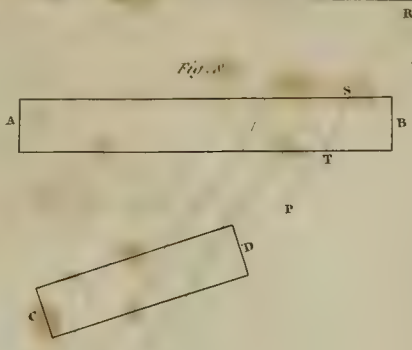
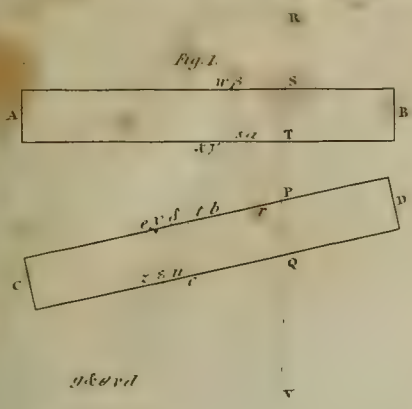
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In all these cases, the fringes are obviously produced by a refraction and a reflexion in each of the two plates, and the interfering fringes are produced by the secondary reflexions within the glass plates.

The fringes, however, which appear upon the first or bright images, are produced in a different manner from those formed by the light that has been reflected from the plate CD; for the light of which they are composed has suffered two or more reflexions within the plate AB, as shewn in fig. 8. and two refractions by the plate CD. These refractions are absolutely necessary to the production of the fringes; for they disappear when the light reaches the eye, without passing through the second plate. Any variation in the distance of the plates, when their inclination and thickness remain the same, ought obviously to produce no change in the appearance of the fringes, as the fits will return in the same manner as before.

In order to compare the preceding phenomena with the Newtonian Theory of Fits, I propose to resume the investigation with plates of parallel glass, that differ very considerably in thickness, and that have their surfaces ground as flat, and polished as highly as possible; and I have no doubt but that all the results may be calculated by means of that beautiful theory.

The fundamental experiment by which I ascertained the production of coloured fringes by two plates of glass of equal thickness, has been repeated and verified by my friend M. Bior of the Institute of France, and was exhibited at a public meeting of that distinguished body.





XVI. *An Analysis of the Mineral Waters of Dunblane and Pitcaithly; with general Observations on the Analysis of Mineral Waters, and the Composition of Bath Water and some others.* By JOHN MURRAY, M. D.
F. R. S. EDIN.

(Read November 20. 1814.)

I PROPOSE to submit to the Society the analysis of a Mineral Water of the Saline Class, which has lately been discovered in the neighbourhood of Dunblane. The subject may have rather more interest than usually belongs to researches of this nature, from the composition of this water being such as promises to afford a spring of considerable medicinal efficacy, and from its resemblance to another mineral water, of some celebrity,—that of Pitcaithly, the analysis of which I have, from this circumstance, been also led to undertake. The investigation, too, may afford some illustrations of the different methods that may be employed in the analysis of waters of this class, and of the facility and precision which are given to these researches, by the results that have been established with regard to the definite proportions in which many bodies combine, and the uniformity of the relations which thus exist between the compounds they form. And it has led to some views with regard to the constitution of mineral waters of the

saline class, which I have applied to the composition of some of the most celebrated mineral waters. In performing the principal experiments on the Dunblane water, I had the advantage of Mr ELLIS's co-operation.

I. ANALYSIS OF THE DUNBLANE WATER.

This Water was discovered last summer, and was first taken notice of, from the circumstance of the frequent resort of flocks of pigeons to the ground where it breaks out. It appears in two springs, at the distance of nearly half a mile from each other, in a field about two miles to the north of Dunblane, the property of the Earl of KINNOUL. This district is at no great distance from the range of the Grampians, to which it ascends; masses of the primitive rocks are spread over the surface, and are found in the beds of the streams; among which the conglomerate rock that seems to skirt the Grampians, is abundant. The prevailing rock of the district itself is the red sandstone, and it is generally covered by a bed of gravel, in many places of considerable depth. It is from this sandstone that the water appears to issue. The spring, however, in both the places where it breaks out, has been laid open only to the depth of two or three feet from the surface, and has not been traced to any extent. Its proper source is therefore unknown, and it also remains uncertain, how far it may be diluted with water from the surface, or from other springs. The water from the lower, or what for distinction may be named the South Spring, is weaker in taste than the water of the North Spring, and from the subsequent experiments, is proved to contain rather

ther less foreign matter. The ingredients, however, are the same, and the difference therefore probably arises from the water of the lower spring being farther diluted in its course. This difference led to the analysis of the water of both springs. It is proper to remark, that both have been submitted to examination after a season unusually dry.

Analysis of the Water of the North Spring.

The taste of this water is saline, with some degree of bitterness. As procured from the principal pool at which it issues, it is free from smell; procured, however, from some other pools, at the distance only of a few feet, its smell is slightly sulphureous, probably owing to impregnation from matter at or immediately under the soil. Its sensible operation on the system is that of a diuretic and purgative. The former effect is usually obtained, when a quantity is taken by an adult, from an English pint to a quart; the latter, when more than a quart is taken. The specific gravity of the water is 1.00475. It suffers no change in its sensible qualities from exposure to the air.

The state of the spring is at present such, that any gaseous impregnation of the water cannot be determined with precision. Bubbles of air frequently rise from the bottom of the pool, but this is merely atmospheric air: transmitted through lime-water, it produced no sensible milkiness; nor does the water appear to contain any free carbonic acid.

The usual re-agents present with the water the following appearances:

1. The colours of litmus, violet, and turmeric, are not sensibly affected.

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2. Muriate

2. Muriate of barytes produces an immediate turbidness, and rather copious precipitation, which is very slightly, if at all removed by nitric acid.
3. Nitrate of silver gives a very dense and abundant precipitate.
4. Water of potash produces a turbid appearance, not very considerable.
5. Carbonate of potash throws down an abundant precipitate, which disappears with effervescence on adding nitric acid.
6. Lime water causes no change.
7. Ammonia does not cause any precipitation, nor does it even impair the transparency of the water.
8. Oxalate of potash, or of ammonia, occasions a copious precipitation.
9. Tincture of galls has no immediate sensible effect; but after an hour or two a purplish tint is exhibited, which deepens from exposure to the air, and inclines to olive-green.

These results establish the following conclusions :

Exper. 1. proves that no free acid or alkaline matter is present, nor any alkaline carbonate.

Exper. 2. denotes the presence of sulphuric acid.

Exper. 3. indicates the presence of muriatic acid.

From Exper. 4. and 5. may be inferred the presence either of lime, or magnesia, or both.

Exper. 6. and 7. prove that magnesia is not present, nor argil.

Exper. 8. proves the presence of lime.

Exper. 9. indicates a minute portion of iron.

The

The saline taste of the water, and the precipitation so abundant by nitrate of silver, render probable the presence of muriate of soda, and it is accordingly obtained, when the water is evaporated nearly to dryness, cubical crystals of it forming in the saline liquid.

From the whole, therefore, the principal ingredients of this water may be inferred to be muriates of soda and lime, with a smaller portion of a sulphate, and a minute quantity of iron. These conclusions suggested the following method of analysis.

An English pint of the water was evaporated to dryness; and the solid residuum was exposed to a heat approaching to redness, until it became perfectly dry. It weighed while warm 47 grains. It quickly attracted moisture from the air, so that its surface soon became humid, and on leaving it exposed for twenty-four hours, a considerable portion was dissolved, forming a dense liquor, while a portion remained undissolved.

The whole solid matter being rendered dry, was submitted to the action of alkohol, with the view of separating by solution the muriates of soda and lime, of which it was supposed to be principally composed. It is well known, that this method is liable, in some degree, to two sources of error; the one, that a little muriate of soda is dissolved by the alkohol with the muriate of lime; the other, that even when a large quantity of alkohol is employed, the undissolved muriate of soda retains a small portion of muriate of lime. In estimating the quantities from the results, these errors, indeed, in some measure, counterbalance each other; but still they may exist in different degrees, according to the quantity and strength of the alkohol, and it is necessary therefore to obtain perfect precision, to obviate them as far as possible.

With:

With this view, the entire matter was digested with repeated portions of alkohol, of the specific gravity of 836, until about six times its weight had been employed; the solvent action being aided by frequent agitation, and an occasional heat of about 100°. It was then lixiviated with a small portion of distilled water, to remove more effectually from the muriate of soda any adhering muriate of lime. The different liquors being mixed, were evaporated to dryness; and this dry mass was again submitted to the action of alkohol, more highly rectified, (being of the specific gravity of 825), and in smaller quantity, so as to dissolve only that part of it which was muriate of lime. A small portion of muriate of soda, which had been dissolved in the first digestion, was thus obtained, and was added to the residue of that operation. The whole undissolved matter being dried at a low red heat, weighed while warm 28.5 grains: it was in small grains, having a taste purely saline. The alcoholic solution afforded, by evaporation, a matter which entered into fusion, and which, after being dried at a heat approaching to redness, weighed while warm 18.2 grains. It was highly deliquescent, so as to increase quickly in weight, and in a short time became humid on the surface.

These two products were evidently principally muriate of soda, and muriate of lime. But it was necessary to ascertain if they were entirely so, as both of them might contain small portions of other ingredients.

The matter dissolved by the alkohol, supposing it to be muriate of lime, would require for its conversion into sulphate of lime about sixteen grains of sulphuric acid, of the usual strength. Eighteen grains were added with a small portion of distilled water, and heat was applied; vapours of muriatic acid were discharged: To render the mutual action more complete, small portions of water were successively added, the soft
mass

mass being frequently stirred, and when the vapours had ceased to exhale, the heat was raised to redness, to expel any excess of acid. The dry matter weighed 22 grains, precisely the quantity that ought to be obtained from 18 grains of muriate of lime:

It was diffused in a quantity of water, which it at first absorbed with a hissing noise. The water, after having been added in successive quantities, with frequent agitation, being poured off, the undissolved matter was dried at a low red heat: it weighed 18.5 grains, and formed a soft white powder, free from taste. The water poured off was very slightly acidulous. This was neutralised by ammonia; it was then evaporated to dryness, and the solid matter was heated to redness. On again submitting it to the action of a small quantity of water, a portion remained undissolved, which weighed when dried 2 grains.

There were thus obtained 20.5 grains of sulphate of lime, a quantity equivalent to 16.7 of dry muriate of lime. The small portion of liquor which remained in the last operation, had a bitterish taste: by spontaneous evaporation, it formed acicular crystals; diluted with distilled water, it became slightly turbid on adding oxalate of ammonia, and more so on the addition of alcohol; but in the latter case, the transparency was restored on adding water. With a minute portion, therefore, of sulphate of lime, it appeared to be principally sulphate of soda, derived from a little muriate of soda, which, notwithstanding the precautions that were employed, had adhered to the muriate of lime.

The matter which remained undissolved by the alcohol, weighed, it has been stated 28.5 grains. It remained to ascertain if it were entirely muriate of soda.

Being

Being agitated with about half an ounce of distilled water, the greater part was dissolved. The portion which remained undissolved, after being washed with small quantities of distilled water, and dried, weighed 2.4 grains. To this matter a little diluted nitric acid being added, a slight effervescence was excited: a thin crust, too, adhered to the sides of the small glass globe in which the last stage of the evaporation had been performed, which was dissolved with effervescence by a weak acid. The quantity of *carbonate of lime* thus indicated, may be estimated at 0.5 grain. The remainder of the undissolved residue being washed and dried, was heated with two or three drops of sulphuric acid; and was thus rendered soluble in water. When neutralized by ammonia, the solution became milky; but its transparency was restored by adding more water; it became quite turbid on adding oxalate of potash, and a precipitate was thrown down by alcohol. It was therefore *sulphate of lime*. Its quantity may be stated at 2 grains.

The solution had a taste purely saline. The test of oxalate of ammonia, however, shewed the presence in it of a small quantity of lime; the addition of the oxalate was therefore continued as long as any precipitation took place, and the precipitate was collected and dried. It weighed 1.3 grains. This production of oxalate of lime evidently arose from the presence of a small portion of muriate of lime, which, notwithstanding the precautions that had been employed, had adhered to the muriate of soda. Supposing that this had not escaped the action of the alcohol, but had been dissolved by it, and in the subsequent stage of the experiment, been converted into sulphate of lime, it would have increased the quantity of this sulphate about 1.2 grains, making it therefore 21.7, equivalent to 17.6 grains of dry *muriate of lime* which the pint of water contains.

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The solution contained also a minute quantity of sulphuric acid; for after removing any slight excess of oxalic acid that might have been present, it still gave a precipitate on the addition of muriate of barytes. Supposing this, as well as the rest of the sulphuric acid, to have existed in the water in the state of sulphate of lime, it will increase the quantity of that ingredient, (calculating from the weight of the precipitate of sulphate of barytes obtained), from the 2 grains formerly noticed to 2.9.

There appeared now to remain nothing but pure muriate of soda. The solution by slow evaporation afforded that salt in cubical crystals, which, dried at a low red heat, weighed 24.5 grains. Allowing 0.8 of this as the portion of product formed by the action of the muriate of barytes, it leaves 23.7 grains. And if to this be added one grain, as the equivalent of the small portion of sulphate of soda, already noticed as formed by the action of the sulphuric acid on the muriate of soda adhering to the muriate of lime after the operation of the alcohol, it gives the quantity of muriate of soda at 24.7 grains.

From these results, the solid ingredients in a pint of this water appear to be

Muriate of soda	-	24.7 grains.
Muriate of lime,	-	17.6
Sulphate of lime,	-	2.9
Carbonate of lime,	-	0.5
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		45.7

With a trace of iron.

Having completed the analysis in this manner, I wished to confirm it by a different method. A very simple one presented
 VOL. VII. P. II. 3 M ed

ed itself,—to reduce by evaporation to dryness,—obtain the sulphate of lime as before,—then, dissolving the mixed mass of muriate of lime, and muriate of soda in water, decompose the muriate of lime by oxalate of ammonia, so as to find the quantity of it present, and after evaporation to volatilize the muriate of ammonia by heat, and thus obtain the muriate of soda. The results in this mode ought to correspond with those in the former; and the one, therefore, afford a confirmation of the other, or lead to the discovery of any fallacy if it exist.

A pint of the water was evaporated to dryness, and afforded, as before, 47 grains of solid matter. This being submitted to the action of a small quantity of distilled water, was dissolved, with the exception of a residue of sulphate of lime, which weighed 2.6 grains, and a little carbonate of lime, which may be estimated, as before, at 0.5 grain.

To the clear solution, a solution of oxalate of ammonia was added as long as any turbid appearance was produced; and after the precipitate had subsided, the liquor was heated nearly to boiling, to render the mutual action and the precipitation more perfect. The precipitate being repeatedly washed with distilled water, was dried by the heat of a sand bath raised gradually, and kept lower than a red heat. It weighed 21 grains. The quantity of muriate of lime which would be equivalent to this, cannot be inferred with certainty, from any previous analysis of oxalate of lime; for as the oxalate cannot be exposed to a red heat without decomposition, it cannot easily be subjected to a precise degree of heat, by which we can be certain of obtaining it in a uniform state of dryness*. It is necessary,

* Referring to those analyses which may be supposed to be most accurate, 21 grains of oxalate of lime will be found equivalent to various proportions, from 17.5 to 19.9 of muriate of lime.

cessary, therefore, that in every case, the quantity of lime should be found in the oxalate that is operated on. The above quantity of 21 grains was converted by calcination into carbonate of lime, and this being decomposed by muriatic acid, the quantity of muriate of lime obtained, dried at a low red heat, and weighed while warm, amounted to 18.3 grains.

The liquor poured off from the precipitate, was evaporated to dryness; and to expel the muriate of ammonia formed by the action of the oxalate of ammonia on the muriate of lime, the heat was continued while any vapours were disengaged, and at the end was raised nearly to redness. The dry mass weighed, while warm, 25 grains. Being dissolved in water, its solution was rendered very slightly turbid by the addition of muriate of barytes, shewing the presence of a minute portion of sulphuric acid. A quantity of precipitate was collected, which, when dried, weighed 0.8 grain. Supposing the sulphuric acid of this to have originally existed in the water, along with the other portion of this acid, in the state of sulphate of lime, it gives a proportion of that sulphate of 0.5 grain, and of course increases the quantity of it from the 2.6 grains obtained by evaporation to 3.1 grains. An equivalent quantity must at the same time be subtracted from the proportion of muriate of lime, which may therefore be reduced to 18 grains. By evaporation of the liquor, muriate of soda was obtained, weighing, when it had been dried at a low red heat 24.3 grains. Of this a small portion (0.4) would be formed by the muriate of barytes, which requires to be deducted; but then the sulphuric acid which existed in the mass, could, after the action of the oxalate of ammonia, and the exposure to a red heat, exist in it only in the state of sulphate of soda, in the production of which an equivalent portion of muriate of soda would be decomposed. The quantity of muriate of soda obtained, therefore, by

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the

the evaporation, may be regarded as the just proportion indicated by the analysis.

