IV.—ON FAULTS IN STRATA.

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(WITH A DIAGRAM.)

SINCE I first saw my short notice on "Faults in Strata" in print (*GEOLOGICAL MAGAZINE*, 1869, Vol. VI., p. 341), I have been anxious to remove a blemish of apparent flippancy in alluding to *systems* as applied to faults. I had indeed in view cases where that method had been used imitatively—with the tacit assumption of an established and well-understood meaning; but I lost sight of the fact that serious special work had been devoted to the subject, and my remark was unqualified. I have to thank my friend Mr. W. T. Blanford for an opportunity of making this correction, as well as of adding some further considerations on so important a point in the principles of stratigraphy.

In his short answer to my paper (*GEOL. MAG.*, 1870, Vol. VII., p. 115), although Mr. Blanford says he differs from me in opinion, it is very gratifying to me to find that there is far more agreement than disagreement between us upon the fundamental points at issue. Firstly, Mr. Blanford seems to grant that dips are unreliable as proofs of faulting. This admission might have removed the surprise expressed that my objections had not been answered, for all the special instances I impugned were of this nature. No one can doubt that faulting is attended by more or less bending of the strata, and such as, if strictly analyzable, would give a correct account of the mechanical action; but it can no longer be matter of opinion that dips of every form and degree are produced at the contact of rocks without any attendant faulting; and there is no mode of discriminating the causes in the effects. In this difficulty the importance of the direct evidence of the contact is not, I venture to maintain, to be underrated. Mr. Blanford's limitation of possible error from this source to small and unimportant cases is based upon the independent evidence (of straightness) which he conceives always to attend great faulting; on any other grounds this restriction could not be made, for the simulation of disturbance takes place on every scale, as, for instance, in the examples I brought forward from the Sub-Himalaya and the Garo Hills, where the throw, on the supposition of faulting, would amount to many hundreds, or even thousands, of feet.

On another most important point, that of the direct evidence, Mr. Blanford gives me all the satisfaction I could have expected. He says, indeed, that he has not, as a rule, noticed signs of friction in sections of undoubted faults in mines or cliffs; but it is not stated that he had looked for them; and as he now only endeavours to show cause for the removal of such marks, it may perhaps be inferred that there is a presumption of their having been always to some extent produced originally. That this evidence varies greatly in degree is certain: thus, Mr. Mammatt in his "Geological facts to elucidate the Ashby Coal-field," in describing the characters of faults, says, "The two sides of the fault are generally so crushed in slipping

as to resemble a mirror; and for the most part the space between the sides consists of coal, stone, and clay, ground to powder." That great faulting can take place without scoring it is difficult to imagine. It was only in support of this point that I alluded to glacial scratchings; being quite aware of the wide differences between the two cases. Thus Mr. Blanford's first objection to the comparison is rather irrelevant to any point at issue, as it only accounts in the usual way for the scarceness of good contact-sections—a fact of which I have had most wearying experience. The second objection, showing why slickensides should be very generally destroyed in faults, as compared to glacial friction surfaces, is slightly overdrawn: some of the best glacial scorings I have ever seen were on ordinary limestone¹ that had been exposed to the various modes of surface or sub-surface weathering since the Glacial epoch. Nor is it, I think, a general opinion that the action of deep underground percolation is more destructive of form than atmospheric agents; it is on the contrary often eminently preservative, by substituting hard pseudomorphs of what may have been originally in a very perishable condition.

There is, however, no doubt that in many cases very strong direct mechanical evidence of faulting has been preserved, enough (independently of the contrary evidence that may be thus elicited) to require some notice of the contact in every case of an asserted boundary fault.

