LXVII. An abstract of a memoir on physical geology; with a further exposition of certain points connected with the subject

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This large quantity of water suddenly separated from a state of combination in which it had been solid, produces, by its absorption of caloric of liquidity, the frigorific property.

The sulphates of zinc and magnesia dissolve in muriatic acid, and by cooling or evaporation are obtained unaltered. The muriatic acid does not appear to produce any change of nature.

When protosulphate of iron is dissolved in muriatic acid, the liquor furnishes by crystallization quantities of unaltered sulphate and of chloride of iron. Sometimes the sulphate retains its common quantity of water of crystallization, but at others I have obtained a salt giving by analysis:

Sulphuric acid	18·7	$\ddot{S} =$	19·5
Protoxide of iron	16·7	Fe =	17·3
Water and loss	14·6	3 H =	13·2
	<hr/> 50·0		<hr/> 50·0

The crystals were always so aggregated that their form could not be accurately determined; they are transparent, harder, and of a much lighter green than ordinary copperas; they are quite permanent, and when dissolved in water give sulphate of iron with the ordinary quantity of water.

The sulphate of alumina crystallizes unaltered from its solution in muriatic acid, but in more beautiful plates than from water. From the solution of sulphate of nickel or of mercury in muriatic acid, these salts are deposited by crystallization unchanged.

23, Lower Gloucester Street, Dublin: March 25, 1836.

LXVII. *An Abstract of a Memoir on Physical Geology; with a further Exposition of certain Points connected with the Subject.* By W. HOPKINS, Esq., M.A., F.G.S., of St. Peter's College, Cambridge.

[Continued from p. 281, and concluded.]

IV. **T**HE two systems of fissures which I have described are those which must be regarded according to this theory as *primary phenomena*, from which, as before stated, the *secondary phenomena* of mineral veins, faults, anticlinal lines, &c., must be derived. For this second part of the subject, I must refer to the second Section of my Memoir, where I have entered in detail into the manner in which these latter phenomena may be conceived to be derived from the former. The number of phenomena which we are thus enabled to account for, as the consequences of a simple cause from which

they are deducible by strict mechanical reasoning, appears to me, in the present state of our speculations and practical knowledge, to give to the theory I have been attempting to develop the strongest claim to the attention of geologists.

It will be observed, that a most essential part of this theory consists in the relation which it assigns between the directions of dislocation and the general configuration of the elevated mass *at the instant previous to its rupture*. It may, at first sight, appear impossible to ascertain what this form may have been, now that we can only examine the mass in its dislocated state; but this difficulty, though it must always exist in a greater or less degree, will not appear so serious a practical one, when we consider that since necessary relations must exist between the form of the mass at the instant above mentioned, and the lines of dislocation, and again between these lines and the actual disturbed form of the mass, some such relations must also exist between this latter and the previous form. Thus if the actual form be approximately conical, we may conclude it to have been also conical at the instant of dislocation; and if the disturbed district be of great length as compared with its width, and presents a well-defined axis of elevation, we may conclude the unbroken elevation to have been approximately cylindrical. Partial elevations which may have been superimposed upon a general one, as already described, at the instant previous to dislocation, must generally be more difficult to detect, since it may frequently be impossible to distinguish present indications of them from similar elevations which may have been produced by the elevatory force entirely *subsequently* to the formation of the fissures. On such points the observer must of course exercise his discrimination and judgement. Deviations from rectilinearity or parallelism in the lines of dislocation are not to be regarded necessarily as anomalies. We frequently speak, it is true, of the law of parallelism in such phænomena, as if that were their essential characteristic, but it is manifest that, according to our theory, this is only a secondary property in them, depending on the rectilinearity of the general axis of elevation. In conical elevations such as those before alluded to (p. 233) there is no approximation to parallelism in the observed fissures; and if the general axis of elevation be curvilinear, the longitudinal fissures preserving their parallelism with it (according to theory) will be also curvilinear, while the transverse fissures perpendicular to the former at their points of intersection will no longer be parallel. These deviations from rectilinearity and parallelism are due to the action of the general elevatory force, and to the nature of the general eleva-

tion. Others more limited may be due to partial elevations. Where they are observed, the local configuration of the mass must be examined, and thence the directions of the consequent tensions superimposed on that of the general mass must be inferred. The equation of page 273 will then determine the resulting directions of the fissures, and therefore the nature of the deviations. In simple cases, the remarks in page 276-277 will enable us to do this with sufficient accuracy to compare the theoretical deduction with the observed phenomena.