The results, then, by this method, agree very nearly with those by the other ; being of solid ingredients in a pint of the water,

Muriate of soda,	-	24.3 grains.
Muriate of lime,	-	18
Sulphate of lime,	-	3.1
Carbonate of lime,	-	0.5
		45.9

With a trace of iron.

With regard to both analyses, a small correction is to be made in the proportion of sulphate of lime. The mode of ascertaining it, by evaporation, being rather imperfect, I afterwards determined it by the more accurate method of precipitation by muriate of barytes ; applying this re-agent with a slight excess of acid, so as to prevent any precipitation of carbonate. The quantity of precipitate thrown down from a pint of the water, amounted, after drying at a low red heat, to 6.1 grains, equivalent to 3.5 grains of sulphate of lime. As the portion of sulphate of lime thus obtained, above that obtained by the evaporation, would remain principally mixed with the muriate of soda, the quantity of that ingredient falls to be reduced a little, and may therefore be stated at 24 grains.

It remained to ascertain the proportion of iron. The quantity, however, was evidently so small as to present a difficulty. Succinate of ammonia, and benzoate of soda, produced little or no effect on the water in its natural state. Infusion of galls produced, after some hours, a dark colour, and a precipitate very slowly subsided. This method has been employed to ascertain

certain minute quantities of iron, and I endeavoured to avail myself of it,—adding to the water infusion of galls, in small successive portions, at the interval of a day or two, as long as the colour appeared to be rendered deeper ; leaving it exposed to the air for a longer time, that the whole matter rendered insoluble might subside ; and, lastly, washing the precipitate, drying and calcining it, to consume the vegetable matter, and obtain the oxide of iron. The difficulty, however, attending this method, is that of precipitating entirely the iron, the liquor never becoming colourless. In one experiment, conducted with much care, the quantity of the calcined product from two pints amounted to 0.4 grain ; but it consisted partly of carbonate of lime. To remove this, pure muriatic acid diluted was added in excess, and a moderate heat was applied ; the precipitate was entirely dissolved, and the liquor acquired a deep yellow colour. Being farther diluted, a little pure ammonia was added to it, in a close phial, to precipitate the oxide of iron, while the lime should remain dissolved. The quantity thus obtained, when dried, amounted to 0.27 grain.

This method being liable to the above objection, I employed another : Two pints of the water were evaporated : when reduced to about two ounces, a brownish coloured sediment was deposited, which was removed ; the evaporation was carried to dryness, and the dry mass was re-dissolved in distilled water. The insoluble residue was of a greyish colour, and to this, the deposite formed during the evaporation was added. It was known by previous experiments, that the greater part of the iron was separated in this way ; the insoluble matter, when digested with muriatic acid, affording a liquor, when diluted with water, which gave, after neutralisation with ammonia, a deep colour with tincture of galls. To ensure, however, the more perfect separation of the iron, ammonia was added to
the

the solution of the solid matter which had been procured by evaporation, and care being taken that the ammonia was free from carbonic acid, little or no precipitation could take place but of oxide of iron. A yellowish flocculent precipitate subsided slowly, which, after being washed, was added to the insoluble residue.

The insoluble matter thus collected consisted, as the preceding steps of the analysis establish, chiefly of sulphate, with a smaller portion of carbonate of lime, with which was mixed the oxide of iron. A drop or two of sulphuric acid was added, to convert the carbonate into sulphate of lime; and heat was applied to expel any excess of acid. A little pure muriatic acid was then added to dissolve the oxide of iron, and to form with more certainty the red muriate, soluble in alcohol, a drop of nitric acid was added along with it. On applying heat, with the addition of a little water, to favour the action, a yellow colour was acquired. When the excess of acid was nearly dissipated, the mass was repeatedly lixiviated with alcohol, in which sulphate of lime being insoluble, the muriate of iron only would be dissolved. The alcohol acquired accordingly a yellow colour. Being evaporated by a gentle heat, it gave a residuum, which, on a drop of nitrous acid being added, became of a deep reddish-brown colour, and after being heated strongly, weighed 0.34 grain. Re-dissolved in muriatic acid, it formed a rich yellow coloured solution, and gave a deep colour with tincture of galls.

Even in this way, the whole iron is not obtained; for the solution of the saline matter, though ammonia had been added to it, to precipitate the iron, still gave a weak colour with galls. The quantity therefore is rather under-rated. Taking the above, however, as the proportion, the whole composition will be in a pint of the water of the North Spring,

Muriate

Muriate of soda,	-	24 grains.
Muriate of lime,	-	18
Sulphate of lime,	-	3.5
Carbonate of lime,	-	0.5
Oxide of iron,	-	0.17
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		46.17

Analysis of the Water of the South Spring.

The water of this spring has a taste similar to that of the other, but rather weaker: it produces similar medicinal effects. In the present state of the spring, its strength is more variable, according to the state of the weather. From this circumstance, and from its being rather weaker, it has probably a greater intermixture of surface-water, or of the water of other springs. When taken up after continued dry weather, it afforded, by evaporation, 42 grains of solid matter from a pint; the other affording, at the same time, 47 grains. Its specific gravity was found to be 1.00419. It was in this state, the strongest in which it was found, that it was submitted to the following examination.

The application of re-agents produced the same appearances as with the water of the North Spring, indicating, therefore, the presence of the same ingredients. To determine this with more precision, and to ascertain the proportions, the same methods of analysis were employed which had been used with regard to the other. It will be sufficient to state the results by one method,—the second of those before described.

A pint of the water was submitted to evaporation, and afforded of dry matter, weighed while warm, 42 grains. This

was

was re-dissolved in distilled water. There remained undissolved a portion, which, when thoroughly dried, weighed 2.5 grains. This suffered a very slight effervescence with muriatic acid, similar to that excited in the insoluble matter of the water of the North Spring; a similar thin crust, too, had formed on the sides of the glass capsule, which was removed with effervescence by a drop of muriatic acid. The relative proportions, therefore, of sulphate and carbonate of lime may be regarded as the same: and the insoluble residue will thus consist of 0.3 of carbonate, and 2.3 of sulphate of lime. By precipitation by muriate of barytes from another pint of the water, similar results were obtained.

To the clear liquor, oxalate of ammonia was added as long as it produced any turbid appearance. The precipitate collected and dried, being converted by calcination into carbonate of lime, afforded, when acted on by muriatic acid, 16 grains of dry muriate of lime.

The solution poured off from the precipitate, was evaporated to dryness, and the dry mass was exposed to a heat gradually raised to redness, until it ceased to exhale any vapour. The muriate of ammonia formed by the action of the oxalate of ammonia on the muriate of lime, was thus expelled, and the muriate of soda of the water remained. It weighed 22.5 grains.

The results, then, by this method, are from a pint of the water,

Muriate of soda,	-	22.5 grains.
Muriate of lime,	-	16
Sulphate of lime,	-	2.3
Carbonate of lime,	-	0.3
Oxide of iron,	-	0.15
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		41.25

The

The proportion of iron I have stated as similar to that of the North-Spring, from the colour produced by the tincture of galls being nearly the same.

From the almost perfect similarity in the composition of the two waters, with regard to the proportions of their ingredients, there is every reason to conclude, that they are from the same spring; the weaker being either mixed with surface water at the pool, or being diluted in its course.

The determination of the composition of this water, suggests the question, whether this is such as to account for the medicinal effects it produces. It acts, as has been stated, as a diuretic, and in a larger dose, as a cathartic. This water, and the mineral water of Pitcaithly, present, in some respects, a peculiarity. The greater number of saline waters which have a purgative quality, contain magnesian salts, to which, as they are known to act as cathartics, the effect is obviously to be ascribed. Of the ingredients of the Dunblane and Pitcaithly Springs, muriate of lime is scarcely known to have any purgative power in its pure form, and if muriate of soda possess it, it is only in an inconsiderable degree. Still, there can be no doubt, that it is to this impregnation that their operation is owing, and they afford a proof, therefore, of what is indeed sufficiently established, that the powers of mineral waters are often much greater than could be expected from the nature and quantity of their ingredients, and that the action of saline substances is increased, and considerably modified, when they are in a state of great dilution.

Independent of its purgative operation, and its adaptation to the treatment of diseases in which this is advantageous, its composition may probably render it a remedy of efficacy in some constitutional affections, particularly in scrofula. Muriate of lime has attained some celebrity in the treatment of

this disease ; it is a substance of considerable activity in its effects on the living system ; and it will probably operate with more effect, and more advantage, in the state of dilution in which it is presented in a mineral spring, than when given in a more concentrated form. The muriate of soda may coincide with it in efficacy, and will be of advantage from its grateful taste, and its stimulant action on the stomach. And the chalybeate impregnation will communicate some degree of tonic power. When employed in such cases, it probably ought to be given in smaller doses, than when the advantage to be derived from it depends on its purgative operation ; and it may even prove more advantageous, if given in a state of greater dilution. I shall in the sequel have to state a view of its composition, which connects it with some mineral springs of great celebrity, and particularly with the Bath waters.

Dunblane, as a watering-place, would be possessed of considerable advantages. Situated between the range of the Ochil Hills and the Grampians, it is well sheltered, and hence enjoys a mild atmosphere ; and the soil, from being a bed of gravel for a number of miles around, is extremely dry,—an advantage inestimable in a moist climate.

II. ANALYSIS OF PITCAITHLY WATER.

The water of Pitcaithly may be regarded as the principal mineral water of the saline class in this country. Dr DONALD MONRO shewed, that, along with a little mild calcareous earth, it contained muriate of soda, with a deliquescent salt, which he inferred to be chiefly “ a calcareous marine,” that is, muriate of lime*. An analysis of it was published a number of years

* *Philosophical Transactions*, vol. lxi.

years ago, executed by Messrs STODDART and MITCHELL of Perth. There are different springs, the waters of which they found to be somewhat different in strength. The nature of the impregnation is in all of them, however, the same.—Selecting the strongest it contains, according to their analysis, the following ingredients in an English pint :

Atmospheric air,	-	0.5 cubic inch.
Carbonic acid gas,	-	1 ———
Muriate of soda,	-	12.5 grains:
——— lime,	-	22.5
Sulphate of lime,	-	0.7
Carbonate of lime,	-	0.6 *

The composition of this water, according to this analysis, is very similar to that of the Dunblane water. No account is given, however, so far as I have been able to discover, of the manner in which it had been executed, and it is therefore uncertain to what state of dryness the ingredients had been brought to which their proportions are referred. Hence no comparative estimate can be made of it with any other mineral water; and this led me to undertake its analysis, in the same manner as that of the Dunblane water.

Pitcaithly is situated in the valley of Strathern, and though at rather a greater distance from the front range of the Grampians than Dunblane, it is not improbable that the spring may have a similar origin with the Dunblane one, and may rise from the red sandstone which appears to form the first rock

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* *Statistical Account of Scotland*, vol. viii.

on descending from the primitive rocks, and to extend over all this district.

The taste of this water is saline, and somewhat bitter. Comparing it with the Dunblane water, both being tasted at the same time, the taste of the Dunblane water is stronger, and in particular more saline than that of the other. The medicinal operation of the Pitcaithly water, in the sensible effects it produces, is diuretic and purgative.

The gaseous impregnation of the water could be examined properly only at the spring, which I had not the opportunity of doing. But having procured a quantity of the water, I submitted it to the same examination as in the preceding analysis, to ascertain its solid contents. The usual re-agents produced the following appearances :

1. The colours of litmus, violet, and turmeric, were scarcely affected. If there were any change, it was that of the litmus becoming more blue, and that of the violet-green; but this was so slight as to be rather doubtful. The turmeric underwent no change.
2. Muriate of barytes produced a turbid appearance and precipitation; but this was much less considerable than in the Dunblane water. The transparency was not restored by nitric acid.
3. Nitrate of silver produced a very dense and copious precipitate.
4. Water of potash gave a milkiness not very considerable.
5. Carbonate of potash threw down a copious precipitate, which disappeared with effervescence on adding nitric acid.

6. Lime

6. Lime water had no sensible effect.
7. Ammonia, when perfectly free from carbonic acid, caused no turbid appearance.
8. Oxalate of ammonia produced an abundant precipitation.
9. Tincture of galls, added in a very minute quantity, did not immediately produce any effect, but after a few hours, a dark colour appeared, which gradually deepened, inclining to an olive-green.

With all these tests, the general results are the same as those from the operation of the same tests on the Dunblane water. In experiment 7th, the ammonia, if not perfectly free from carbonic acid, produced a slight turbid appearance, and even when in its purest state, a very slight opalescent hue was perhaps apparent; but this obviously depended on the presence of a little carbonic acid; for when a drop or two of nitric acid was previously added, and the water heated, no such appearance was produced; or, if boiled strongly, without any addition of acid, on restoring the original quantity of liquid, by adding distilled water, the transparency was not in the slightest degree altered on adding pure ammonia. The slight precipitate, too, which did occur in any case, was dissolved by the most minute quantity of muriatic acid with effervescence; and this solution became turbid on adding oxalate of ammonia, proving the precipitate to have been carbonate of lime.

The same general conclusions, then, with regard to the nature of the ingredients, are to be drawn from the preceding results as from the application of the same tests to the Dunblane water. They suggest of course a similar mode of analysis. I preferred the second of the methods above described, as being the most simple, and easy of execution.

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An English pint of the water was submitted to evaporation. Before the matter became dry, numerous cubical crystals were formed, indicating the presence of muriate of soda; when dry, the solid matter entered readily into fusion with effervescence, denoting the predominance of muriate of lime. The dry matter was highly deliquescent. After exposure to a heat inferior rather to redness, it weighed while warm 35 grains.

This dry matter was re-dissolved in about ten times its weight of distilled water. A small portion remained undissolved, which, being washed and dried, weighed 1.2 grain. A little diluted muriatic acid dropt upon this, excited slight effervescence; but the greater part remained undissolved, and weighed, after washing and exsiccation, 0.9 grain. It was *sulphate of lime*. A very thin crust adhered to the sides of the glass globe in which the last stage of the evaporation had been performed. This was dissolved with effervescence by diluted muriatic acid, and the solution became quite turbid on adding oxalate of ammonia. The quantity of *carbonate of lime* thus indicated, adding the portion abstracted, as above, from the sulphate, cannot be estimated at more than 0.5 grain. These results were confirmed by precipitation from another portion of the water by muriate of barytes, the proportions indicated being nearly the same.

The liquor poured off from the insoluble residue, being diluted with distilled water, oxalate of ammonia was added to it, as long as any turbid appearance was produced; and after the subsidence of the precipitate, the liquor was boiled a little, to render the decomposition and precipitation complete. The clear liquor was then evaporated to dryness, and the dry mass was exposed to heat, to volatilize the muriate of ammonia, the product of the action of the oxalate of ammonia on the muriate of lime; the heat being continued as long as any vapours exhaled,

exhaled, and at the end being raised to redness. The muriate of soda thus obtained, weighed 13.4 grains. By solution and crystallisation it was obtained in cubes.

The precipitate of oxalate of lime having been thoroughly washed, was exposed in a sand bath to a heat short of redness, until it had ceased to exhale any vapours, and appeared perfectly dry; it weighed 23.8 grains. The portion of muriate of lime equivalent to any quantity of oxalate of lime, cannot, as has been already remarked, be exactly assigned, from the difficulty of bringing the oxalate to one uniform state of dryness. But, according to the most accurate analyses, 23.8 grains of dry oxalate are equivalent to 20 grains of dry muriate. To avoid any error, however, the oxalate was converted into carbonate of lime by calcination; and this, decomposed by muriatic acid, afforded 19.5 grains of dry muriate of lime.

The proportions, then, of the saline ingredients in an English pint of the Pitcaithly water, are according to this analysis,

Muriate of soda,	-	13.4 grains.
Muriate of lime,	-	19.5
Sulphate ———	-	0.9
Carbonate ———	-	0.5
		34.3

To which are to be added of aerial ingredients,

Atmospheric air,	-	0.5 cubic inch.
Carbonic acid gas,	-	1 cubic inch.

It also gives slight indications of the presence of iron; but as far as can be judged from the shade of colour produced by tincture of galls, the quantity is much smaller than in the Dunblane

blane water. It does not admit, therefore, of being determined with much accuracy by actual experiment.

After I had completed the preceding analysis, a view occurred to me with regard to the composition of these waters, different from that which has been stated above; and which, if just, may lead to conclusions of some interest with regard to the constitution of mineral waters of the saline class. This I have lastly to illustrate.

III. OBSERVATIONS ON THE COMPOSITION OF SALINE MINERAL WATERS.

It is a question not unequivocally determined, and perhaps not capable of being determined, in what state the saline ingredients of a mineral water exist,—whether the acids and bases are in those binary combinations which constitute the different neutral salts, or whether they exist in simultaneous combination, the whole acids being neutralised by the whole bases. If the former, which is the more common, and perhaps the more probable opinion, be adopted, it is at least certain, that the state of combination may be modified by the analytic operations, and that the binary combinations obtained by these, may not be precisely those which existed in the water. In the case of the Dunblane water, for example, the ingredients obtained are muriate of soda, muriate of lime, and sulphate of lime. Now it is possible that the sulphate of lime may

may be a product of the operation, not an original ingredient. The sulphuric acid may exist rather in the state of sulphate of soda, and when, in the progress of the evaporation, the liquor becomes concentrated, this salt may act on a portion of the muriate of lime, and by mutual decomposition, form corresponding portions of muriate of soda, and sulphate of lime.