It is not, I think, implied in my paper that this direct evidence is the only, or an indispensable, proof for faulting: it is even shown that this evidence may be most deceptive as to the amount of throw. I thought it unnecessary to state that my paper had reference only to the special point of contact-phenomena. There are several other considerations connected with faulting to which I did not allude; among them, that of straightness, upon which Mr. Blanford lays so much stress. That I fully recognize the importance of this character is sufficiently shown by the fact that I have mapped and described conditionally as great faults, and chiefly on this evidence of straightness, several features in the Sub-Himalayan rocks (Mems. Geol. Survey India, vol. iii., p. 134, etc.). I should still prefer to reserve the word *direct* for the character of the contact: it is incomparably the simplest, the most self-evident, and the least ambiguous; the appearance described in the passage I have quoted could only be produced by faulting. There is no other kind of evidence of which such an exclusive statement could be made; no one would affirm that a sharply curved or a zig-zag fault is impossible—it is a question of form, in which many conditions are involved. On the other hand, I believe that Mr. Blanford has overstated the case against the occurrence of straightness in orographical features; and that, therefore, this character of straightness, as well as that of dips, is liable to be simulated in natural boundaries. As an instance, I may quote from

¹ Hard Carboniferous Limestone surfaces may frequently be seen in the North of England and in Scotland; beautifully glaciated, but the glaciated surface has usually been protected by a covering of drift or vegetable soil. We should doubt their preservation on soft rock if exposed for long periods to the weather.—EDIT. GEOL. MAG.

Mr. F. R. Mallet's description of the Vindhyan rocks (Mem. Geol. Survey India, vol. vii., p. 106), "The escarpment of the Kymon range, which faces the same for more than 200 miles between Rotasgarh and Bilheri, is nearly a straight line for the entire length without any indentation except the curve at Bidjigarh." Other instances might be given, though of less extent, from the Rajgir and Ghidaur hills. The word *straight* is quite as strictly applicable here as to the coal-measure boundaries; but it is right to note that the latter are in gneissose rocks. In steep ranges, formed along a steady strike, transverse drainage-cuts are apt to be few and narrow: when to such considerations we add from the other side the broken nature of the observations upon which geological boundaries must often be mapped, and that such interruptions and concealments would be most likely to coincide with original indentations of the boundary, I certainly must consider it a safe and necessary principle, rather than a false one, to require some reference to the evidence of the actual contact. No one would question the fact of a fault if he believed the feature to be in fact as straight as it appears when drawn with a ruler on a geological map. But every field geologist knows how rarely it is possible really to vouch for this, so that, unless corroborative evidence is given for a fault, it is difficult for any one who understands the possibilities of error, to accept the feature as demonstrated. I cannot but feel that on the principle here advocated my colleague and I are really at one.

In objecting to my example from the Garo Hills, Mr. Blanford again slightly departs from the special object the case was used to illustrate, viz., the widely different inferences to be drawn from the partial and from the full observations of that single section; without the examination of the contact at that spot, the practice most in vogue amongst geologists would, from the rest of the evidence, assume a fault, and place it at the contact; whereas the contact-section clearly proves that there is no fault there, and as clearly suggests that the great relative movement took place by flexure. No one would consider the question of the whole boundary finally settled upon the evidence of a single section; nor would any one apply the evidence of a clear contact-section to others, unless there were agreement of the more general and easily observed features. It is, on the other hand, admissible to give its full weight to all the evidence in hand; and it would be an unwarrantable restriction to our practical rules not to admit one clear section as a clue in the interpretation of several obscure ones of the same general feature. The evidence of straightness, upon which Mr. Blanford would still introduce a master-fault here, would be in such a case doubly liable to error, for flexures are by their nature as liable to the law of straightness as faults are. Professor H. D. Rogers, who paid so much attention to the details of stratigraphy, went so far as to say that all faults are upon broken flexures. And it must be noted that the natural boundary exposed by the flexure in question is a floor, not an edge, of deposition; and may be a "plane of marine denudation," thus escaping from the veto Mr. Blanford would place upon straight natural boundaries. A de-

scription of my observations in the Garo and Khasia Hills has lately been published (Mem. Geol. Survey India, vol. vii., pt. 1), and it may be noticed what confirmation the view taken of the Garo section receives from the observations of the same feature in the Khasia district, where many clear sections are exposed, exhibiting continuously the great flexure of the strata from the high level of the plateau to the very base of the range (compare sections on pp. 154 and 193).