V. It has already been stated, that all consideration of those portions of the earth's crust, in which a regularly jointed or laminated structure may have prevailed previous to their elevation, has been excluded from the investigations contained in my memoir, because I wished to keep them distinct from any speculations respecting the operation of other causes than the one whose effects I proposed to develop. In our general speculations, however, on theories of elevation, it is necessary to consider how far these dislocations which, according to the theory I have been discussing, must be regarded as primary phenomena, are anything more than secondary ones, depending on lines of less resistance, produced by some such particular structure as that above mentioned. Much valuable information respecting joints may be expected from the forthcoming work of Professor Phillips on the Geology of Yorkshire; but at present we know but little accurately about this important feature in the structure of rocks, and of its cause, I conceive, absolutely nothing. We have, therefore, no positive reason to conclude that this peculiarity of structure has generally been superinduced after the elevation of the mass in which it exists, though in some cases there appears little doubt of such having been the fact. It is highly important, however, to determine whether any perfect coincidence does exist in the directions of dislocation, and of joints in the same district; and it is to be hoped that geologists will direct their earnest attention to this subject. Should this coincidence be established generally in districts where the fractures are parallel and at right angles to each other, it will still be important to ascertain how far it exists in elevations approximating to the conical form; and, in all cases, whether the directions of joints bear any relations to the configuration of the mass, as modified by partial elevations. All these are points of interest which we may hope by accurate and careful observation to determine. Should the coincidence, however, between joints and lines of fracture be perfectly established, we shall still have to consider whether the joints, by their prior existence in the undisturbed mass, have determined the lines of fracture, or

[whether] these latter phænomena have exercised an influence in determining the positions of the joints, supposed to be subsequently formed in the elevated mass. Now, assuming the coincidence just mentioned, there must of course be the same relations between the general conformation of the elevated mass and the directions of joints, as those which have been already stated to exist between that conformation and lines of dislocation; and therefore, if we assume the prior existence of joints, and also that the lines or axes of elevation have been principally determined by the points of application of the elevatory force in the lower portion of the elevated mass, we must necessarily conclude, that some relation must exist between the causes which have produced the jointed structure, and the action of the elevatory force; *i. e.* between the action of a force extraneous to the mass, and that internal molecular action, to which it would seem absolutely necessary to refer the formation of joints in the undisturbed mass. To assert such relation to be physically impossible, would, in the present state of our knowledge, perhaps, be absurd; but it does appear to me that the difficulty of conceiving it is so great as to form a most serious, if not a fatal objection, to any theory in which it should be involved as a necessary consequence. To avoid this objection, we might proceed on the hypothesis, that the lines or axes of elevation have been principally determined by the lines of less resistance along the joints, rather than as above supposed; and such might be the case, if the principal line of action of the elevatory force* should not deviate materially from either of the two directions at right angles to each other in which we are assuming joints to exist. If, however, that principal line should approximate to an angle of 45° with these rectangular directions, and should be of considerable length, the hypothesis would be, I conceive, altogether inadmissible.

Supposing then the accurate coincidence of the directions of joints and those of fracture to be hereafter established, it would appear that the hypothesis of the laws discoverable in lines of fracture being generally due to the prior existence of some regular structure in the undisturbed mass, would still involve serious difficulties, on account of the relations existing so generally between the lines of fracture and the configuration of the elevated mass, for which, with the above hypothesis, it

* It must be recollected that we can only judge of the superficial form and dimensions of the mass to which the elevatory force has been applied, by those of the actual elevations. These appear unquestionably, I conceive, to justify the notion of sufficiently determinate *lines of action* of the elevatory force, at least in a sufficient number of instances to give due weight to the argument in the text.