A question of this kind is not merely one of speculation, but the solution of it may sometimes throw light on the properties of mineral waters, particularly on their powers of affecting the living system. The present affords a very good example of this. Sulphate of lime is a substance apparently inert. If it exist, therefore, as such in the water, it can contribute nothing to its efficacy. But in the other state of combination which is supposed, both the quantity of the muriate of lime, the more active ingredient, will be greater, and the presence of sulphate of soda will in part account for the purgative operation which the water exerts.

There is no very direct, and perhaps no decisive experiment by which this question may be determined; for any method which would cause the *separation* of either substance as a binary compound, may also be conceived to operate by causing its *formation*. Thus, though sulphate of lime is obtained by evaporation, this is no proof of its prior existence, since the concentration of the solution might equally cause its formation, by favouring the action of the sulphate of soda, if it exist, on the muriate of lime. Its separation by a precipitant, by alcohol for example, even if it were obtained, is liable to the same ambiguity; a certain degree of concentration of the watery solution would be necessary for the effect, and the farther operation of the alcohol might be precisely on the same principle, —diminishing the solvent power of the water, and thus aiding

the force of cohesion, in determining the combination of the ingredients which form the least soluble compound. If a different mode of analysis were had recourse to, if the whole lime, for example, were precipitated by any re-agent, there would still remain the uncertainty with what it had been combined, whether entirely with muriatic, or partly with sulphuric acid; and there is no mode of determining this, by obtaining the other product of the action of the re-agent, which would not be liable to equal ambiguity; or, if the sulphuric acid were abstracted by a re-agent, there would equally be the uncertainty, whether it had been previously combined with soda or lime.

If sulphate of lime did not separate when the water was reduced by evaporation so far, that, from the known solubility of the sulphate, the precipitation of it ought to take place to a certain extent, it might be concluded that it did not exist. Yet even this conclusion, were the fact found to be so, (which it is on making the experiment), is invalidated by the result, sufficiently established, that salts, by their mutual action, often increase the solubility of each other, and the sulphate of lime might, from this cause, be retained dissolved, in a smaller quantity than it would require by itself for its solution.

One kind of proof may be given, that of shewing, that a much larger quantity of sulphate of soda, than what analysis indicates in this water, may exist in it, without any precipitation of sulphate of lime. I added to different portions of the water (four ounces each) 5, 10, 15, 20, and 30 grains of sulphate of soda. In none of the experiments was there any immediate effect, and even after twenty-four hours, there was no turbid appearance, or apparent change. In the greater number of these proportions, the quantity of sulphate of soda was
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more than sufficient to convert the whole muriate of lime in the water into sulphate; and, according to the known solubility of this sulphate, the quantity of water was not sufficient to retain it all dissolved. This quantity was even reduced to a certain extent by evaporation, without any precipitation. The result seems therefore to prove, that sulphate of lime had not been formed, and that sulphate of soda may exist with muriate of lime in solution without decomposition, in the state of dilution which this mineral water affords.

Another result which I obtained, and which so far favours the opinion that the sulphate of lime is formed in the progress of the evaporation by the reciprocal action of sulphate of soda and muriate of lime, is, that when a small portion of sulphate of soda has been added, the quantity of sulphate of lime obtained is increased: when 10 grains, for example, of crystallised sulphate of soda were added to a pint of the water, after evaporation to dryness, 4 grains of sulphate of lime, which is double the proportion that the water would otherwise have yielded, were obtained,—affording a proof that when sulphate of soda is dissolved in the water, it produces, in the progress of the evaporation, a corresponding portion of sulphate of lime, and of course also of muriate of soda.

These results do not absolutely establish the conclusion, that the sulphuric acid exists in this water in the state of sulphate of soda; yet, on the whole, this is the more probable opinion. If it be admitted, the preceding statement of the ingredients, and their proportions, must be altered. The sulphate of lime is of course to be omitted. The sulphate of soda, which is to be substituted for it, cannot be obtained by any method; but the quantity of it may be inferred, from the quantity of sulphate of lime which is formed by its action on the muriate of lime. Real sulphate of lime, and real sulphate of soda, are

very nearly equivalent to each other with regard to the proportions of their acid and base ; so that the quantity of the one may nearly be substituted for that of the other ; 3.5 of sulphate of lime being equal to 3.7 of sulphate of soda. But this sulphate of lime is formed at the expence of a portion of muriate of lime, and its formation is accompanied with the production of a little muriate of soda ; hence the proportion of the former must be a little larger, and that of the latter a little smaller, than have been before stated. 3.5 grains of sulphate of lime are equivalent to 2.8 of muriate of lime, which quantity, therefore, is to be added to the proportion above assigned. The equivalent portion of muriate of soda to be subtracted is 3. The whole proportions, therefore, will be the following :

Muriate of soda,	-	21 grains.
Muriate of lime,	-	20.8
Sulphate of soda,	-	3.7
Carbonate of lime,	-	0.5
Oxide of iron,	-	0.17
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		46.17

The quantity of sulphate of lime obtained in the analysis of the Pitcaithly water, being so much smaller than that in the Dunblane, it may perhaps be considered as an original ingredient ; or if even the opposite view be adopted, the change in the proportions, as indicated by the analysis, is much less. They may be stated as follow :

Muriate of soda,	-	12.7 grains.
Muriate of lime,	-	20.2
Sulphate of soda,	-	0.9
Carbonate of lime,	-	0.5

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The carbonate of lime contained in both waters, may, it is obvious, according to the same view, be a product of the operation, and may exist in the water in the state of carbonate of soda. Yet the quantity is so small, and carbonate of lime is so generally diffused in the mineral kingdom, that it may perhaps be regarded as an original ingredient. On the other hand, it seems to be nearly insoluble in water, and this favours the supposition that it is a product of the analysis. It is unquestionably so in the mineral waters in which it has been stated to exist in much larger quantity, and in which there is not, at the same time, any excess of carbonic acid, by which it might be retained dissolved.

The view of the constitution of this mineral water which I have now explained, suggested a method of analysis which I may state, both as it accords with, and in some measure confirms it, and illustrates some circumstances connected with the mutual action of the sulphate of soda, and muriate of lime, to which I shall afterwards have to refer. It affords, too, an excellent illustration of the definite proportions in which many bodies combine, and the uniformity of results which are obtained from their action on each other, in consequence of this law.

Supposing the sulphate of lime obtained from this water by evaporation, to be formed by the action of sulphate of soda on a portion of its muriate of lime, it might be inferred, that by adding the due proportion of sulphate of soda, the whole muriate of lime it contains may be converted into sulphate of lime; and this, from its insolubility, being easily separated from the muriate of soda, the quantity of it, and of course the quantity of muriate of lime, will be ascertained. From the preceding analysis, 18 grains of muriate of lime appear to exist in a pint of the Dunblane water. Now this quantity requires
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for its decomposition 23.1 of real sulphate of soda; and the products of this decomposition are 22.1 of real sulphate of lime, and 19 of muriate of soda*. The former of these products being collected and dried, may be weighed, and the latter being deducted from the whole quantity of muriate of soda obtained by evaporation, the remainder will be the quantity originally contained in the water. The obtaining these quantities, therefore, or near approximations to them, will be at once a confirmation of the preceding analysis, and of the accuracy of these proportions.

A pint of the water was evaporated to about one-fourth; the quantity of real sulphate of soda required for the decomposition of its muriate of lime, it has been just stated, is 23.1 grains. But, by previous trials, I had found, that a small excess of sulphate of soda renders the decomposition more complete; 24 grains, obtained by exposing crystallised sulphate of soda to a red heat, were, therefore, added. The liquor soon became turbid and thick. I had also found, that to render the decomposition more complete, it is of advantage not to evaporate at once to dryness, but to add small quantities of water occasionally for some time during the boiling. The experiment having been conducted in this manner, a precipitate of sulphate of lime was collected, which, when washed and dried, weighed 19 grains. The liquor being evaporated, afforded of dry salt 51.6 grains. But on dissolving this salt in water, a deposit of sulphate of lime remained undissolved; and even on again evaporating to dryness, and re-dissolving in water, a small portion was deposited for three successive times. The whole

* The inspection of the scale of chemical equivalents, gives at once these numbers; and this highly useful instrument, lately invented by Dr WOLLASTON, facilitates greatly all such researches, by the number of results it presents without the necessity of calculation.

whole quantity of sulphate of lime thus collected amounted to 5.8 grains, and of course increased the former quantity of 19 to 24.8 grains. Supposing the quantity of sulphate of lime originally contained in the water, or, what is the same thing, capable of being produced, in its evaporation, from its own ingredients, to amount to 3.8 grains, this leaves 21 grains formed by the action of the sulphate of soda which had been added on the muriate of lime; and this is equivalent to 17.1 grains of muriate of lime. The saline matter obtained by evaporation of the solution, weighed after exposure to a red heat 44.4 grains. Of this, supposing it to be all muriate of soda, 18 grains would be formed by the action of the sulphate of soda on the muriate of lime; and there remain 26.4 grains as the quantity which the water had contained. This quantity is rather larger, and that of muriate of lime rather smaller than what are obtained by the other analyses. But the saline matter was found not to be entirely muriate of soda; its solution became turbid on the addition both of muriate of barytes, and of oxalate of ammonia, indicating the presence of sulphuric acid, and of lime, either in the state of sulphate of lime, retained in solution, or of muriate of lime and sulphate of soda, remaining undecomposed. An excess of sulphate of soda of 0.9 grain, it has already been stated, had been employed, which reduces the weight of the salt to 25.5 grains; and if a little more be subtracted on account of the lime it contained, and be added to the muriate of lime, it will give proportions nearly the same as those before assigned; and the results by this method will thus correspond with those by the others.

Having stated this view of the composition of this water, I have now to consider it under a more general light, and to
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point out a few applications which follow from it, connected with the chemical constitution of waters which contain similar ingredients.

Sulphate of lime has been often stated as an ingredient existing in mineral waters, with muriate of soda, and muriate of lime. It is almost superfluous to remark, that it is probable the original ingredients, in all such cases, are sulphate of soda and muriate of lime, and that the sulphate of lime is a product of the operation, or rather, that the portion of it equivalent to the quantity of muriate of soda, has this origin.

It is a curious fact, which strongly confirms this, that in almost all the analyses of mineral waters since the time of BERGMAN, when they can be presumed to have been executed with any precision, where sulphate of lime is an ingredient, muriate of soda is also present. It is obvious, that, if the sulphate of lime has this origin, muriate of soda must also be formed. On the other hand, in the greater number of those analyses in which muriate of soda is an ingredient, we find also sulphate of lime; and, with the exception of the water of Harrogate, sulphate of lime is always present, where muriate of soda and muriate of lime are conjoined.

But the principal interest belonging to this view, is derived from its relation to a question which has often been brought under discussion,—Whether chemical analysis is capable of discovering the sources of the medicinal virtues of mineral waters? This question some have been disposed to decide in the negative, from finding examples of waters possessed of active powers, in which analysis does not detect any ingredients of adequate activity.

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On the general question, the remark by Dr SAUNDERS is perfectly just, that, "considering the comparative accuracy to which chemists are at present able to carry their inquiries, we can hardly suppose, that whatever slight error might occur in the estimation of minute quantities, the actual existence of any powerful agent on the human body, in any mineral water, should escape the nicety of research." Yet though this is just, and though we can have no hesitation in rejecting the opinion which would ascribe the medicinal qualities of mineral waters to unknown or mysterious causes, or which would deny all power to those in which an active chemical composition cannot be discovered, difficulties on this subject undoubtedly exist, and there is some room for that scepticism which has been extended to this department of the *Materia Medica*.

Of this no better example can be given, than the celebrated Bath water. It has always been found difficult to account for its powers, the ingredients which are obtained in its analysis being substances of little activity, and the principal ones, indeed, being apparently inert. It contains in an English pint, along with a slight impregnation of carbonic acid, about 9 grains of sulphate of lime, 3 grains of muriate of soda, 3 grains of sulphate of soda, $\frac{2}{7}$ ths of a grain of carbonate of lime, $\frac{1}{7}$ th grain of silica, and $\frac{1}{7}$ th grain of oxide of iron. Now, from these ingredients unquestionably no medicinal power of any importance could be expected. They are either substances altogether inert, or are in quantities so minute, as, in the dose in which the water is taken, to be incapable of producing any sensible effect. Some have from this circumstance been disposed to deny altogether any virtues to these waters; but the reverse of this appears to be established by sufficient evidence, and what is still less equivocal, the injurious effects they sometimes produce, and the precautions hence necessary in their use, sufficiently demonstrate their active powers. To account

for these, therefore, various hypotheses have been proposed. The observation has been urged, which, to a certain extent, is undoubtedly just, that substances given in small doses in a state of great dilution, may, from this dilution, produce more effect on the general system, than the quantity given would lead us to expect. The temperature of the water, too, it has been supposed, may have a considerable share in aiding the effect; and these two circumstances in particular, it has been imagined, may favour the action of the iron. This is the view of the subject given by Dr SAUNDERS, in his Treatise on Mineral Waters. Some of the other ingredients, too, it has been supposed, may exert unknown powers. Thus, some effect has been ascribed to the agency of the nitrogen gas which rises through the water. And Dr SAUNDERS himself, apparently not very well satisfied with the reasoning he had employed, allows some weight to the opinion suggested by Dr GIBBES, that the siliceous earth assists in the general effect of the Bath waters;—remarking, that though there is only a grain of it in half a pint of the water, this forms no objection, when the great powers of very minute quantities of active substances are considered; that neither is its insolubility in the animal fluids an objection, as it exists in the water in a state of solution; and that though it has neither taste nor smell, it may be an active substance, since there are indisputably powerful medicines, which have little of either of these qualities.

All this, it is superfluous to observe, is extremely unsatisfactory. With regard to the iron, the only active substance,—allowing full weight to the observations, that small quantities of active medicines, under great dilution, operate with increased power, and that a high temperature may aid their operation on the stomach,—still we cannot believe that one-sixtieth of a grain, the quantity in a pint of this water, can produce any import-

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ant medicinal effect. And with regard to the other substances, the reasoning whence their possible operation has been inferred, instead of removing the difficulty, rather places it in a clearer light.

The view of the constitution of mineral waters stated above, enables us to assign to the Bath water a much more active chemical composition. There is every probability that muriate of lime is its powerful ingredient. The principal products of its analysis are sulphate of lime, muriate of soda, and sulphate of soda. The proportion of sulphate of lime is such, that part of it must pre-exist in the water, but part of it, there is reason to conclude, is a product of the analysis; the muriate of soda is entirely so, and the quantity of sulphate of soda is larger than what the analysis indicates. In other words, there exist in it muriate of lime, sulphate of soda, and sulphate of lime, and during the evaporation, the muriate of lime being acted on by a portion of the sulphate of soda, muriate of soda and a corresponding portion of sulphate of lime are formed.

On the probability of this view, I need not, after the preceding illustrations, offer any observations. The obtaining certain saline compounds from a mineral water by evaporation, leads, no doubt, at first to the conclusion, that they are its ingredients; it is the conclusion, accordingly, which has hitherto been always drawn, and we are disposed to regard this as evidence establishing this conclusion, in some measure, in opposition to any different view of the composition. But this is merely oversight or prejudice. If it can be shewn, that the elements of these compounds may equally exist in the water in a different state of combination, which the evaporation must change, the conclusion that they do exist in such a state is *a priori* as probable, as the conclusion that they exist in the state in which they are actually obtained. It is demonstrable, that if muriate of lime and sulphate of soda exist in a mineral water, or, what

is even less ambiguous, if they be dissolved together in pure water, they must by evaporation be obtained, as muriate of soda and sulphate of lime. The actual obtaining, therefore, of these latter compounds, is no proof that they pre-existed as such in the water, to the exclusion of the opposite view. Which conclusion is to be adopted, must be determined on other grounds; and from the various facts I have stated, I believe it may be regarded as the more probable opinion in such cases, that the original ingredients are sulphate of soda and muriate of lime. Since sulphate of soda exists in the Bath water, and since muriate of soda and sulphate of lime are obtained in its analysis, it is scarcely possible to refuse assenting to the conclusion, that these are formed by the action of sulphate of soda on muriate of lime.

On this view of the composition of the Bath water, it is easy to assign the proportions of the ingredients, from the products which are obtained in its analysis. In the formation of 3.3 grains of muriate of soda, which is the quantity obtained from a pint of the water, 3.1 grains of muriate of lime must be decomposed: 4 grains of sulphate of soda would be required to produce this decomposition; and, at the same time, 3.8 grains of sulphate of lime would be formed.

The latest, and no doubt the most accurate analysis of the Bath water, that by Mr PHILLIPS, gives the following view of its composition:

In an English pint,	Carbonic acid,	-	1.2 inches.
	Sulphate of lime,	-	9 grains.
	Muriate of soda,	-	3.3
	Sulphate of soda,	-	1.5
	Carbonate of lime,	-	0.8
	Silica,	-	0.2
	Oxide of iron,	-	$\frac{1}{8}$ grain.