There is another important stratigraphical character affecting indirectly, but in a very marked manner, the question of faulting. Most of us receive our first impressions of stratigraphy from diagrams and models, and it need not be argued how tenacious first impressions are. The type thus implanted might be called the sandwich, or the bread and butter type of stratification; and, however correct it may be for the very small sections in which it is exhibited, it becomes, as generalized by the green mind, a serious misrepresentation of nature. Thus from such surface indications as those given in the figure (see Woodcut), the unchecked bread-and-butter principle would at once

To illustrate the case of an *assumed* fault, where only a natural boundary (A) exists.
(A.B.C. assumed Fault).

suggest a fault at (A.) the right-hand boundary. If there were any apparently suggestive circumstances, such as an obscure junction, or comparative straightness of the boundary, there would be no escaping the inference, and the full section given would be that represented by the broken lines of the figure (A.B.C.); whereas a stricter examination of the evidence might reduce the features to the more natural cast represented by the curved dotted lines. If such a mistake could occur in this simplest condition of the case, what are not the risks of error when the compression has been very great? As I have already got into sufficiently hot water in search of examples, I must leave the reader to cater for himself.

The word *system*, as applied to features of direction in stratigraphy, will recall pre-eminently to every geologist the illustrious name of De Beaumont. It seems at first sight strange that an important theory—so elaborately worked out, brought forward with the required circumstances of an inductive discovery, steadily maintained by accomplished (although perhaps prejudiced) authorities, with a fair display of accruing verifications (see “Rapport sur les Progrès de la Stratigraphie,” par M. L. E. de Beaumont. Paris, 1869),—should not have received adequate examination at the hands of some competent critic, by a full and impartial comparison with reliable

observations. There are, no doubt, self-evident objections to the theory as it stands, such as the repeated recurrence of disturbance along the same lines of fissure at remote intervals of time; or, the production of intersecting fissures by one and the same force. There is even room for ridicule in the paternal weakness with which almost irrelevant phenomena are attempted to be affiliated and entangled in the cords of the *réseau pentagonal* by its venerable author. But such *critique de tendance* may be left to those who have only words to discuss; what is wanted is to know how much truth there is in this great idea—whether in geological or in primitive times (for men are returning to a belief in a primitive earth), great phenomena of disturbance were subjected to any recognizable law of form; and whether in order of succession these resulted in any symmetry of distribution, pentagonal or otherwise. It may indeed be that the very complexity and elasticity of this system, admitting perhaps of its being framed and supported by a plausible amount of evidence, through its adaptability to almost any evidence, should present a corresponding difficulty to any sufficient refutation; so that we may have to leave it to time to settle its merits. This is at least a convenient opinion for those to adopt who have neither the power nor the opportunity to undertake a full discussion of the subject. And it is no small consolation for such humbler workers to know that the intelligent observation of rock-structure in the field is the most useful way of contributing to the solution of those great problems. It was in this sense that I ventured, in my former paper on faults, to give warning against the temptation to an easily secured semblance of finish by systematizing structural features according to direction, without a due preliminary investigation of the other more essential characters—the special and the local significance of the things so classified.

I will attempt to illustrate the meaning and force of my remark by a brief discussion of the recent endeavour, on the part of a distinguished British geologist, to give an independent value to the element of direction. Professor Haughton's studies on joint-systems and their mechanical origin (*Phil. Trans.* for 1858 and 1864) are more searching than the wider system of De Beaumont; they take up details of structure that the latter author apparently found convenient to overlook. The speculations are based upon the discussion of a large collection of data, and are altogether most instructive; but in so far as applicable to practice they seem to me vague and uncertain; by which I mean that they would scarcely, if at all, lighten the burthen of observation to be undertaken in the investigation of a new field, by enabling one to draw any safe inference upon the more easily obtained evidence.

The leading facts of this inquiry have long been familiar to observers:—that in every region of disturbance certain directions of strike are predominant, and that disturbances are of different ages. The object of further researches has been to ascertain the limits of the various effects of disturbance, and to discover any order in them, or in the relations of the general systems to each other.