seems almost impossible to conceive any efficient physical cause. On the other hand (still supposing the coincidence of the directions of joints and of lines of fracture), if we assume the formation of joints to have been posterior to the elevation of the mass, this coincidence will still remain to be accounted for. Our ignorance, however, of the process by which this structure may have been superinduced, will not at present allow us to do this. The fact must continue to offer a theoretical difficulty, but one, I conceive, very different in its nature to that above stated, since it would appear, I think, probable that, taking a portion of the elevated mass bounded by adjoining fissures, the position of the joints subsequently formed in it should have some relations to the boundaries of that portion. The fact, therefore, of the coincidence (or rather parallelism) of direction above mentioned, while it presents a difficulty, does not seem to offer any *à priori* objection to the theory which involves it. It is proper, however, to observe that though it should appear, for the reasons now stated, that a preference may generally be due to the theory which would assign the production of fissures to the elevatory force alone, we should by no means be justified in the rejection in every instance of that which attributes the directions of those fissures to the previous structure of the mass, and especially in those cases in which we fail to recognise distinct lines of elevation, or the usual relations between them and the lines of dislocation. And here it may be remarked as a striking fact, that the only mining district in this country in which there is any difficulty (as far as I have yet ascertained) in tracing these relations, is that in which, for independent reasons, it appears most necessary to recognise the influence of a previously veined or jointed structure on the directions of its dislocations. I allude to the mining district of Cornwall.

In the above reasoning I have assumed the *accurate* coincidence of the directions of joints and of lines of fracture, and it is important to observe that this accuracy of coincidence is essential to the theory which would assign the latter phenomena to the prior existence of the former. A difference of a few degrees in the angular positions of the above lines would, if clearly established, be fatal to this theory, because, as I have already explained, although a fissure produced by an elevatory force would cross a line of less resistance under a certain condition, without change of direction, that condition cannot be generally satisfied when the angle between the fissure and line of less resistance is small, and in such case the fissure will be propagated exactly along the latter line. Observations on this point would therefore demand great care and accuracy.

It is also important to remark, that the accurate coincidence above spoken of will require two coexisting systems of joints to be at right angles to each other, since such is the law recognised in lines of dislocation. Observation, however, so far as it has proceeded, appears in many instances, I believe, to contradict this law in the directions of joints. Should any other laws be established hereafter, or very frequent deviations from the one just mentioned, it will probably be found necessary to abandon altogether the notion that lines of dislocation have been principally determined by the directions of joints, rather than by the mode of action of the elevatory force.

The theory which it has been the object of my memoir to develop, enables us to account for nearly all the more important phenomena of elevation; but before we finally decide on its relative claims to our adoption, we are manifestly called upon to remove as far as possible, by accurate observation, the uncertainty which still remains respecting the possible influence of a jointed structure in producing what I have termed the primary phenomena of elevation. These speculations are thrown out with the hope of indicating some of the more critical points of inquiry on which the ultimate determination of this question must turn, and which are generally best indicated in such cases by theoretical discussion. The necessary relations which I have shown must exist, according to one of these theories, between the directions of dislocation and the general, and in some cases local, conformation of the elevated mass, will probably do much towards enabling us ultimately to decide between them; and it is therefore of the first importance that the observer who may hereafter wish to elucidate this subject, should remark these relations as carefully as those which may exist between the dislocations themselves, or the joints with which they may be associated*.

We may here observe, that the only difference between the two theories we have considered, consists in the cause which they assign for what I have termed, with reference to the

* It would be important, as before intimated, to observe the directions of joints in a conical elevation with lines of dislocation diverging from its vertex. I am not aware that the existence of a similarly diverging system of joints has ever been suspected. It would also be highly desirable to observe whether there be any continuity in the joints of two contiguous but distinct formations, and particularly when one formation is primary and the other sedimentary. The perfect continuity of the veins in Cornwall, in passing from the killas to the granite, forms a curious feature in the geology of that district, if we are to regard the former as a sedimentary deposit. In such case, it would clearly demonstrate that the regular structure to which, I conceive, many of those veins must be referred, was superinduced in that district after the great dislocations which must have accompanied the injection of the granite.

theory with which I have been more particularly occupied, *primary* phænomena. The *secondary* phænomena of faults, mineral veins, anticlinal lines, valleys, &c., will be deducible from the primary ones just in the same manner in both theories, so that nearly the whole of the investigations contained in the second section of my memoir will be equally applicable to both these theories.