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But considering the composition according to the preceding view, the ingredients and their proportions will be,

Carbonic acid,	-	1.2 inches.
Sulphate of lime,	-	5.2 grains.
Muriate of lime,	-	3.1
Sulphate of soda,	-	5.5
Carbonate of lime,		0.8
Silica,	-	0.2
Oxide of iron,	-	$\frac{1}{8}$ grain.

The peculiarity in the composition of the Bath water, compared with the greater number of saline mineral waters, is, that it contains a larger quantity of sulphate of soda than is necessary to convert its muriate of lime into sulphate of lime. Hence no muriate of lime is obtained after evaporation in its analysis; hence even a portion of sulphate of soda is indicated; and hence the large proportion of sulphate of lime which that analysis yields. In the Dunblane and Pitcaithly waters, the sulphate of soda is deficient; the muriate of lime is in large quantity, and is accompanied with muriate of soda: hence the entire want of sulphate of soda, the small quantity of sulphate of lime, and the large proportion of muriate of lime in their analyses.

Muriate of lime, it is well known, is a substance of considerable power in its operation on the living system; in quantities which are even not large, it proves fatal to animals. When taken to the extent of six grains, the quantity of it which, according to the preceding view, exists in a quart of the Bath water, it cannot be inactive. It is very probable, too, that a given quantity of it will prove much more active in a state of great dilution in water, than in a less diluted form; as, in this
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diluted state, it acts, when received into the stomach, over a more extended surface ; and, besides this, whatever effect may be due to the high temperature of the Bath water, in aiding the operation of the minute portion of iron it contains, the same effect must be equally obtained in aiding the operation of the much larger quantity of muriate of lime. The conclusion, indeed, as to the importance of this effect, is much more probable with regard to the muriate of lime, than to the iron ; for supposing the quantity of the former to exist in the Bath water, which has been assigned, the dose of it taken in a quart of the water, is not far from its proper medium dose, and is at least equal to one-half the largest dose which can be given, and continued without producing irritation ; while the dose of the iron is not the one-hundredth of that which is usually prescribed. Under the circumstances, therefore, in which the muriate of lime is presented in the Bath water, it is reasonable to infer that it must be productive of considerable immediate effect.

The speculation is farther not improbable, that, to produce its more permanent effects on the system as a tonic, it is necessary it should enter into the circulation. In a dilute state of solution, it may pass more easily through the absorbents ; while, in a more concentrated state, it may be excluded, and its action confined to the bowels. Hence the reason, perhaps, that in some of the diseases in which it is employed, scrofula particularly, it has frequently failed, its exhibition having been in doses too large, and in too concentrated a form. And hence it is conceivable, that in a more dilute state, as that in which it may exist in the Bath water, besides its immediate operation, it may produce effects as a permanent tonic, more important than we should otherwise expect*.

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* I may mention in confirmation of this, that I found a mineral water of considerable celebrity in Yorkshire, that of Ilkley, and which in particular was held

I may add, that the iron in the Bath water is probably not in the state of oxide or carbonate, as has been supposed, but in that of muriate. The muriate is the most active preparation of iron, and so far, increased activity may be given to the slight chalybeate impregnation; and some modification of power may even be derived from the combined operation of muriate of lime and muriate of iron.

It deserves to be remarked, that in the most essential ingredients, the muriate of lime and the iron, the Dunblane and Pitcaithly waters are similar to the Bath water, only with regard to the former ingredient much stronger; the other differences are unimportant; the larger quantity of sulphate of lime, and the small quantity of silica in the latter, cannot be supposed to contribute any thing to its medicinal operation; the difference in the proportion of sulphate of soda is trivial, and the larger proportion of muriate of soda in the other waters, may rather be an advantage, rendering them more agreeable to the taste and to the stomach. The principal difference will therefore be that of strength with regard to the most active ingredient, the muriate of lime. The quantity of this is so large, that the tonic quality of the Dunblane or the Pitcaithly waters can scarcely be observed, and perhaps even scarcely obtained; their action being more peculiarly on the bowels. It is accordingly as a saline purgative that the Pitcaithly water has been celebrated; and it is principally in those diseases in which this effect is sought to be obtained, that it has been used.

held in high estimation as a remedy in scrofulous affections by several eminent medical practitioners, to be water uncommonly free from all foreign matter, with the exception of very minute quantities of muriate of soda, and muriate of lime. I had the opportunity of observing, at the same time, proofs of its medicinal efficacy.

used. The Dunblane water, from the similarity of its operation, would no doubt be employed in diseases of a similar kind. But whatever advantage might be derived from this purgative effect, it cannot fail to be perceived, that a different operation, not less useful, may be obtained from them. If sufficiently diluted, so as to avoid altogether the operation on the bowels, the stimulant operation on the stomach and general system might be exerted by these waters, similar to that of the Bath waters, and under this form they might prove useful in diseases very different from those in which they might otherwise be employed. As they would require, too, large dilution to reduce them to this state, the temperature of the Bath water might easily be given, by adding the requisite proportion of hot water, by which a greater similarity of operation would be obtained. And the Dunblane water in particular, containing so much larger a proportion of iron than the Bath water does, the dilution requisite to give it the same strength, with regard to the muriate of lime, would still leave an equal degree of chalybeate impregnation. If the preceding observations, therefore, are just, the Dunblane and Pitcaithly waters may be converted, in all the essential parts of the chemical composition, into a water similar to that of Bath.

From the preceding statement of their composition, it is easy to discover how this may be done. To give the same proportion of the principal ingredient, the muriate of lime, the Dunblane water would require to be diluted with from six to seven parts of pure water; the same degree of dilution would bring it to nearly the same strength with regard to the iron; if a pint of it were diluted with this portion of water, about 35 grains of sulphate of soda would require to be added, to render the composition, with regard to this ingredient, perfectly alike, if this were thought essential. The only remaining differences

ferences would then be, the presence of about 2.8 grains of muriate of soda in each pint of the reduced Dunblane water, the deficiency of 5.5 grains of sulphate, and 0.7 grain of carbonate of lime, and the absence of 0.2 grain of siliceous earth, differences in all respects probably of no importance whatever. The simple expedient, indeed, of diluting one part of the Dunblane water with from six to seven parts of warm water, (or if the sulphate of lime in a state of solution should be supposed to be possessed of any active power, with four or five parts) and adding, if the chalybeate impregnation were not found sufficiently active, a few drops of tincture of muriate of iron, would probably serve every purpose. And if sufficient confidence could be given to the substitution on the part of those employing these waters medicinally, the Dunblane water, thus altered, might probably be taken with as much advantage as the Bath water in the diseases in which it has been found useful.

It is obvious, too, that if the artificial preparation of the Bath water were attempted, it could be done much more easily according to this view, than by endeavouring to dissolve the actual products of its analysis, which, indeed, it would be impracticable to do. Muriate of lime, and sulphate of soda, dissolved in water of the due temperature, with the addition of a minute portion of muriate of iron, would probably afford a composition approaching as nearly to the natural composition, as is either practicable or necessary in the imitation of any mineral water.

A similar view may be taken of the composition of Cheltenham water. Its analysis affords sulphate of soda, sulphate of magnesia, and sulphate of lime, with muriate of soda, muriate of magnesia, carbonate of magnesia, and oxide of iron. There is no just reason, however, to infer with certainty, that all these are its real ingredients. It is as probable, and, indeed,

more so, that, previous to the evaporation by which they are obtained, it contains muriate of lime, which being acted on by the sulphate of soda, forms muriate of soda, and sulphate of lime. It is even not improbable, that the carbonate naturally existing in the water, is not carbonate of magnesia, but carbonate of soda, which, re-acting, from the concentration by the evaporation, on sulphate or muriate of magnesia, causes the production of the carbonate of magnesia with a corresponding portion of sulphate or muriate of soda; or, what is equally probable, and presents the same ultimate results, the sulphate of magnesia may, in the progress of the evaporation, be first acted on, by the carbonate of soda, forming carbonate of magnesia and sulphate of soda; and the sulphate of soda, during the farther concentration, may act on the muriate of lime, and form muriate of soda, and sulphate of lime. It is much more probable, indeed, from the known insolubility of carbonate of magnesia, that it is produced in this way, than that it should exist in a state of solution in so large a quantity, as that in which it is afforded by the evaporation. And thus this water will present a striking example, that the real ingredients of a mineral water, and their proportions, may be very different from those obtained by the direct analysis; for it is too obvious, after the preceding observations, to require illustration, that the actual production of certain ingredients by evaporation, or any other analytic process, is no certain proof that they pre-existed in the water. It is obvious, too, that if it were proposed to imitate the Cheltenham water by artificial preparation, it could be done much more easily according to this view, than by attempting to dissolve the ingredients obtained by the analysis, an attempt, indeed, which would not succeed. The Dunblane or Fitcaithly water might be converted, so far as regards the saline ingredients, into a water similar to that of
Cheltenham,

Cheltenham, by the addition of a little sulphate of magnesia, or more nearly, by the addition of a little of the bitter of sea water; and where in the use of these waters, a continued purgative operation is required, such an addition might always be made with advantage. They might even be made to receive the impregnation of carbonic acid of the Cheltenham water, by adding the magnesia in the state of carbonate, with the due proportions of sulphuric and muriatic acids in a close vessel.

The water of Harrowgate affords in its saline ingredients another illustration of the same views. The principal ingredient is muriate of soda, with which are present muriate of magnesia, muriate of lime, sulphate of magnesia, carbonate of magnesia, and carbonate of lime. Now nothing is more probable, than that the two last substances are not original ingredients, but are products of the analysis formed by the action of carbonate of soda existing in the water on portions of its muriate of magnesia and muriate of lime, whence also the quantity of muriate of soda is increased.

Lastly, A similar view may be extended to some of the most celebrated foreign mineral springs. Those of Spa, Pyrmont, and Seltzer form a very valuable order of mineral waters, to which we have none analogous in this country;—what have been called the alkaline carbonated waters, distinguished by the leading character of being largely impregnated with carbonic acid gas, and containing a considerable proportion of carbonate of soda. With this are associated carbonate of magnesia, carbonate of lime, and muriate of soda. Now this association of muriate of soda with these earthy carbonates, while there is also carbonate of soda present, leads almost necessarily to the belief, that the real ingredients are carbonate of soda, muriate of magnesia, and muriate of lime; that the carbonate of soda

is in larger proportion than what is indicated by the analysis; that it acts during the evaporation of the water on the muriates of magnesia and lime, and forms the carbonates of these earths which are obtained with corresponding portions of muriate of soda; and that it is only what muriate of soda there may be above this, that exists as an original ingredient.

The Seltzer water, which is the purest of this order of waters, as containing neither iron, nor any sulphate, affords in particular a very excellent illustration of this. It contains, according to BERGMAN'S analysis, in an English pint,

Carbonic acid gas,	-	17 cubic inches.
Carbonate of lime,	-	3 grains.
Carbonate of magnesia,	5	—
Carbonate of soda,	-	4 —
Muriate of soda,	-	17.5 —

But adopting the opposite view, the composition, so far as the uncertainty of the state of the products, to which BERGMAN'S estimate is referred, admits of calculating the proportions, will be,

Carbonic acid gas,	-	17 cubic inches.
Muriate of lime,	-	3.3 grains.
Muriate of magnesia,	-	5 —
Muriate of soda,	-	7.8 —
Carbonate of soda,	-	10.3 — dry, or 18 crystallised*.

It

* The following is the calculation from which these proportions are assigned. Three grains of carbonate of lime are equivalent to 3.3 of real muriate of lime: 5 grains of carbonate of magnesia, in the state in which it was obtained by BERGMAN, that is, the powder precipitated and dried, are equivalent to 5 grains of

It might be supposed, that so large a proportion of carbonate of soda, could not exist with the muriates of magnesia and lime without decomposing them; that this view of the constitution of this water is therefore precluded; and that BERGMAN'S is just. And, in this case, the non-precipitation of the carbonates of magnesia and lime, may be supposed to be owing to the solvent power of the excess of carbonic acid; to which cause, accordingly, it has been ascribed. But on making the experiment, I found that the above quantities might be dissolved in a pint of water, independent of the presence of the excess of carbonic acid, without any apparent decomposition; the solution being transparent, and remaining so on exposure to the air. The same fact has even been observed with regard to the natural water; for although on exposure to the air it becomes vapid, and its taste is merely sensibly alkaline, the carbonates are not precipitated; the precipitation takes place only when heat is applied, so as to evaporate the water to a certain extent. And with regard to this, a fact is mentioned by BERGMAN not less conclusive. The carbonate of lime is first deposited, with scarcely any mixture of carbonate of magnesia; the latter separates only by continued evaporation; and it is even necessary to evaporate to dryness, and redissolve in hot water, to obtain it entirely,—proving that it does

of real muriate of magnesia. In converting the first of these muriates into carbonate, 3.2 grains of dry common carbonate, or sub-carbonate of soda, would be expended; and in the conversion of the second muriate, 5.7 grains, making 8.9 grains, to which are to be added 1.4 grain, the quantity contained in the 4 grains of the crystallised carbonate obtained as the direct product of the analysis, making in all, as stated above, 10.3 grains. Lastly, in these decompositions of the earthy muriates, 9.7 grains of muriate of soda would be formed, which, deducted from the 17.5 obtained in the analysis, leaves 7.8 as the quantity which the water really contains.

does not pre-exist in the water, dissolved by an excess of carbonic acid, but that it is produced during the evaporation, and must therefore be formed by the action of carbonate of soda on muriate of magnesia.

This view of the composition of this water, accords much better than the other, both with its sensible qualities, and its medicinal powers. Its taste, after the carbonic acid has escaped from it, on exposure to the air, is rather strongly alkaline, which would scarcely be the case, if it contained only four grains of crystallised carbonate of soda in a pint, but which is to be expected if it contain eighteen grains. It operates as an antacid and diuretic, and is productive of much benefit in all dyspeptic affections, in diseases of the urinary organs, and in those general affections of the system which require a mild tonic power. There are few mineral waters, Dr SAUNDERS observes, which have acquired a higher reputation; and there are few, he adds, that deserve greater consideration, from the real medicinal virtues it possesses. It will be difficult to give a satisfactory account of the origin of these virtues, if we regard it as water impregnated with carbonic acid, holding in solution so minute a portion of carbonate of soda, with the larger proportions of muriate of soda and carbonates of magnesia and lime. But if we consider it as containing along with its free carbonic acid, a considerable quantity of carbonate of soda, with smaller proportions of muriate of soda, muriate of magnesia, and muriate of lime, we assign to it a composition of much greater power, and adequate to account for the effects it produces. Such is the activity of this water, that its medium dose is only half an English pint, a degree of power which accords much better with the one view of its composition than with the other*.

Large

* The water of Malvern may be regarded as of similar composition, only
much

Large quantities of Seltzer water have been imported into this country, and artificial preparations of it are in frequent use. If these are founded on BERGMAN'S view of its composition, they can scarcely succeed; probably, therefore, this is not attempted. The view which I have suggested, renders its artificial preparation much more easy. The ingredients may be dissolved in water, and the solution impregnated with carbonic acid gas. Or, what is easier, these steps of the process may be conjoined. The muriate of lime may be formed by adding the requisite quantity of carbonate of lime to the due proportion of muriatic acid diffused in water, and the vessel being closed, the escape of the carbonic acid gas may be prevented. The muriate of magnesia, and the muriate of soda, may be formed in a similar manner from the carbonates of magnesia and soda. And the quantity of carbonic acid thus afforded, will be very nearly that which is required. To form the muriate of lime, 3 grains of carbonate are to be used; to form the muriate of magnesia, 5 grains of the carbonate of that earth; and to form the 7.8 grains of muriate of soda, 12.3 grains:

much weaker, and without any free carbonic acid. Dr WILSON'S analysis gives the following ingredients, and their proportions in a gallon:

Carbonate of soda,	-	5.33 grains.
———— lime,	-	1.6
———— magnesia,	-	0.9199
———— iron,	-	0.625
Sulphate of soda,	-	2.896
Muriate of soda,	-	1.553
Residuum,	-	1.687

The muriate of soda, there is every probability, is a product of the operation, formed by the action of carbonate of soda on muriate of lime; or if sulphate of lime formed part of the residuum, as is probable, by the action of sulphate of soda on muriate of lime.

grains of crystallised carbonate of soda. These quantities contain 6.2 grains of carbonic acid, or 13 cubic inches, a quantity not much beneath that which the Seltzer water contains. The neutral carbonate of soda, or bi-carbonate, as it is named, may even be substituted in the preparation; and if the due proportion of this be used. (11 grains), it will yield 6 cubic inches additional, making the whole quantity 19 cubic inches, 2 more than the quantity in the water*.

I might apply the same view to a number of other analyses of mineral waters, even the most recent. But though this would not be altogether uninteresting, it is scarcely necessary to extend the illustration farther. The general conclusion may, I believe, be drawn, that in the analysis of saline mineral waters, the actual products of the analytic operation are not always to be regarded as the real ingredients. A different view of the composition is often to be taken, and may in many cases be applied, so as to afford a more satisfactory solution of their active powers.