Professor Haughton's first and most completely worked example

is taken from the patch of Old Red Sandstone on the coast of the county of Waterford. The rock covers an oblong area of about twenty-four square miles. Its most conspicuous variety is a massive conglomerate, some 200 feet thick, to which, for the sake of greater uniformity, all the observations were confined. Readings were taken of the bearings of 345 well-marked planes of fracture, whether joints or cleavage, faulted or unfaulted. When those readings are plotted, as in the accompanying diagram, no case being omitted, the great majority of them exhibit a marked natural arrangement, not only in single groups or systems, but these again showing a distinct distribution in pairs, in which the mean directions of the two groups are at right-angles, or conjugate to each other; forming together what Dr. Haughton calls a conjugate system. Two such double systems are conspicuously prominent, A C, and A' C': the A groups lie on either side of north-east; and the C groups on either side of north-west. Traces of two other conjugate systems, A'' C'', A''' C''', are discernible. There is, of course, an arbitrary margin in the demarcation of these groups: A and A' have each a range of 25° , while for A''' there is only a space of 12° left. It may be admissible that the range of effects of any force should be proportionate to the intensity of the action; but this variability would be a grave element of uncertainty when the systems were less marked than in the case before us.

The planes of cleavage, of jointing, and of faulting, are separately noticed. A platy or flaggy structure, varying from a quarter to four inches in thickness, was observed, having the direction of the group A, sometimes of A': "now as the lines of greater force in a system of elevated beds must lie in the vertical plane, or nearly so, which contains the direction of the dip of the beds, and as the strike of the beds is intermediate between the systems A and A', it is evident we have a close approximation to Sharpe's law," that cleavage planes are at right-angles to the direction of the greatest force. The aforesaid platy structure, therefore, represents cleavage, as formed in this conglomerate; and thus a key was obtained "to the explanation of the physical structure of the whole district," the direction of the main forces being ascertained. Dr. Haughton considers a more or less pliant condition necessary to the production of cleavage; and that the force which produced it was probably the first that acted upon these rocks.

The true joint-planes are at right angles to those of cleavage; and it is a characteristic of both that the hard pebbles of the conglomerate are cut clean across by them. In the case of the cleavage planes this is explained by the development of a latent cleavage even in the hard pebbles. The jointing is supposed by Dr. Haughton to have been produced by the shrinking of the consolidated rock-mass, consequent upon evaporation and drying, the planes being determined at right-angles to the lines of minimum force to which the rock had been subjected, and therefore conjugate to the cleavage planes.

There are other planes of fracture besides those of cleavage and of jointing. Of twenty cases of faulting, nineteen occurred in the

groups A C, A' C'; and one in C". The chief dislocations are in the groups C and A'. "If we had no other evidence of the physical structure of the conglomerate than is supplied by the foregoing systems of faults, we should be entitled to conclude that two systems of elevating forces had acted, either simultaneously or in succession." "The planes of the elevating forces were probably vertical planes, nearly in the azimuths of 30° west of north, and 33° north of west." The conjugate system A C was principally concerned in producing the inner or latent structure; while the system A' C' was probably the most powerful. "Both were concerned in producing the elevation of the beds, which have an average strike of east 35° north, and an average dip of $13^{\circ}.42$ south. The strike of the beds is thus seen to be intermediate between the systems A and A', but lying nearest to the former, which I conceive to have acted first in order of time." "From the discussion of the planes of structure not belonging to the two principal conjugate systems of the district, I feel little hesitation in coming to the conclusion, that there was at least one more system of forces at work in producing the final structure of the Old Red Sandstone conglomerate:" the system A" C" is represented by fourteen observations of planes having a mean strike of 6° south of east, and twelve planes bearing 5° east of north. There is one observation of a bearing east 15° north, the solitary representative observed of the bearing conjugate to that of C", which is represented by six readings having a mean strike of north 15° west. "Although it is a single observation, I believe it affords evidence (although not strong) of the probable existence in this district of a fourth system of forces having acted upon the conglomerate beds."

The foregoing statements illustrate, in the author's own words, the conclusions he draws from the tabulation of his observations. It may be remarked that much of the exactness, especially as regards the minor systems, may be due to the arbitrary selection of the range of the single groups; thus, the single observation in A" is but 2° removed from the great group A; and three of the twelve observations in C" are much closer to the group A' than to the nearest reading in C". These alterations would, however, only slightly affect the striking distinctness and correspondence of the two principal conjugate systems. But the chief observation to be made, with reference to the object of my paper, is upon the purely abstract nature of the inferences that can be drawn from the systems, as such: every properly geological inference (upon the magnitude, direction, and succession of the phenomena) is based upon independent evidence—the direction of upheaval of the strata, the throw of the faults, the theory of cleavage and jointing. The learned author of the paper is evidently not deceived on this point, for in speaking even of the two main conjugate systems on their own merits he says that the forces which produced them may have acted either simultaneously or in succession. An unguarded reader might, however, suppose that four "systems of forces" represented by four conjugate systems of fracture, indicated four periods of dis-

turbance, instead perhaps of four main components of one great phenomenon of compression.