In that section I have entered, as before intimated, with considerable detail into an examination of the secondary phænomena of elevation, such as anticlinal lines, longitudinal and transverse valleys, ejected and injected horizontal beds of trap, veins of trap and granite; and also the different phænomena of mineral veins, such as the *throw* and depth of a vein, the comparative widths of the best bearing veins and cross courses, and the *shifts* or *heaves* so frequently recognised at the intersections of veins. I have also stated reasons for concluding that the fissures of mineral veins must have been filled by some process of infiltration or segregation (which I profess not further to define) from the surrounding mass; and here, viewing this point with reference to the subject of joints, I would further observe, that the formation of a vein (by which is here meant the matter contained in the fissure) might take place along an open joint, exactly in the same manner as along a fissure produced by any other means. If, therefore, we find veins (such as those before alluded to in Cornwall) which cannot be supposed to originate in the dislocating effects of an elevatory force, we should carefully examine how far the directions of these veins appear to coincide with those of the leading joints. From a hasty inspection of the Cornish veins, I have a strong impression that this coincidence will be found to exist in that district. It would be important to ascertain this fact by careful and detailed observation; for, should it be established, it will immediately destroy the hypothesis of the *contemporaneous formation* of such veins as a necessary alternative, and at least remove one inconceivable process from the speculations of geologists, more especially with respect to those who may at once be disposed to allow this mode of formation of the Cornish veins, while they contend for the fact of the mass in which they are found being a sedimentary deposit. The difficulty of the theory of all similar veins will be reduced by my hypothesis to that of the formation of joints, a process hard enough to conceive, but which has its analogy in that of crystallization, and must of necessity be recognised. The process of contemporaneous formation of veins, without the previous formation of fissures as receptacles for the segregated or infiltrated matter, appears to me inconceivable in itself, and

unsupported by any analogies drawn from the known operations of nature.

VI. There is another point on which I have touched incidentally in the conclusion of my memoir—the application of the principles already explained to the theory of Elie de Beaumont, respecting the parallelism of mountain-chains of contemporaneous elevation. I have before stated, that in whatever manner we may conceive an elevatory force to be produced, there seems no reason why we should not suppose it, in some cases, to have acted at a much greater depth than in others. Now I have already explained (p. 278) the reason for concluding that the formation of a system of fissures, according to our theory, must be simultaneous; and also how the simultaneous formation is facilitated by the circumstance of *time* being necessary for the transmission of the relaxation of the mass produced by the opening of a fissure. From that explanation it will easily be seen, that if a number of fissures commence simultaneously in the lower portion* of an elevated mass of great thickness, and great superficial extent, it is most probable that those only will reach the upper surface which are remote from each other. These fissures will be large, and all the phænomena resulting from them may be expected to be on a proportionate scale. Anticlinal lines† will almost necessarily be formed along them; and thus it is as easy to account for two parallel mountain ranges, as for two neighbouring anticlinal lines on a scale of comparatively small magnitude; and our theory will thus assign a physical cause for the law of parallelism in mountain chains of contemporaneous elevation, as contended for by M. Elie de Beaumont, if, at least, the application of that geologist's theory be restricted within certain limits. I have no intention, however, of now insisting on this extensive action of the physical cause I have been considering; but I would observe, that the extent of this action can only be determined by that of those portions of the earth's surface throughout which the laws of observed phænomena may be continuous, and in accordance with our theoretical deductions.

To persons not habituated to the investigation of the accurate relations between mechanical causes and their effects, much of the previous reasoning may appear too refined to be applicable to our subject; but it must always be recollected, that this reasoning is immediately applied to hypothetical pro-

* I have shown that these fissures must generally commence in some lower portion of the elevated mass. See Memoir, p. 43.