I may only farther remark, that a view somewhat different may also be applied, founded on the doctrine, that the primary ingredients of the compound salts, obtained by the analysis of mineral waters, are in simultaneous combination, and not in
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* The following is the easiest method of conducting the process. About 35 grains of muriatic acid, of the strength usually met with in the shops, are put into a strong bottle with a pint of water; the acid being introduced at the bottom of the water by a long funnel. Three grains of pure white marble, in coarse powder, are dropt in, and the bottle is closed. When these are dissolved, 5 grains of the common carbonate of magnesia in powder are added, and after the solution of this, 32 grains of crystallised carbonate of soda, or, what is equivalent to this, and preferable, as affording more carbonic acid, 27 grains of bi-carbonate of soda, are put in. The bottle is closed accurately, shaken, and inverted. In a short time a perfect solution takes place, and a liquor is obtained transparent, which sparkles when poured out, and has a pleasant taste.

the state of binary compounds. Even this view, were it adopted, would afford a better explanation of their active powers, than the view of their composition which is usually received, since it could not at least be affirmed, that such a combination must be inactive. The opinion itself, however, is much less probable; for if fairly followed out, it leads to the conclusion, that all combinations of compound bodies are simultaneous combinations of the primary elements,—a conclusion from which no inference with regard to specific qualities could be drawn, and which is inconsistent, therefore, with the conclusions which in many cases we are able actually to form. We are led, therefore, to the admission, that the state of binary combinations exists; and it is only necessary to guard against the error of supposing that the products of the analysis are always the original ingredients.

The importance of the subject, and its relation to the question, how far chemical analysis is capable of accounting for the medicinal efficacy of mineral waters, will, I hope, afford an apology for the introduction of some of the preceding observations, though they may not fall strictly under the objects usually submitted to the Society.

In a succeeding paper, I shall have to offer some remarks on the analysis of Sea-water, and salt brines, suggested by the view which I have explained in this. And the same view may perhaps lead to the illustration of a geological problem, hitherto involved in considerable difficulty, the origin of Rock Salt, and the relation of this mineral to the saline impregnation of the ocean.

XVII. *Biographical Account of the late JOHN ROBISON, LL.D.*
F. R. S. EDIN. and *Professor of Natural Philosophy*
in the University of Edinburgh. By JOHN PLAYFAIR,
F. R. S. L. & E. &c.

(*Read 20th February 1815.*)

THE distinguished person who is the subject of this memoir, was born at Boghall, in the parish of Baldernock, near Glasgow, in the year 1739. His father, JOHN ROBISON, had been early engaged in commerce in Glasgow, where, with a character of great probity and worth, he had acquired considerable wealth, and, before the birth of his son, had retired to the country, and lived at his estate of Boghall.

His son was educated at the grammar school of Glasgow. We have no accounts of his earliest acquirements, but must suppose them to have been sufficiently rapid, as he entered a student of Humanity, in the University of Glasgow, in November 1750, and in April 1756 took his degree in Arts.

Several Professors of great celebrity adorned that University about this period. Dr SIMSON was one of the first geometers of the age; and Mr ADAM SMITH had just begun to explain in his lectures those principles which have since been de-

livered with such effect in the Theory of Moral Sentiments, and in the Wealth of Nations. Dr MOORE was a great master of the Greek language, and added to extensive learning a knowledge of the ancient geometry, much beyond the acquirement of an ordinary scholar.

Under such instructors, a young man of far inferior talents to those which Mr ROBISON possessed, could not fail to make great advancement. He used, nevertheless, to speak lightly of his early proficiency, and to accuse himself of want of application, but from what I have learnt, his abilities and attainments were highly respected by his cotemporaries, and he was remarked at a very early period for the ingenuity of his reasonings, as well as the boldness of his opinions. According to his own account, his taste for the accurate sciences was not much excited by the pure Mathematics, and he only began to attend to them, after he discovered their use in Natural Philosophy.

In the year following that in which he took his degree, Dr DICK, who was joint Professor of Natural Philosophy with his father, died, and Mr ROBISON offered himself to the old gentleman as a temporary assistant. He was recommended, as I have been told, by Mr SMITH, but was nevertheless judged too young by Mr DICK, as he was not yet nineteen. The object to which his father, a man of exemplary piety, wished to direct his future prospects, was the Church, to which, however, he was at this time greatly averse, from motives which do not appear; but certainly not from any dislike to the objects or duties of the Clerical Profession. It was very natural for him to wish for some active scene, where his turn for Physical, and particularly Mechanical Science, might be exercised, and the influence of those indefinite and untried objects, which act so powerfully on the imagination
of

of youth, directed his attention toward London. Professor DICK and Dr SIMSON joined in recommending him to Dr BLAIR, Prebendary of Westminster, who was then in search of a person to go to sea with EDWARD, Duke of York, and to assist his Royal Highness in the study of Mathematics and Navigation. When Mr ROBISON reached London in 1758, he learnt that the proposed voyage was by no means fixed, and after passing some time in expectation and anxiety, he found that the arrangement was entirely abandoned. This first disappointment in a favourite object could not fail to be severely felt, and had almost made him resolve on returning to Scotland.

He had been introduced, however, to Admiral KNOWLES, whose son was to have accompanied the Duke of YORK, and the Admiral was too conversant with Nautical Science, not to discover in him a genius strongly directed to the same objects. Though the scheme of the Prince's nautical education was abandoned, the Admiral's views with respect to his son remained unaltered, and he engaged Mr ROBISON to go to sea with him, and to take charge of his instruction. From this point it is, that we are to date his nautical as well as scientific attainments.

About the middle of February 1759, a fleet sailed from Spithead under the command of Admiral SAUNDERS, intended to co-operate with a military force which was to be employed, during the ensuing summer, in the reduction of Quebec. Young KNOWLES, whom Mr ROBISON had agreed to accompany, was a midshipman on board the Admiral's ship, the *Nep-tune* of 90 guns; but in the course of the voyage, being promoted to the rank of Lieutenant in the *Royal William* of 80 guns, Mr ROBISON went with him on board that ship, and was there rated as a midshipman.

The

The fleet arrived on the coast of America in April; but it was not till the beginning of May that the entire dissolution of the ice permitted it to ascend the River St Lawrence, and that the active scene of naval and military operations commenced, which terminated so much to the credit of the British arms. A person whose seafaring life was to be limited to two years, may well be considered as fortunate, in witnessing, during that short period, a series of events so remarkable as those which preceded and followed the taking of Quebec. Though great armies were not engaged, much valour and conduct were displayed; the leaders on both sides were men of spirit and talents; and, on the part of the English, the most cordial co-operation of the sea and land forces was worthy of men animated by the spirit of patriotism, or the love of glory; the fate also of the gallant Leader, who fell in the moment of victory, and in the prime of life, by repressing the exultation of success, gave a deeper interest to the whole transaction.

Of the operations of this period Mr ROBISON was by no means a mere spectator. A hundred seamen, under the command of Lieutenant KNOWLES, were drafted from the Royal William into the Stirling Castle, the Admiral's ship. Mr ROBISON was of this party, and had an opportunity of seeing a great deal of active service. At this time, also, he was occasionally employed in making surveys of the river and the adjacent grounds; a duty for which he was eminently qualified, both by his skill as a mathematician, and his execution as a draughtsman.

It is, however, much to be regretted, that his papers, whether memorandums or letters, give no account of the incidents of this period; so that we are left to conclude, from the history of the times, what were the events in which he must have taken,

ken part, or to gather, from the imperfect recollection of his conversation, the scenes in which he was actually engaged. I have heard him express great admiration at the cool intrepidity which he witnessed, when the fire-ships, sent down the stream against the English navy, at anchor in the river, seemed to present a wall of fire, extending from one bank to another, from which nothing that floated on the water could possibly escape. Without the smallest alarm or confusion, the British sailors assailed this flaming battlement in their boats, grappled the ships which composed it, and towed them to the shore, where they burnt down quietly to the water's edge.

An anecdote which he also used to tell, deserves well to be remembered. He happened to be on duty in the boat in which General WOLFE went to visit some of his posts, the night before the battle, which was expected to be decisive of the fate of the campaign. The evening was fine, and the scene, considering the work they were engaged in, and the morning to which they were looking forward, sufficiently impressive. As they rowed along, the General, with much feeling, repeated nearly the whole of GRAY'S ELEGY, (which had appeared not long before, and was yet but little known,) to an officer who sat with him in the stern of the boat; adding, as he concluded, that "he would prefer being the author of that poem to the "glory of beating the French to-morrow."

To-morrow came, and the life of this illustrious soldier was terminated, amid the tears of his friends, and the shouts of his victorious army. Quebec fell of course; and soon afterwards the fleet under Admiral SAUNDERS, sailed for England. When they arrived on the coast, they were informed that the Brest fleet was at sea, and that Sir EDWARD HAWKE was in search of it. Without waiting for orders, Admiral SAUNDERS sailed to reinforce HAWKE, but came too late, the celebrated victory
over

over CONFLANS, in Quiberon Bay, having been obtained (on the 20th of November) a few days before he joined. Whether the Royal William accompanied the rest of the fleet on this occasion, I have not been able to learn. The body of General WOLFE was brought home in that ship, and was landed at Spithead, on the 18th of November. From that date to the beginning of next year, I find nothing concerning the Royal William, when that ship, with the Namur and some others, under the command of Admiral BOSCAWEN, sailed on an expedition to the Bay of Quiberon. On this service the Royal William remained between five and six months, having been twice sent to cruise off Cape Finisterre, for five weeks each time.

About this period, a series of letters from Mr ROBISON to his father begins; and though the letters do not enter much into particulars, they leave us less at a loss about the remaining part of his seafaring life.

On the 3d of August the Royal William returned to Plymouth, the greater part of the crew being totally disabled by the sea-scurvy, from which Mr ROBISON himself had suffered very severely. He writes to his father, that, out of seven hundred and fifty able seamen, two hundred and eighty-six were confined to their hammocks, in the most deplorable state of sickness and debility, while one hundred and forty of the rest were unable to do more than walk on deck. This circumstance strongly marks, to us, who have lately witnessed the exertions of British sailors, in the blockade of Brest, and other ports of the enemy, the improvement made in the art of preserving the health of seamen within the last fifty years. The Royal William, notwithstanding the state of extreme distress to which her crew was reduced, by a continuance at sea, of hardly six months, was under the command of Captain HUGH

PIGOTT,

FIGOTT, one of the most skilful officers of the British navy. Mr ROBISON, indeed, never at any time mentioned his name without praise, for his knowledge of seamanship, and the address with which he used to work the ship, in such bad weather, as rendered her almost unmanageable to the other officers. The art of preserving the health of the seaman, is a branch of nautical science which had at that time been little cultivated. Our great Circumnavigator had not yet shewn, that a ship's crew may sail round the globe, with less mortality than was to be expected in the same number of men, living for an equal period in the most healthful village of their native country.

Mr ROBISON's letters to his father, about this time, are strongly expressive of his dislike to the sea; and of his resolution to return to Glasgow, and to resume his studies, particularly that of Theology, with a view of entering into the Church. These resolutions, however, were for the present suspended, by a very kind invitation from Admiral KNOWLES, to come and live with him in the country, and to assist him in his experiments: "Thus, (says the Admiral), we shall be useful to one another." What these experiments were, is not mentioned, but they probably related to ship-building, a subject which the Admiral had studied with great attention. Mr ROBISON, accordingly, continued to enjoy a situation, and an employment, that must, both, have been extremely agreeable to him, till the month of February in the year following, when Lieutenant KNOWLES was appointed to the command of the Peregrine sloop of war, of 20 guns. Whether the plan of nautical instruction, which Mr ROBISON proposed for his pupil, was not yet completed, or whether he had, after all, come to a resolution of pursuing a seafaring life, (of which there is an appearance in some of his letters), he embarked in the Peregrine,

grine, and he even mentions his hopes of being made purser to that ship. The first service in which Captain KNOWLES was employed, was to convoy the fleet to Lisbon. In a letter from Plymouth, where they were forced in by the weather, Mr ROBISON paints, in strong colours, the difference between sailing in a small ship, like the Peregrine, and a first rate, like the Royal William, and the uncomfortable situation of all on board, during a gale which they had experienced in coming down the Channel. The voyage, however, gave him an opportunity of visiting Lisbon, on which the traces of the Earthquake were yet deeply imprinted; and the ship continuing to cruise off the coast of Spain and Portugal, he had occasion to land at Oporto, and other places on the Portuguese coast. In the month of June he returned to England; and from this time quitted the navy, though he did not give up hopes of preferment. He returned, to live with Admiral KNOWLES, and in the end of the same summer, was recommended by him to Lord ANSON, the First Lord of the Admiralty, as a proper person to take charge of HARRISON's Time-keeper, which, at the desire of the Board of Longitude, was to be sent, on a trial-voyage, to the West Indies.

The ingenious artist just named, had begun the construction of his chronometer, on new principles, as early as the year 1726, and with the fortitude and patience characteristic of genius, had for thirty-five years struggled against the physical difficulties of his undertaking, and the still more discouraging obstacles which the prejudice, the envy, or the indifference of his cotemporaries, seldom fail to plant in the way of an inventor. Notwithstanding all these, he had advanced constantly from one degree of perfection to another, and it was his fourth time-keeper, reduced to a portable size, and improved in all
other

other respects, that was now submitted to examination. It was intended that Mr ROBISON should accompany young HARRISON and the time-keeper, in a frigate, the Deptford, to Port Royal in Jamaica, in order to determine, on their landing, the difference of time, as given by the watch, and as found by astronomical observation. The time-keeper, accordingly, was put into the hands of Mr ROBERTSON, of the Naval School at Portsmouth, who determined its rate, from nine days that it remained in his custody, to be $2\frac{2}{3}$ sec. slow, per day, and also, the error to be 3 sec. slow, on the 6th of November, at noon, according to mean solar time.

The Deptford sailed on the 18th of November, and arrived at Port Royal on the 19th of January; on the 26th, Mr ROBISON observed the time of noon, and found it to answer to $4^h. 59^m. 7\frac{1}{2}$ sec. by the watch, and this being corrected for the error of three seconds, and also for the daily accumulation of $2\frac{2}{3}$ sec. for eighty-one days five hours, the interval between the observations, the difference of longitude between Portsmouth and Port Royal came out $5^h. 2^m. 47$ sec.; only four seconds less than it was known to be from other observations.

The instructions of the Board farther required, that, as soon as an opportunity could be found, the same two gentlemen should return with the watch to Portsmouth, that, by a comparison of it with the time there, the total error, during both voyages, might be ascertained. The opportunity of return occurred sooner than they had any reason to expect; for the Spanish war having now broke out, an alarm of an invasion of Jamaica from St Domingo, occasioned the Governor to dispatch the Merlin sloop of war to England, to give intelligence of the danger. Mr ROBISON and Mr HARRISON obtained leave to return in the Merlin, and sailed on the 28th, having been but a few days in Jamaica. This voyage was an

epitome of all the disasters, short of shipwreck, to which seafaring men are exposed. They experienced a continuation of the most tempestuous weather, and the most contrary winds, from the moment they quitted the Bahamas, till they arrived at Spithead. To add to their distress, the ship sprung a great leak, three hundred leagues from any land, and it required the utmost skill and exertion to keep her from sinking. In a terrible gale, on the 14th of March, their rudder broke in two, so that they could no longer keep the ship's head to the wind, and if the gale had not speedily moderated, they must inevitably have perished. When the voyage was near a conclusion, and they were congratulating themselves on the end of their troubles, the ship was found to be on fire, and the flames were extinguished with great difficulty. They reached Portsmouth on the 26th of March, and on the 2d of April the mean noon by the watch was found to be at $11^{\text{h}} 51^{\text{m}} 31\frac{1}{2}^{\text{sec}}$, and, making correction for the error and rate, this amounted to $11^{\text{h}} 58^{\text{m}} 6\frac{1}{2}^{\text{sec}}$, so that the whole error, from the first setting sail, was only $1^{\text{m}} 53\frac{1}{2}^{\text{sec}}$, which, in the latitude of Portsmouth, would not amount to an error, in distance, of twenty miles.

When Mr ROBISON undertook the voyage to Jamaica, he made no stipulation for any remuneration, and Lord ANSON assured him, that he should have no reason to repent the confidence which he placed in the Board. But when, on his return, he came to look for the reward, to which the success and trouble of the undertaking certainly entitled him, he soon found that he had greatly erred, in leaving himself so much at the mercy of unforeseen contingencies. Lord ANSON was ill of the disease of which he died, and was not in a condition to attend to business. Admiral KNOWLES was disgusted with the Admiralty, and with the Ministry, by which he thought himself ill-used; so that Mr ROBISON had nothing to look for from
personal

personal kindness, and could trust only to the justice and moderation of his claims. These were of little advantage to him; for such was the inattention of the Lords of the Admiralty, and the Members of the Board of Longitude, that he could not obtain access to any of them, nor even receive from them any answer to his memorials.

The picture which his letters to his father present, at this time, is that of a mind suffering severely from unworthy treatment, where it was least suspected. Men in office do not reflect, while they are busy about the concerns of nations, how much evil may be done by their neglect to do justice to an individual. They may be extinguishing the fire of genius, thrusting down merit below the level it should rise to, or prematurely surrounding the mind of a young man, with a fence of suspicion and distrust, worse than the evils which it proposes to avert. Like other kinds of injustice, this may, however, meet with its punishment; though the victim of unmerited neglect may remain for ever obscure, and his sufferings for ever unknown, he may also emerge from obscurity, and the treatment he has met with may meet the eye of the public. It is probable that the member of these Boards most conspicuous for rank or for science, would not have been above some feeling of regret, if he had learnt that the young man whose petitions he disregarded, was to become the ornament of his country, and the ill treatment he then met with, a material fact in the history of his life.