In the later paper (1864) Prof. Haughton develops his mechanical views more precisely. A number of new observations are tabulated from four localities—Donegal, the Mourne Mountains, Fermanagh, and Cornwall, and compared with those from Waterford. In none of the new regions, except perhaps Cornwall, is the natural grouping of the readings so manifest as in the example from Waterford; so of course there is more scope for arbitrary selection; when the bearings range pretty closely from pole to pole, the formation of conjugate systems is a simple process.

Looking independently at the Donegal observations, the most simple combinations of joints are—a Primary system having a range of 20° , and a mean bearing of 26° north of east; a Conjugate Primary having a range of 20° , and a mean bearing of 29° west of north; and a Secondary system with a range of 6° , and a bearing of 31° east of north. The latter is very nearly the bearing of the A system; and the two former when divided yield two, corresponding very closely with the systems A and C, and two representing the systems A'' and C''. The Primary Systems of Donegal are therefore tabulated as (A-A''), (C-C''). It may be remarked that between the small groups A and A' in Donegal, there are observations disregarded, although well within the range of the corresponding systems in Waterford, and which if included, as one would think they should be, would make the agreement much less remarkable than it is pronounced to be.

In the Mourne region the joints are still more equally distributed from pole to pole; but six systems, each with a range of only 10° , are readily told off, and lettered according to the nearest groups of the original set, many observations being omitted.

The majority of the observations recorded in Fermanagh are readily included in five systems; having the relation of Primary, Conjugate, and Secondary. All the observations in Cornwall are also amenable to a similar order: the main conjugate system corresponding very closely to the system A C of Waterford.¹

It might seem, from the persistent and somewhat forced manner in which the several systems are throughout ticketed in approximate correlation, that Professor Haughton attached some occult meaning to them, analogous to that pervading the De Beaumont theory: but such is not the case; the absence of such agreement, except in the A C system in Waterford and Cornwall, is expressly noticed. The object of the uniform lettering is to exhibit the data upon which the learned author bases his theory of Primary, Conjugate, and Secondary Joint-systems, having angular relations constant within narrow limits of variation. Tables xi. and xii. of the paper under notice exhibit these limits as deduced from the five series of observations. The slightly elastic method of selection of the several systems may have

¹ These observations for Cornwall would seem to be very incomplete: in M. De Beaumont's recent work, already referred to, at p. 553, M. Moissenet gives a range of 60° , with a mean to E.N.E., for lodes of tin; a range of 60° , mean a little north of east, for copper lodes; and a range of 50° , mean a little west of north, for lodes of lead.

helped to the coincidences exhibited by the figures in those Tables; but on the whole they fairly support the interpretations put upon them.

The abstract statement of the theory is very intelligible: when a force acts transversely upon a stratified mass, it tends (by incipient folding or by cleavage) to produce fissures at right-angles to its direction; this is the system which (when discernible) would be called Primary. When a force again acts upon the mass thus fissured its effect will vary: if the component perpendicular to the Primary fissures predominates (a continuation of the first force would be altogether so), the first tendency will be to form cracks at right-angles to the first set, and to be called the Primary-Conjugate system. A continuance of this force, or a force having a predominant component parallel to the Primary fissures, will tend to produce fissures having angles with those systems, respectively dependent upon the coefficient of friction of the rock; thus forming what Dr. Haughton calls Secondary systems, oblique to the direction of the force; and which are in turn conjugate to each other.