† See Memoir, p. 51.

blems, to which it is strictly applicable, and which are chosen so as to bear the closest analogy to the corresponding ones which nature presents to us; and it is simply on the strictness of this analogy that we are called upon to decide, in judging of the admissibility of our mode of investigation. It is important to have a clear conception of this principle, on which the application of strict analysis to the problems of nature must always be made. In fact, this is the principle on which every one must tacitly (sometimes perhaps unconsciously) proceed, in forming a distinct idea of the necessary relations between any physical cause acting under complicated conditions, and its remoter consequences. We must form our conclusions from the consideration of some comparatively simple but strictly analogous case, and apply them to the actual one, with such limitations as circumstances may require. The advantage which the mathematician possesses, consists in this—that the standard case to which he refers his more complex problem, is a definite one, from which he has means of deducing his results free from that uncertainty which necessarily attends other modes of investigation. It is a standard case of this kind, which I have endeavoured to supply for geological theories of elevation; nor am I without hopes, that the attempt may at least so far succeed as to remove some of that indefiniteness on this subject, by which the earlier speculations in every science must almost necessarily be characterized. More particularly, perhaps, may this be asserted of geology, which, notwithstanding the rapidity of its growth, is yet hardly strong enough to emerge from the cloudiness in which its phraseology alone, with reference to the phenomena of elevation, by addressing itself more to the imagination than the judgement of the student, has sometimes been sufficient to involve it. An impression has thus been too frequently created, that little hope exists of elevating the science to any rank among the stricter physical sciences. Such a notion, however, appears to me most fatal to its healthy progress. The author of the *Principles of Geology*, whatever may be thought of some of his theoretical views, must be allowed by all to have set us an example well calculated to improve in this respect the tone of geological speculation, in as much as he has boldly grappled with the difficulties of his problems in detail, and not been content to meet them with indeterminate generalities. In these investigations I have endeavoured to act upon the same principle, as the only one on which, if we are to speculate at all, we can speculate with safety; and if, perchance, a somewhat vague and misty sublimity which has appertained to this

branch of the science should thus be diminished, ample compensation will be made if we should in return confer upon it a portion, however small, of the more naked dignity of demonstrative truth.

St. Peter's College, Jan. 7, 1836.

LXVIII. *Catalogue of Fossil Fish in the Collections of Lord Cole and Sir Philip Grey Egerton, arranged alphabetically; with References to the Localities, Geological Positions, and published Descriptions of the Species.* By Sir PHILIP DE MALPAS GREY EGERTON, M.P., F.R.S., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following "Catalogue of Fossil Fish" was printed for private distribution, but I am induced to solicit its insertion in your widely-circulated Journal in the hopes that it may prove of interest beyond the immediate pale of my personal friends and acquaintance;—to the geological adept, as exhibiting in a tabular form the stratigraphical position of two hundred and twenty-seven species,—to the student in fossil ichthyology, as affording a clue to the depositories of many new and rare specimens destined to appear in the forthcoming numbers of Dr. Agassiz's "*Recherches sur les Poissons Fossiles*."

To the discriminating eye and classic orthography of that distinguished naturalist I am indebted for the diagnosis and nomenclature of the new genera and species enumerated in the Catalogue, as well as for the identification of such as were already known. I remain, Gentlemen, yours, &c.

Oulton Park, Feb. 6, 1836.

PHILIP GREY EGERTON.

The letter c or e prefixed to a species denotes the Collection to which it belongs when not common to both.

GENUS AND SPECIES.		STRATA AND LOCALITIES.	
	Acanthoderma spinosum		Engi, canton Glaris.
	Acanus oblongus*		Engi, canton Glaris.
c	——— <i>new species not yet named</i>		Engi, canton Glaris.
e	Acipenser, <i>new species not yet named</i>	(Lias.)	Lyme Regis.
	Acrodus nobilis	(Lias.)	Lyme Regis.
c	——— gibberulus	(Lias.)	Lyme Regis.

* This genus is found in the Pläner Kalk.