But though we must condemn the neglect of which Mr ROBISON had so much reason to complain, we do by no means regret that the recompense, which he or his friends had in view, was not actually conferred on him. This was no other than an appointment to the place of a purser in a ship of war; a sort of preferment which, to a man of the genius, information,
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and accomplishment of Mr ROBISON, must have turned out rather as a punishment than a reward. It was, however, the object which, by the advice of Sir CHARLES KNOWLES, he now aspired to ; and, indeed, he had done so, ever after his first voyage in the Royal William ; for it appears that he had wished to be made Purser to the Peregrine, at the time when Lieutenant KNOWLES was appointed to the command of that ship, though, considering its smallness, the situation could have been attended with little emolument*.

Thus disappointed in his hopes, Mr ROBISON resolved on returning to Glasgow, in order to qualify himself for entering into the Church. Indeed, the idea of prosecuting his original destination seems often to have occurred to him, even when his views appeared to have a very different direction. When he left the Royal William in 1761, he was not without serious intentions of resuming the study of Theology. This appears, both from a letter he wrote to his father, about that time, and from one which he himself received from young KNOWLES, who rallies him on his new profession, and on the singularity of having acquired a taste for theological studies in the ward-room of a man of war. When he undertook the voyage to Jamaica, he would have wished to have had the patronage of his employers, for obtaining some ecclesiastical preferment rather than

* It is, however, true, that the place of Purser was afterwards offered to Mr ROBISON, but such a one as he could have no temptation to accept. In 1763, when Lord SANDWICH was First Lord of the Admiralty, his solicitations were so far listened to, that he was appointed to the Aurora, of 40 guns, then on the stocks. As the ship must be long of being in commission, and the pay of the Purser, in the mean time, very inconsiderable, Mr ROBISON declined accepting this appointment.

than naval; and only agreed to the latter, as it lay more in the way of the Board of Longitude to help one to promotion in the Navy than in the Church. It appears, that he had never ceased to express to Dr BLAIR a desire of assuming the clerical character; and he actually had, from that gentleman, the offer of a curacy in a living of his own, to which, however, the emolument annexed was so small, that, after consultation with his father, he declined accepting of it.

But however Mr ROBISON's views may have varied, to one object he steadily adhered, viz. the cultivation of science, and the acquisition of whatever knowledge the situations he was placed in brought within his reach.

He returned, therefore, to Glasgow; and a man whose object was the prosecution of science, could not arrive at any place in a more auspicious moment, as that city was about to give birth to two of the greatest improvements, which, in the eighteenth century, have distinguished the progress either of the sciences or the arts. The one of these was the discovery of Latent Heat, by the late Dr BLACK; the other, was the invention of what may be properly called a New Steam-engine, by Mr WATT. The former of these eminent men was then the Lecturer on Chemistry in the University, and had just been led, by a train of most ingeniously contrived experiments, to the knowledge of a principle which seemed to promise better for an explanation of the process which takes place when heat is communicated to bodies, than any thing yet known in chemistry, viz. that when water passes from a solid to a fluid state, as much of its heat disappears, as would have raised its temperature, had it remained solid, 140 degrees higher than that which it actually possesses. Mr ROBISON was already known to Dr BLACK, having been introduced to him

him before he left Glasgow; but at that time he had not studied chemistry, to which, however, he was now bending his attention. He had the advantage of being initiated in it by the author of the discovery just mentioned, and the new views struck out by his master, did not fail to interest him in a study, which, from that time, came to occupy a new place in physical science.

Mechanics had always been his favourite pursuit, and his turn to whatsoever was connected with it, had brought him to be acquainted with Mr WATT before 1758, when he left the University. Mr WATT, who, at that time, exercised the profession of a mathematical instrument maker, was employed in fitting up the astronomical instruments bequeathed to the Observatory by the late Dr MACFARLANE of Jamaica. Mr ROBISON, on his return, found him still residing in Glasgow, and exercising the same profession, and their former intimacy was naturally renewed. In 1764, an occurrence such as to an ordinary man would have been of no value, gave rise to the improvement of the steam-engine. A model of the common engine, NEWCOMEN'S, which belonged to the Natural Philosophy Class, was put into Mr WATT'S hands in order to be repaired. As the model worked faster than the large engines, it was found impossible to supply it with steam, and it was in the attempt to obviate this difficulty, and in trying to produce a more perfect vacuum, that the idea of condensing the steam in a separate vessel first occurred to him. At the same time, by a curious coincidence, his experiments led him to conclusions concerning the great quantity of heat contained in steam, that were only to be explained on the principle of latent heat. Mr ROBISON lived in a state of great intimacy with Mr WATT, and was so much acquainted with the first steps of this invention, that his evidence on the subject of the originality

originality of it, was afterwards of great use in ascertaining the justness of his claim.

There could not be a better school for philosophical invention than Mr ROBISON enjoyed at this time, and accordingly, he used always to say, that it was not till his second residence at Glasgow that he applied to study with his whole mind.

Dr BLACK was elected Professor of Chemistry in the University of Edinburgh in the summer of 1766; and, on leaving Glasgow, recommended Mr ROBISON as his successor. He was accordingly made choice of, and began his first course of chemical lectures in October 1766. He was appointed for one year only, but his success assured his continuance without any other limit than such as depended on himself.

He had also the charge of the education of the late Mr MACDOWAL of Garthland, and of Mr CHARLES KNOWLES, a son of the Admiral. But of the particulars, during four years, about this time, I have been able to obtain little information.

The friendship of Admiral KNOWLES had been all along exerted toward Mr ROBISON, with an extraordinary degree of zeal and assiduity, and was now the means of procuring for him a very unlooked-for preferment, which removed him from his academical duties at Glasgow. The Empress of Russia, convinced of the importance of placing her marine on the best footing, made an application to the Government of this country, for permission to engage in her service some of the most able and experienced of our naval officers, to whom she might entrust both the contrivance and the execution of the intended reformation. The request was agreed to, and the person recommended was Admiral Sir CHARLES KNOWLES, who had long applied, with great diligence, to the

study of naval architecture, as well as to that of every branch of his profession; and who, about fifty years before, had been sent to Portugal on a similar mission. A proceeding so free from that jealousy which often marks the conduct of great nations no less than the dealings of the most obscure corporations, is particularly deserving of praise. From the first moment that this offer was made to the Admiral, he communicated it to Mr ROBISON, whom he wished to engage as his Secretary, and to whom, as he says in his letters, he looked for much assistance in the duty he was about to undertake. A very handsome appointment was made for Mr ROBISON, and in the end of December 1770, he set out with Sir CHARLES and his family on the journey to St Petersburg, over land.

Admiral KNOWLES held the office of President of the Board of Admiralty; and his intention was, that Mr ROBISON should have the place of Secretary. The Russian Board, however, being constituted more on the plan of the French than the English, there was no place corresponding to that of our Secretary of the Admiralty. Mr ROBISON continued, therefore, in the character of Private Secretary to the Admiral.

During the first year of the Admiral's residence in Russia, and for the greater part of the second, Mr ROBISON remained with him, employed in forming and digesting a plan for improving the methods of building, rigging, and navigating the Russian ships of war, and for reforming, of consequence, the whole detail of the operations in the naval arsenals of that Empire.

These innovations, however, met with more resistance than either the Admiral or his Secretary had permitted themselves to suppose. The work of reform, conducted by a foreigner,
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even when he is supported by despotic power, must proceed but slowly ; jealousy, pride, and self-interest, will continually counteract the plans of improvement, and by their vigilance and unceasing activity, will never wholly fail of success. All this was experienced by Admiral KNOWLES ; yet there is no doubt that material advantages were derived, by the Russian navy, from the new system which he was enabled, partially, to introduce.

Mr ROBISON, from his first arrival at St Petersburg, had applied with great diligence to the study of the Russian language, and had made himself so much master of it, as to speak and write it with considerable facility. In summer 1772, a vacancy happening in the mathematical chair attached to the Imperial Sea Cadet Corps of Nobles, at Cronstadt, Mr ROBISON was solicited to accept of that office. His nautical and mathematical knowledge qualified him singularly for the duties of it, and his proficiency in the Russian language, removed the only objection that could possibly be proposed. When he accepted of the appointment, the salary of his predecessor was doubled, and the rank of Colonel was given him. Besides delivering his lectures as Professor, he officiated also as inspector of the above corps, in the room of General POLITIKA, who had retired, or been sent to his estates in the Ukraine.

The lectures which he gave were very much admired, and could not fail to be of the greatest use to his pupils. Few men understood so well the theory and the practice of the arts they profess to teach ; few had enjoyed the same opportunities of seeing the mathematical rules of artillery and navigation carried into effect on so great a scale. To his own countrymen, resident at Petersburg, Mr ROBISON was an object of no less affection than admiration.

In 1773, the death of Dr RUSSELL produced a vacancy in the Natural Philosophy Chair of the University of Edinburgh. Principal ROBERTSON, who was ever so attentive to the welfare of the University over which he presided, though not personally acquainted with Mr ROBISON, yet knowing his character, had no doubt of recommending him to the Patrons of the University, who, on their part, with no less disinterestedness, listened to his recommendation, and Mr ROBISON was accordingly elected. It is said, that when the news of this appointment reached him, he at first hesitated about the acceptance of it, principally from the fear of appearing insensible to the kindness and favour which he had experienced from the Russian Government. The moment, too, when it was known that this invitation had been given him, further offers of emolument and preferment were made him by that Government, of such a kind as it was supposed he could not possibly resist. At length he determined, and no doubt wisely, however splendid the prospects held out to him might be, to accept of a situation that would fix him permanently in his native country. He therefore declined the offers of the Empress of Russia, and in June 1774 sailed from Cronstadt for Leith, followed, as one of those friends he left behind in Russia has expressed it, by the regrets, and accompanied by the warmest good wishes, not only of all who had shared in his friendship, but of all to whom he was known. The Empress was so far from being offended with his determination, however much she wished to prevent it, that she settled a pension on him, accompanied with a request, that he would receive under his care two or three of the young cadets who were to be selected in succession.

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Mr ROBISON was admitted at Edinburgh the 16th September 1774, and gave his first course of lectures in the winter following. The person to whom he succeeded had been very eminent and very useful in his profession. He possessed a great deal of ingenuity, and much knowledge, in all the branches of Physical Science. Without perhaps being very deeply versed in the higher parts of the mathematics, he had much more knowledge of them than is requisite for explaining the elements of Natural Philosophy. His views in the latter were sound, often original, and always explained with great clearness and simplicity. The mathematical and experimental parts were so happily combined, that his lectures communicated not only an excellent view of the principles of the science, but much practical knowledge concerning the means by which those principles are embodied in matter, and made palpable to sense.

Mr ROBISON, who now succeeded to this chair, had also talents and acquirements of a very high order. The scenes of active life in which he had been early engaged, and in which he had seen the great operations of the nautical and the military art, had been followed, or accompanied, with much study, so that a thorough knowledge of the principles, as well as the practice, of those arts, had been acquired. His knowledge of the mathematics was accurate and extensive, and included, what was at that time rare in this country, a considerable familiarity with the discoveries and inventions of the foreign mathematicians.

In the general outline of his course, he did not, however, deviate materially from that which had been sketched by his predecessors, except, I think, in one point of arrangement, by which
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he passed from Dynamics immediately to Physical Astronomy. The sciences of Mechanics, Hydrodynamics, Astronomy and Optics, together with Electricity and Magnetism, were the subjects which his lectures embraced. These were given with great fluency and precision of language, and with the introduction of a good deal of mathematical demonstration. His manner was grave and dignified. His views always ingenious, and comprehensive, were full of information, and never more interesting and instructive than when they touched on the history of science. His lectures, however, were often complained of, as difficult and hard to be followed, and this did not, in my opinion, arise from the depth of the mathematical demonstrations, as was sometimes said, but rather from the rapidity of his discourse, which was in general beyond the rate at which accurate reasoning can be easily followed. The singular facility of his own apprehension, made him judge too favourably of the same power in others. To understand his lectures completely, was, on account of the rapidity, and the uniform flow of his discourse, not a very easy task, even for men tolerably familiar with the subject. On this account, his lectures were less popular than might have been expected from such a combination of rare talents as the author of them possessed. This was assisted by the small number of experiments he introduced, and a view that he took of Natural Philosophy which left but a very subordinate place for them to occupy. An experiment, he would very truly observe, does not establish a general proposition, and never can do more than prove a particular fact. Hence, he inferred, or seemed to infer, that they are of no great use in establishing the principles of science. This seems an erroneous view. An experiment does but prove a particular

lar fact ; but by doing so in a great number of cases, it affords the means of discovering the general principle which is common to all these facts. Even a single experiment may be sufficient to prove a very general fact. When a guinea and a feather, let fall from the top of an exhausted receiver, descend to the bottom of it in the same time, it is very true that this only proves the fact of the equal acceleration of falling bodies in the case of the two substances just named ; but who doubts that the conclusion extends to all different degrees of weight, and that the uniform acceleration of falling bodies of every kind, may safely be inferred.

A society for the cultivation of literature and science had existed in Edinburgh ever since the year 1739, when, by the advice, and under the direction of Mr MACLAURIN, an association, formed some years before for the improvement of Medicine and Surgery, enlarged its plan, and assumed the name of the Philosophical Society. This Society, which had at different times reckoned among its members some of the first men of whom this country can boast, had published three volumes of Memoirs, under the title of *Physical and Literary Essays* ; the last in 1756, from which time the Society had languished, and its meetings had become less frequent. At the time I am now speaking of, it was beginning to revive, and its tendency to do so was not diminished by the acquisition of Mr ROBISON, who became a member of it soon after his arrival. It had often occurred, that a more regular form, and an incorporation by Royal Charter, might give more steadiness and vigour to the exertions of this learned body. In 1783, accordingly, under the auspices of the late excellent Principal of this University, a Royal Charter was obtained, appointing certain persons
named.

named in it as a New Society, which, as its first act, united to itself the whole of the Philosophical.

Professor ROBISON, one of those named in the original charter, was immediately appointed Secretary, and continued to discharge the duties of that office, till prevented by the state of his health several years after.

The first volume of the Transactions of this Society, contains the first paper which Professor ROBISON submitted to the public, a "Determination of the Orbit and the Motion of the "Georgium Sidus, directly from Observations," read in March 1786. This planet had been observed by Dr HERSCHELL on the 13th March 1781, and was the first in the long list of discoveries by which that excellent observer has for so many years continued to enrich the science of Astronomy. Its great distance from the sun, and the slowness of its angular motion, which last amounts to little more than four degrees from one opposition to the next, made it difficult to determine its orbit with tolerable accuracy, from an arch which did not yet exceed an eighteenth part of the whole orbit. This was an inconvenience which time would remedy; but impatience to arrive even at such an approximation as the facts known will afford, is natural in such cases, and Professor ROBISON, as well as several other mathematicians, were not afraid to attempt the problem, even in this imperfect state of the data. It is well known that the observations which best serve the purpose of determining the orbit of a planet, are those made at its oppositions to the sun, when an observer in the Earth and in the Sun would refer the planet to the same point in the starry heavens, or when, in the language of astronomers, its heliocentric and geocentric places coincide. Of these

these oppositions in the case of this planet, there were yet only four which had been actually observed. Dr HERSCHELL had, however, discovered the planet soon after the opposition of 1781 was passed, and though of course that opposition was not seen, yet from the observations that were made so soon after, Professor ROBISON thought he could deduce the time with sufficient accuracy. The opposition of the winter 1786 he observed himself; for though there was, unfortunately, no observatory at Edinburgh, he endeavoured to supply that defect on the present occasion by a very simple apparatus, viz. a telescope on an equatorial stand, which served to compare the right ascension and declination of the planet with those of some known stars which it happened to be near. His general solution of the problem is very deserving of praise; and though the method pursued is in its principle the same with all those which ever since the time of KEPLER have been employed for finding the elements of a planetary orbit, it appears here in a very simple form, the construction being wholly geometrical, and easily understood. The elements, as he found them, are not very different from those that have since been determined from more numerous and more accurate observations.

When Dr HERSCHEL first made known this most distant of the planets, many astronomers believed that they had discovered the source of those disturbances in our system, which had not yet been explained. Professor ROBISON was of this number; for he tells us, in the beginning of his paper, that he had long thought that the irregularities in the motion of Jupiter and Saturn, which had not been explained by the mutual gravitation of the known planets, were to be accounted for by the action of planets of considerable magnitude, beyond the orbit of Saturn. Subsequent inquiry, however, has not ve-

rified this conjecture; the irregularities of Jupiter and Saturn have since been fully explained, and are known to arise chiefly from their action on one another, a very small part only being owing to that of the Georgium Sidus, or of any of the other planets.

The next publication of Professor ROBISON, was a paper in the second volume of the same Transactions, "On the Motion of Light, as affected by Refracting and Reflecting Substances, which are themselves in Motion *."