Upon applying the known coefficients of friction of various rocks to the construction just stated, the range of variation of the angles is found to agree with that derived from observations between the several conjugate systems. Thus Professor Haughton has achieved a demonstration that rocks in undergoing fracture in the earth's crust are subjected to mechanical laws. "From this it follows that a single hypothesis as to the direction of a system of forces is sufficient to account for the existence of these conjugate systems of joints, involving six directions, and it also explains the rudely hexagonal jointing of many rocks, if the coefficient of friction be such as to render the angle θ nearly 30° ." It is not clear to me how the original force, normal to A, can produce the conjugates to its two Secondary fissure-planes; but let that pass.¹

In thus introducing an intelligible principle of order into a chaotic subject, Prof. Haughton has conferred a great boon upon field-geologists; but I fail to perceive the independent practical importance the author would attach to it. He concludes with this remark:—"Many other consequences flow from the preceding investigations, into which I have not time to enter, but which may be readily found and turned to practical use by the field-geologist who prefers one hypothesis to many." While paying field-geologists the compliment of supposing them as clear-sighted as himself, Prof. Haughton rather passes a slur upon their pursuit, by suggesting that their judgments are for the most part an arbitrary decision between hypotheses. The notion indicates appropriately the geological barrenness of the theory. These systems, manipulate them how we may, give one a very minimum of information upon the rocks. Because a conjugated system of fracture may have been formed by one and the same force, it does not at all follow in any particular case that they were so

¹ The possibility of widely discordant systems of fissures being produced by one and the same act of disturbance, as proved by Professor Haughton, seems completely to stultify such attempts as that of M. Moissenet to identify the fissures of a single locality with many distinct systems of the Réseau Pentagonal.

formed; and it would be rash in the extreme to come to any conclusion on the point without a close search for collateral evidence. Prof. Haughton has himself courageously given us an example of how to work his system independently, and to apply his rule of preferring one hypothesis to many. In a recent paper (Journ. Royal Geol. Soc., Ireland, vol. ii., p. 163, 1868-9) he takes up the lines of elevation described by Mr. Hull in the Carboniferous rocks of Lancashire (Quart. Journ. Geol. Soc. Lond., April, 1868) as forming a simple illustration of his system of Primary Conjugate and Secondary joints—"an easy consequence of the same system of forces acting upon that district, and by no means necessitating the hypothesis of distinct sets of forces for each system of faults or axes." The system of forces that produced the Lancashire axes acted in a north and south direction, causing first the Pendle or Primary axis of elevation; secondly, the Pennine, or conjugate axis of elevation; and, lastly, the lines of fracture or Post-jurassic system of joints. Thus the observed geological order of occurrence of these systems, determined by Mr. Hull, coincides with the order that might have been predicted from the action of mechanical forces."

Now, a brief reference to another paper of Mr. Hull's (Quart. Journ. Geol. Soc., Lond., 1864, vol. xx., p. 242,) describing the Pennine range, will show that the north and south joints in that range are only subordinate features in a great system of flexure, which would, according to the universal practice of geologists, and according to the rule used by Professor Haughton himself in his paper on Waterford, be referred to the action of an east and west system of forces, and perfectly inexplicable by a north and south force.

I can only repeat my caution against the too rational as well as against the irrational use of *systems*.

While thus freely criticizing Dr. Haughton's work, I do so with misgivings. My respect for his intellect and for the powerful instrument he is master of, is so great, that I am quite prepared to be demonstrated in error. However, having studied his papers with all the light that is in me, I feel pretty sure that a large majority of geologists would take the view I have taken; and I am willing to be victimized, that we may all learn.¹

11, HASTINGS STREET, CALCUTTA, 30 July, 1870.

¹ In Mr. Medlicott's former article on "Faults in Strata," the following errata escaped correction (see GEOL. MAG., vol. VI., 1869). Page 341, line 13 from commencement of article, for 'experimental' read *experienced*; second line from foot of same page, for 'implicit' read *explicit*. Page 342, line 21 from foot of page, for 'having' read *leaving*; line 20 from foot same page, for 'instance' read *evidence*. Page 343, line 3 from foot of page, for 'lose' read *box* ("box the compass"); last line same page, dele 'such.' Page 344, line 10 from top of page, for 'marks' read *modes*. Page 345, line 26 from foot of page, for 'equivalents' read *equivalence*; same page, line 7 from foot, for 'run' read *seen*; same page, line 5 from foot of page, for 'their' read *thin*. In justice to the 'Reader,' we beg to explain that the Author's MS. of this article (in 1869) was in a handwriting *exceedingly difficult to read*. It is always desirable that illegible articles should be recopied before they are sent to the printer, as it saves numerous errors. This is especially the case where the author, from his residence abroad, is unable to read and correct his own proof sheets.—Edit. GEOL. MAG.