The phenomena of the aberration of the fixed stars are well known to depend on the velocity of the earth's motion combined with the velocity of light; the quantity of the aberration, when all other things are given, being directly as the first, and inversely as the second. It is not, however, the general or the medium velocity with which light traverses space, but it is the particular velocity with which it traverses the tube of the telescope, that determines the quantity of this aberration. Were it possible, therefore, to increase or diminish that velocity, the aberration would be diminished in the first case, and increased in the second. But, according to the principles now generally received in optics, the velocity of light is increased, when it traverses a denser medium, or one in which the refraction is greater; and therefore were the tube of a telescope to be filled with water instead of air, the aberration would be diminished. Professor ROBISON, and his friend Mr WILSON, Professor of Astronomy at Glasgow, had speculated much on this subject, and made many attempts to obtain a water telescope, but, hitherto, without effect. A paper of BOSCOVICH on the same subject, seemed to suggest some new views, that might render the experiment more easy to be made. That philosopher maintained,

* *Edinburgh Transactions*, vol. ii. p. 83.

maintained, that in ascertaining the effect of a water telescope on the motion of light, the observation of celestial objects might be dispensed with, and that of terrestrial substituted in its place. He argued, that while light moves with an uniform velocity, the telescope must be directed, not to the point of space which the object occupied when the particle was sent off which is entering the telescope, but to a point advanced before it by a space just equal to that which both the object and the observer have passed over in the time in which the particle has passed from the object to the eye. It is therefore directed exactly to the place which the object is in when the light from it enters the eye. If, therefore, the ray, on entering the telescope, is made to move faster than it did before, the telescope must not be inclined so much, and the apparent place of the object will fall behind its true place. If the ray is retarded on entering the water, the contrary must happen. Hence a number of very unexpected phenomena would result, affording, without having recourse to the heavenly bodies, a direct proof of the motion of the earth in its orbit, as well as a resolution of the question, whether light is accelerated or retarded on passing from a rarer to a denser medium*.

On this reasoning Professor ROBISON has very well remarked, that it would be just, if the light, on entering the water telescope, had only its velocity changed, and not its direction. But this is not the case; for the ray that is to go down the axis of the telescope, is not perpendicular to the surface of the fluid; it makes an angle with it, depending on the aberration, and therefore in some cases less by 20" than a right angle. On this account, the effect is not produced which BOSCOVICH's reasonings lead us to expect.

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* BOSCOVICH, *Opera Math.* tom. II. opusc. 3.

The sequel of the paper is also full of ingenious remarks.

In December 1785, Mr ROBISON was attacked by a severe disorder, which, with but few intervals of relaxation, continued to afflict him to the end of his life, and which, though borne with much resignation, and resisted with singular fortitude, could not but at length impair both the vigour and the continuity of his exertions. The disorder seemed to be situated between the urethra and the perineum. At times it was accompanied with the severest pain, and with violent spasms, which were easily excited. The disease, however, was only known by the pain produced; and never, by any visible or palpable symptom, gave information of its nature, as no change in the parts which were the seat of it could ever be observed. A complaint of this nature, it is evident, must have less chance of being removed than any other, and it accordingly baffled the art of the most skilful medical men, both in Edinburgh and London.

Notwithstanding this state of suffering, his general health was not for a long time materially injured, nor the powers of his mind relaxed, so that he continued to prosecute study with vigour and steadiness. A malady which was both severe and chronical, admitted of no palliative so good as the comfort of domestic society, which Mr ROBISON happily enjoyed, having married soon after he settled in Edinburgh. The care and attention of Mrs ROBISON, and the affectionate regards of his children, as they grew up, were blessings to which, with all his habits of study and abstraction, he was ever perfectly alive.

This indisposition did not prevent him from engaging, about this time, in a very laborious undertaking. A work, with the
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title of *Encyclopædia Britannica*, undertaken at Edinburgh several years before this period, was now undergoing a third edition, in which it was to advance from three to eighteen volumes. Twelve of these had been already published, under the direction of the original editor, Mr COLIN MACFARQUHAR, when, on his death, the task of continuing the work was committed to the care of the Reverend Dr GLEIG, and about the same time Professor ROBISON became a contributor to it. He was the first contributor who was professedly and really a man of science, and from that time the *Encyclopædia Britannica* ceased to be a mere compilation. Dictionaries of Arts and Sciences, in this island, had hitherto been little else than compilations; and though in France, the co-operation of some of the most profound and enlightened men of the age, had produced a work of great merit and celebrity, with us compositions of the same class had been committed to the hands of very inferior artists. The accession of Professor ROBISON was an event of great importance in the history of the above publication.

It was in the year 1793 that he began to write in this book, and it was at the article Optics, with him a very favourite science, that his labours commenced. From that time he continued to enrich the *Encyclopædia* with a variety of valuable treatises, till its completion in 1801.

The general merit of the articles thus composed, makes it difficult to point out particulars. Those in which theoretical and practical knowledge are combined, are of distinguished merit; such are Seamanship, Telescope, Roof, Water-works, Resistance of Fluids, Running of Rivers. To these I must add the articles Electricity and Magnetism in the Supplement, where the theories of ÆPINUS are laid down with great clearness and precision, as well as with very considerable improvements. In ascertaining the law of the electric attraction, his
experiments

experiments were ingenious, as well as original, and afforded an approximation to the result which the great skill and the excellent apparatus of COULOMB have since exactly ascertained. In the Supplement is also contained a very full account of the Theory of BOSCOVICH; a subject with which he was much delighted, and which he used to explain in his lectures, with great spirit and elegance.

These articles, if collected, would form a quarto volume of more than a thousand pages. I am persuaded, that when brought together, and arranged by themselves, they will make an acceptable present to the public; and I have the satisfaction to state, that such a work is now preparing, under the direction of an Editor whose remarks or corrections cannot but add greatly to its value. Notwithstanding the merit which the separate articles possess, they are not entirely free from the faults incident to whatever is composed for a work already in the press. The condensation and arrangement, to which time is such an essential condition, even with men of the first talents, must be often wanting, in such circumstances; and there are, accordingly, in the articles now referred to, a diffuseness, and sometimes a want of order, that may easily be corrected, without injuring the authenticity of the work.

Though the Encyclopædia employed Professor ROBISON very much during the whole of the seven years that it continued, he nevertheless found leisure for some researches of a very different nature. At the period of which I now speak, the French Revolution had arrested the attention, and excited the astonishment of all Europe; and the satisfaction with which the first efforts of a nation to assert its liberties, had been hailed from all quarters, was, by the crimes and excesses which followed, quickly converted into grief and indignation. A body was put in motion sufficient to crush whole nations under its weight;

weight; none had the power or the skill to direct its course; what movements it might communicate to other bodies, how far it would go, or in what quarter, it seemed impossible to foretel. The amazement became general; no man was so abstracted from the pursuits of the world, or so insulated by peculiarities of habit and situation, as not to feel the effects of this powerful concussion. All fixed their eyes on the extraordinary spectacle which France exhibited; where, if time is to be measured by the succession of events, a year was magnified into an age; and when in a few months one might behold more old institutions destroyed, and more new ones projected or begun, than in all the ten centuries which had elapsed between CHARLEMAGNE and the last of his successors:—In a word, where the ancient edifice, founded in the ages of barbarism with such apparent solidity, strengthened and adorned in the progress of civilisation with so much skill and labour, was in one moment levelled with the dust. A general state of alarm and distrust was the effect of the convulsions which men saw every where around them; where the institutions held as sacred from their origin, or venerable from their antiquity, and essential to the order of society, were seen, not falling to pieces from natural decay, but blown up by the force of a sudden and unforeseen explosion. From such a condition of the world, jealousy and credulity could not fail to arise. When danger is all around, every thing is of course suspected; and when the ordinary connection between causes and effects cannot be traced, men have no means of distinguishing between the probable and the improbable; so that their opinions are dictated by their prejudices, their impressions, and their fears. Such, accordingly, was the state into which mens minds were brought at this extraordinary crisis; and even in this country, removed, as we were, from the danger, by so strong

strong a barrier of causes, both moral and physical, the alarm was general and indiscriminate. The progress of knowledge was supposed by many to be the cause of the disorder; panegyrics on ignorance and prejudice were openly pronounced; the serious and the gay joined in declaiming against reason and philosophy; and all seemed to forget, that when reason and philosophy have erred, it is by themselves alone that their errors can be corrected.

The fears that had thus taken possession of mens' minds, were often artificially increased. It was supposed that the general safety depended on the general alarm; that the more the terror was extended, the more would the object of it be resisted; and hence, doubtless, many felt it their interest, and some considered it their duty, to magnify the danger to which the public was exposed.

It is evident, that an inquiry into the causes of the French Revolution, undertaken at a moment of such agitation, was not likely to bear the review of times of calm and sober reflection. It was at this moment, however, and under the influence of such impressions, that Mr ROBISON undertook to explain the causes of that revolution. He was deeply affected by the scenes that were passing before him. He possessed great sensibility, and his mind, peculiarly alive to immediate impressions, felt strongly the danger to which the social order of every nation seemed now to be exposed. The crimes which the name of Liberty had been employed to sanction, filled him with indignation, and the contempt of religion, affected by many of the leaders of the Revolution, wounded those sentiments of piety which he had uniformly cherished from his early youth.

In such circumstances, a mind accustomed to inquire into causes, as his had long been, could not abstain from the attempt

tempt to trace the sources of so extraordinary a succession of events. As to the circumstances which first led him, and led him, I think, so unhappily, to look for those sources in the institutions of Free Masonry, or in the combination of some German mystics, I have nothing satisfactory to offer. He was accustomed to refined and subtle speculations, and naturally entertained a partiality for theories that called into action the powers by which he was peculiarly distinguished.

In 1797, he published a book, entitled, "Proofs of a Conspiracy against all the Religions and Governments of Europe." He supposes, that this conspiracy originated in the Lodges of the Free Masons, but that it first assumed a regular form in the hands of certain philosophic fanatics, distinguished in Germany by the name of *Illuminati*; that after the suppression of this society by the authority of Government, the spirit was kept alive by what was called the German Union; that its principles gradually infected most of the philosophers of France and Germany, and lastly broke forth with full force in the French Revolution.

The history of *Illuminatism*, as it is called, forms the principal part of the work; and on a subject involved in great mystery, where all the evidence came through the hands of friends or of enemies, it was exceedingly difficult for one living in a foreign country, and a stranger to the public opinion, to obtain accurate information. Accordingly, the events related, and the characters described, as proofs of the conspiracy, are of so extraordinary a nature, that it is difficult to persuade one's self that the original documents from which Mr ROBISON drew up his narrative were entitled to all the confidence which he reposed in them.

I do not mean to question the general fact, that there did exist in Germany a society having the vanity to assume the

name just mentioned, and the presumption or the simplicity to believe that it could reform the world. In a land where the tendency to the romantic and the mysterious seems so general, that even philosophy and science have not escaped the infection, and in states where there is much that requires amendment, it is not wonderful if associations have been formed for redressing grievances, and reforming both religion and government. Some men, truly philanthropic, and others, merely profligate, may have joined in this combination; the former, very erroneously supposing, that the interests of truth and of mankind may be advanced by cabal and intrigue; and the latter, more wisely concluding, that these are engines well adapted to promote the dissemination of error, and the schemes of private aggrandisement. An ex-Jesuit may have been the author of this plan, and whether he belonged to the former or the latter class, may have chosen for the model of the new arrangement, those institutions which he knew from experience to be well adapted for exercising a strong but secret influence in the direction of human affairs.

In all this there is nothing incredible; but the same, I think, cannot be asserted, when the particulars are examined in detail. It is extremely difficult, as has already been remarked, for a foreigner, in such circumstances as Mr ROBINSON'S, to avoid delusion, or to determine between the different kinds of testimony of which he must make use. With me, who have no access to the original documents, and if I had, who have neither leisure nor inclination to examine them, an opinion can only be formed from the internal evidence, that is, from the nature of the facts, and the style in which they are recorded. The style of the works from which Mr ROBINSON composed his narrative, is not such as to inspire confidence; for, wherever it is quoted, it is that of an angry and inflated

flated invective. The facts themselves are altogether singular, arguing a depravity quite unexampled in all the votaries of *illumination*. From the perusal of the whole, it is impossible not to conclude, that the alarm excited by the French Revolution, had produced in Mr ROBISON a degree of credulity which was not natural to him. The suspicion with which he seems to view every person on the continent, to whom the name of a Philosopher can be applied, and the terms of reproach and contempt to which, whether as individuals or as bodies, they are always subjected, make it evident that the narrative is not impartial, and that the author was prepared, in certain cases, to admit the slightest presumption as clear and irrefragable evidence. When, indeed, he speaks of such obscure men as composed the greater part of the supposed conspirators, we have no direct means of determining in what degree he has been misled. But when we see the same sort of suspicion and abuse directed against the best known and most justly celebrated characters of the age, we cannot but lament the prejudices which had taken possession of an understanding in other matters so acute and penetrating.

Among the men engaged in public affairs, of whom Europe boasted during the last century, there was perhaps none of a higher character than TURGOT, who, to the abilities of a statesman, added the views of a philosopher; was a man singularly patriotic and disinterested, distinguished by the virtues both of public and private life, and having, indeed, no fault but that of being too good for the times in which he lived. Yet Mr ROBISON has charged this upright and humane minister with an exercise of power, which would argue the most extreme depravity. He states*, that there existed in Paris a com-

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bination

* *Proofs of a Conspiracy, &c.* 4th Edit. Note, p. 584.

bination under the direction of the Wits and Philosophers, who used to meet at the house of Baron D'HOEBACH, having for its object the dissection of the brains of living children, purchased from poor parents, in order to discover the principle of vitality. The police, he adds, interposed to put a stop to these bloody experiments, but the authors of them were protected by the credit of TURGOT.

All this is asserted on the authority, it should seem, of some anonymous German publication. I will not enter on the refutation of a calumny with the fabrication of which Mr ROBISON is not chargeable, though culpable without doubt, for having allowed his writings to become the vehicle of it. Truth and justice require this acknowledgment; and, in making it, I think that I am discharging a duty both to Mr ROBISON and myself:—It is a duty to Mr ROBISON, in as much as a concession made by a friend, is better than one extorted by an adversary; it is a duty to myself, because I should feel that I was doing wrong, were I even by silence to acquiesce in a representation which I believed to be so ill-founded and unjust.

The *Proofs of the Conspiracy*, notwithstanding these imperfections, or perhaps on account of them, were extremely popular, and carried the name of the author into places where his high attainments in science had never gained admission for it. In the course of two years, the book underwent no less than four editions. It is a strong proof of the effect on the minds of men produced by the French Revolution; and of the degree in which it engrossed their thoughts, that the history of a few obscure enthusiasts in Bavaria or Wirtemberg, when it became associated with that Revolution, was read in Britain with so much avidity and attention.

The

The defects of the evidence were concealed by the prejudices and apprehensions which were then so general. The people of this country were disposed to believe every thing unfavourable to the French nation, but particularly to the philosophers. All might not be equally culpable, but to discriminate between them was not thought of much importance, and it was the simplest, if not the fairest way, to divide the demerit equally among the whole. The rhapsodies of BARRUEL had already prepared the public for such impartial decisions, and had held up every man of genius and talents, from MONTESQUIEU to CONDORCET, as objects of hatred and execration.

But whatever opinion be formed of the facts related in the history of this conspiracy, it is certainly not in the visions of the German Illuminati, nor in the ceremonials of Free Masonry, that we are to seek for the causes of a Revolution, which has shaken the civilised world from its foundations, and left behind it so many marks, which ages will be required to efface. There is a certain proportionality between causes and their effects, which we must expect to meet with in the moral no less than in the natural world; in the operations of men as well as in the motions of inanimate bodies. Whenever a great mass of mankind is brought to act together, it must be in consequence of an impulse communicated to the whole, not in consequence of a force that can act only on a few. A Hermit or a Saint might have preached a crusade to the Holy Land, with all the eloquence which enthusiasm could inspire; but if a spirit of fanaticism and of chivalry had not pervaded every individual in that age, they would never have led out the armies of Europe to combat before the walls of Jerusalem. Neither could the influence of a small number of religious or philosophic fanatics, sensibly accelerate or retard those powerful causes which prepared from afar the
destruction

destruction of the French monarchy. When opposed to these causes, such influence was annihilated; when co-operating with them, its effects were imperceptible. It was a force which could only follow those already in action; it was like "dash-
"ing with the oar to hasten the cataract," or, "waving with
"a fan to give swiftness to the wind*."

It is, however, much easier to say what were not, than what were, the causes of the French Revolution; and in dissenting from Professor ROBISON, I will only remark in general, that I believe the principal causes to be involved in this maxim, That a certain relation between the degree of Knowledge diffused through a nation, and the degree of Political Liberty enjoyed by it, is necessary to the stability of its government. The knowledge and information of the French people, exceeded the measure that is consistent with the entire want of political liberty. The first great exigency of Government, therefore, the first moment of a weak administration, could hardly fail to produce an attempt to obtain possession of those rights, which, though never enjoyed, can never be alienated. Such an occasion actually occurred, and the revolution which took place was entire and terrible. This also was to be expected; for there seems to be among political institutions, as among mechanical contrivances, two kinds of equilibrium, which, though they appear very much alike in times of quiet, yet, in the moment of agitation and difficulty, are discovered to be very different from one another. The one is tottering and insecure, in so much that the smallest departure from the exact balance leads to its total subversion. The other is stable, so that even a violent concussion only excites some vibrations backward and forward, after which every thing settles in its
own

* FERGUSON'S *Essay on Civil Society*, Part III. Sect. 4.

own place. Those governments in which there is no political liberty, and where the people have no influence, are all unavoidably in the first of these predicaments: those in which there is a broad basis of liberty, naturally belong to that in which the balance re-establishes itself. The same weight, that of the people, which in the first case tends to upset the balance, tends in the second to restore it: and hence, probably, the great difference between the result of the French Revolution, and of the revolutions which formerly took place in this country.

It will be happy for mankind, if they learn from these disasters, the great lessons which they seem so much calculated to enforce, and if while the people reflect on the danger of sudden innovation, their rulers consider, that it is only by a gradual reformation of abuses, and by extending, rather than abridging, the liberties of the people, that a remedy can be provided against similar convulsions.

But I return willingly from this digression, to those branches of knowledge, where, in describing what Mr ROBISON has done, the language of truth and of praise will never be found at variance with one another.

In autumn 1799, this country had the misfortune to lose one of its brightest ornaments, Dr BLACK, who had laid the foundation of the Pneumatic Chemistry, and discovered the principle of Latent Heat. The Doctor had published very little; and his discoveries were more numerous than his writings. His lectures, however, had drawn much attention; they presented the first philosophical views of chemical science; they were remarkable for their perspicuity and elegance, and this, joined to the simplicity and gracefulness of manner in which they were delivered, made them universally admired.

It

It was now proposed to publish these lectures ; but this required that they should be put into the hands of some one able to perform the part of an editor, and to prepare for the press the notes from which the Doctor used to read his lectures. The person naturally thought of was Mr ROBISON, one of Dr BLACK's oldest friends, and so well skilled in chemistry, that no one could be supposed to execute the work with more zeal or more intelligence. The task, however, was by no means easy. Dr BLACK with a very large share of talent and genius, with the most correct taste and soundest judgment, with no habits that could dissipate his mind, or withdraw it from the pursuits of science, was less ardent in research, and less stimulated by the love of fame, than might have been expected from such high endowments. A state of health always delicate, and subject to be deranged by slight accidents, was probably the cause of this indifference. Hence the small number of his writings, and his sudden stop in that career of discovery on which he had entered with such brilliancy and success. Of much that he had done, the world had never heard any thing, but from verbal communication to his pupils, and on the subject of latent heat, no written document remained to ascertain to him the property of that great discovery. The only means of repairing this loss, and counteracting the injustice of the world, was the publication which Professor ROBISON now undertook with so much zeal, and executed with so much ability. Dr BLACK had used to read his lectures from notes, and these often but very imperfect, and ranged in order by marks or signs only known to himself. The task of editing them was therefore difficult, and required a great deal both of time and labour, but was at last accomplished in a manner to give great satisfaction. The truth, however, is, that the time was past when this work would have met in the world with the reception

tion which it deserved. Chemical theories had of late undergone great changes, and the language of the science was entirely altered. Dr BLACK, on the subject of these changes, had corresponded with LAVOISIER, and the mutual respect of two great men for one another, was strongly marked in the letters which passed between them. The Doctor had acceded to the changes proposed by the French chemist, and had even adopted the new nomenclature; but his notes had not undergone the alterations which were necessary to introduce it throughout. It would now have been difficult to make those alterations; and Mr ROBISON, who was not favourable to the new chemistry, did not conceive that by making them, he was permanently serving the interest of his friend. He conceived, indeed, that there was unfairness in the means employed by LAVOISIER, for bringing Dr BLACK to adopt the new system of chemistry, and has thrown out some severe reflections on the conduct of the former, which appear to me to rest on a very slight foundation.

It was quite natural for a man, convinced, like LAVOISIER, of the importance of the improvements which he had made in chemistry, to be desirous that they should be received by the most celebrated Professor of that time,—by the very man, too, whose discoveries had opened the way to those improvements. His letters to Dr BLACK, contain expressions of respect and esteem, which, I confess, appear to me perfectly natural, and without any thing like exaggeration or deceit. Indeed it is not probable that M. LAVOISIER, even if he could himself have submitted to flatter or cajole, could conceive that any good effect was to arise from doing so, or that there was any other way of inducing a grave, cautious, and profound phi-

losopher, to adopt a certain system of opinions, but by convincing him of their truth? He had, with those who knew him, the character of a sincere man, very remote from any thing like art or affectation. We must therefore ascribe the view which Mr ROBISON took of this matter, to the same system of prejudices on which we have had already occasion to animadvert. Such, indeed, was the force of those prejudices, that he considered the Chemical Nomenclature, the new System of Measures, and the new Kalendar, as all three equally the contrivances of men, not so much interested for science, as for the superiority of their own nation. Now, whatever be said of the Kalendar, the project of uniform Weights and Measures is admitted to be an admirably contrived system, which Britain is now following at a great distance; and the New Nomenclature of Chemistry to be a real scientific improvement, adopted all over Europe. Many of the radical words may depend on false theories, and may of course require to be changed; but though the *matter* pass away, the *form* will remain; the words of the language may perish, but the mould in which the language was cast will never be destroyed*. The Lectures appeared in 1803.

The

* The high opinion which Mr ROBISON elsewhere expresses of LAVOISIER, is very remarkable. In his *Astronomy*, published a year after the Lectures, in stating Hook's anticipation of the Principles of Gravitation, he concludes thus: "It is worthy of remark, that in this clear and candid and modest exposition of a rational theory, Hook anticipated the discoveries of NEWTON, as he anticipated with equal distinctness and precision, the discoveries of LAVOISIER, a Philosopher inferior perhaps only to NEWTON." (*Elements of Mechanical Philosophy*, p. 285.)

The last of Mr ROBISON'S works was one which he had long projected, though he now set about the completion and arrangement of it, for the first time. It was entitled, *Elements of Mechanical Philosophy, being the substance of a course of lectures on that science*. "Mechanical Philosophy" was, with him, a favourite expression; it was understood as synonymous with Natural Philosophy, and included the same branches. The first volume, the only one he lived to finish, included Dynamics and Astronomy, and was published in 1804. It is a work of great merit, and is accessible to those who have no more than an elementary knowledge of the mathematics. The short view of the phenomena prefixed to the Physical Astronomy is executed in a masterly manner. The same may be said, and perhaps even with more truth, of the Physical Astronomy itself; for there are very few of the elementary treatises on that branch of science which can be compared with it, either for the facility of the demonstration, or the comprehensiveness of the plan. The first part is meant to be popular and historical, and is so at the same time that it is philosophical and precise. The work is indeed highly estimable, and is entitled to much more success in the world than it has actually had.

We have already taken notice of Mr ROBISON'S illness, with which he had been now afflicted for the long period of nineteen years. His sufferings, though not equal, had been often extremely severe. They had occasionally rendered him unable to discharge his duty in the College, and of late his friend, the Reverend Dr THOMAS MACKNIGHT, had, with great kindness and ability, frequently supplied his place. Against such a continuance of ill health, with so little hopes of recovery as could be entertained for a long time past, hardly any mind could be
expected

expected to remain in full possession of activity and vigour. This is the more difficult, as the valuable medicine which alone in such cases can assuage pain, contributes itself at length to weaken the mind, and to destroy its energy. The combat which Mr ROBISON had maintained against these complicated evils, had indeed been wonderfully vigorous and successful, and the last of his works is quite worthy of his days of most perfect health and enjoyment.

The body could not resist so well as the mind. In the end of January 1805, he was suddenly seized with a severe illness, which put an end to his life in the course of forty-eight hours. There was a general disturbance of the system, which, without having the character of any defined disease, exhibited those symptoms of universal disorder which denote a breaking up of the constitution, and never fail to terminate fatally.

On reviewing the whole of his character, and the circumstances of his life, it is impossible not to see in him a man of extraordinary powers, who had enjoyed great opportunities for improvement, and had never failed to turn them to the best account. He possessed many accomplishments rarely to be met with in a scholar, or a man of science. He had great skill and taste in music, and was a performer on several instruments. He was an excellent draughtsman, and could make his pencil a valuable instrument either of record or invention. When a young man, he was gay, convivial, and facetious, and his *vers de société* flowed, I have been told, easily and with great effect. His appearance and manner were in a high degree favourable and imposing; his figure handsome, and his face expressive of talent, thought, gentleness, and good temper.

When

When I had first the pleasure to become acquainted with him, the youthful turn of his countenance and manners was beginning to give place to the grave and serious cast, which he early assumed; and certainly I have never met with any one whose appearance and conversation were more impressive than his were at that period.

Indeed his powers of conversation were very extraordinary, and when exerted, never failed of producing a great effect. An extensive and accurate information of particular facts, and a facility of combining them into general and original views, were united in a degree of which I am persuaded there have been few examples. Accordingly, he would go over the most difficult subjects, and bring out the most profound remarks, with an ease and readiness which was quite singular. The depth of his observations seemed to cost him nothing; and when he said any thing particularly striking, you never could discover any appearance of the self-satisfaction so common on such occasions. He was disposed to pass quite readily from one subject to another; the transition was a matter of course, and he had perfectly, and apparently without seeking after it, that light and easy turn of conversation, even on scientific and profound subjects, in which we of this island are charged by our neighbours with being so extremely deficient.

The same facility, and the same general tone, was to be seen in his lectures and his writings. He composed with singular facility and correctness, but was sometimes, when he had leisure to be so, very fastidious about his own compositions.

In the intercourse of life, he was benevolent, disinterested, and friendly, and of sincere and unaffected piety. In his interpretation

terpretation of the conduct of others, he was fair and liberal, while his mind retained its natural tone, and had not yielded to the alarms of the French Revolution, and to the bias which it produced.

His range in science was most extensive ; he was familiar with the whole circle of the accurate sciences, and there was no part of them on which, if you heard him speak or lecture, you would not have pronounced it to be his *fort*, or a subject which he had studied with more than ordinary attention. Indeed, the rapidity with which his understanding went to work, and the extent of ground he seemed to have got over, while others were only preparing to enter on it, were the great features of his intellectual character. In these he has rarely been exceeded. With such an assemblage of talents, with a mind so happily formed for science, one might have expected to find in his writings more of original investigation, more works of discovery and invention. I must remark, however, that from the turn his speculations and compositions took, or rather received from circumstances, we are apt to overlook what is new and original in a great part of them. An article in a Dictionary of Science must contain a System, and what is new becomes of course so mixed up with the old and the known, that it is not easily distinguished. Many of Mr ROBISON's articles in the *Encyclopædia Britannica* are full of new and original views, which will only strike those who study them particularly, and have studied them in other books. In *Scamanship*, for example, there are many such remarks ; the fruit of that knowledge of principle which he combined with so much experience and observation. *Carpentry*, *Roof*, and many more, afford examples of the same kind. The publication now under the management of Dr BREWS-

TER,

STER, will place his scientific character higher than it has ever been with any but those who were personally acquainted with him. With them, nothing can add to the esteem which they felt for his talents and worth, or to the respect in which they now hold his memory.

END OF THE SEVENTH VOLUME.

A P P E N D I X,

Containing Lists of the OFFICE-BEARERS and MEMBERS elected
since November 1812.

23d November 1812.

OFFICE-BEARERS.

Sir JAMES HALL, Baronet, President.

Lord WEBB SEYMOUR, }
Lord MEADOWBANK, } Vice-Presidents.

Professor PLAYFAIR, Secretary.

JAMES BONAR, Esq. Treasurer.

THOMAS ALLAN, Esq. Keeper of the Museum.

PHYSICAL CLASS.

Sir GEORGE MACKENZIE, Baronet, President.

THOMAS CHARLES HOPE, M. D. Secretary.

Counsellors.

JAMES GREGORY, M. D.

Lord HERMAND.

DUGALD STEWART, Esq.

ALEXANDER KEITH, Esq.
 JAMES RUSSELL, Esq. Surgeon.
 DANIEL RUTHERFORD, M. D.

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.
 THOMAS THOMSON, Esq. Secretary.

Counsellors.

DAVID HUME, Esq.
 Reverend Principal BAIRD.
 The LORD PRESIDENT.
 LORD ROBERTSON.
 Reverend Sir HENRY MONCREIFF WELLWOOD, Baronet.
 Reverend ARCHIBALD ALISON.

25th January 1813.

MEMBERS ELECTED.

HONORARY.

M. LE COMTE LA PLACE, Member of the Institute, and of the
 Board of Longitude of France, &c.
 M. LE COMTE LAGRANGE, Member of the Institute and Board
 of Longitude of France, &c.
 M. G. CUVIER, Member of the Institute of France, Professor
 of Anatomy, &c.

ORDINARY.

ORDINARY.

Reverend ALEXANDER MURRAY, Professor of Hebrew and Oriental Languages in the University of Edinburgh.

WILLIAM SOMMERVILLE, M. D. Deputy Inspector of Military Hospitals in Scotland.

JAMES HARE, M. D. late of Calcutta.

ALEXANDER BOSWELL, Esq. of Auchinleck.

HENRY DAVIDSON, M. D. Physician in Edinburgh.

22d November 1813.

OFFICE-BEARERS.

Sir JAMES HALL, Baronet, President.

Lord MEADOWBANK, }
Lord WEBB SEYMOUR, } Vice-Presidents.

Professor PLAYFAIR, Secretary.

JAMES BONAR, Esq. Treasurer.

THOMAS ALLAN, Esq. Keeper of the Museum.

PHYSICAL CLASS.

Sir GEORGE MACKENZIE, Baronet, President.

THOMAS CHARLES HOPE, M. D. Secretary.

Counsellors.

Professor DUGALD STEWART,

ALEXANDER KEITH, Esq.

JAMES RUSSELL, Esq. Surgeon.

DANIEL RUTHERFORD, M. D.

JAMES BRYCE, Esq. Surgeon.

DAVID BREWSTER, LL. D.

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.

THOMAS THOMSON, Esq. Secretary.

Counsellors.

The LORD PRESIDENT.

Lord ROBERTSON.

Reverend Sir HENRY MONCREIFF WELLWOOD, Bart.

Reverend ARCHIBALD ALISON.

WALTER SCOTT, Esq.

Reverend Dr JAMIESON.

24th January 1814.

MEMBERS ELECTED.

HONORARY.

M. L'ABBE' HAUY, Professor of Mineralogy in the Museum of Natural History, and Member of the National Institute of Paris.

ORDINARY.

HENRY JARDINE, Esq. W. S.

Mr PATRICK NEILL.

Lord Viscount ARBUTHNOTT.

Reverend JOHN THOMSON, Duddingston.

Reverend JOHN FLEMING, Flisk.

Dr JOHN CHEYNE, Dublin.

HAY DONALDSON, Esq. W. S.

Sir JAMES MACINTOSH, Baronet, M. P.

Colonel TYTLER, Assistant Quarter-Master General for Scotland.

Reverend ALEXANDER BRUNTON, D. D. Edinburgh.

Professor GEORGE GLENNIE, Marischall College, Aberdeen.

28th November 1814.

OFFICE-BEARERS.

Sir JAMES HALL, Baronet, President.

Lord MEADOWBANK, }
Lord WEBB SEYMOUR, } Vice-Presidents.

Professor PLAYFAIR, Secretary.

JAMES BONAR, Esq. Treasurer.

THOMAS ALLAN, Esq. Keeper of the Museum.

PHYSICAL CLASS.

Sir GEORGE MACKENZIE, Baronet, President.

THOMAS CHARLES HOPE, M. D. Secretary.

Counsellors.

JAMES RUSSELL, Esq.

Dr RUTHERFORD.

JAMES BRYCE, Esq.

DAVID BREWSTER, LL. D.

Sir W. FORBES, Baronet.

Dr ANDREW COVENTRY.

LITERARY

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.

THOMAS THOMSON, Esq. Secretary.

Counsellors.

Reverend Sir HENRY MONCREIFF WELLWOOD, Baronet.

Reverend ARCHIBALD ALISON.

WALTER SCOTT, Esq.

Reverend Dr JAMIESON.

Lord GLENLEE.

Dr THOMAS BROWN.

 19th January 1815.

MEMBERS ELECTED.

HONORARY.

Baron ALEXANDER DE HUMBOLDT, Member of the Institute of France.

M. ARAGO, Member of the Institute of France, Astronomer Royal, and Professor of the Polytechnic School.

M. GAY LUSSAC, Member of the Institute of France, and Professor of the College of France.

M. BIOT, Member of the Institute of France, of the Board of Longitude, and Professor of Natural History in the College of France.

ORDINARY

ORDINARY.

GILBERT LAING MEASON, Esq. of Lindertis.

ROBERT STEVENSON, Esq. Civil Engineer.

THOMAS LAUDER DICK, Esq. younger of Fountainhall.

JOHN YULE, M. D. Physician in Edinburgh.

HENRY HOME DRUMMOND, Esq. younger of Blair Drummond.

WILLIAM CHARLES WELLS, M. D. Physician in London, and F. R. S.

CHARLES GRANVILLE STEWART MENTEATH, Esq. of Closeburn.

WILLIAM THOMAS BRANDE, Esq. F. R. S. and Professor of Chemistry in the Royal Institution of London.



