THE "IRON BLOW" AT THE LINDA GOLDFIELD.

By R. M. Johnston, F.L.S.

At the last meeting of this Society a paper, contributed by Mr. G. Thureau, F.G.S., was read, which calls for some observations from me. Before commenting upon the matters which have caused differences of opinion, however, let me express my sincere regret that any unfortunate remark of mine should have led him to suppose that I do not appreciate the scientific ability of the author of the paper in question. Having said this much, it will, I hope, be granted that the existence of differences of opinion upon geological matters which are obscure may nevertheless exist, and, in fact, continually happen—between the greatest names in science—without questioning the talents or training of those who may espouse irreconcilable opinions.

The differences of opinion as between myself and Mr. Thureau, fortunately, are not of a serious nature, and, according to Mr. Thureau's recent explanation, I perceive they are more due to the confused way in which descriptive terms are employed than to any real differences of opinion. The question between us has been altogether misconceived by Mr. Thureau, and even in his last paper he often leaves me in doubt whether he is referring (1) to the original agencies by which the original metalliferous deposit was formed, or (2) to the causes which produced subsequent modifications. If Mr. Thureau had discussed the Iron Blow question without confusing these two fundamental considerations it would have placed the issues between us in a very small compass. I shall endeavour to keep free from this confusion by discussing the two questions separately:—

I. (*a*) Under what circumstances and by what agency was the fissure formed originally?

(*b*) From *whence* and by what *agencies* were its present altered and unaltered contents derived?

(*c*) By what *mode* were the original matters deposited or obtained?

First, then, we have to enquire—

Under what circumstances and by what agency was the fissure originally formed?

The schists and conglomerates in which the great fissure occurs are evidently of Silurian age, and the forces which operated in dislocating them must, therefore, have been exerted not earlier than this period. From the abundant

evidence at our command of crumpled, distorted, folded, and metamorphosed strata, common in rocks of this age, there is little doubt of the fact that the dynamic forces at work were far more potent than at present, although not different from forces still in operation, whose throes, like those of Krakatoa and Tarawera, are still mighty enough to produce vast local disturbances. There is little doubt in my opinion, therefore, that the fissure at the Linda was originally caused by the same dynamic forces which caused the dilating, folding, and metamorphosis of the crystalline rocks, and that these mighty effects were primarily caused by the gravitation of the outer crust towards the shrinking and cooling central mass of the earth. Mallet's lucid exposition of this theory, many years ago, has convinced the large body of geologists of the reasonableness of this; and I may be pardoned if I cannot discover any flaw in its sufficiency to account for all the dynamical phenomena observable at the Iron Blow.

The next consideration is—Was the opening of the fissure accompanied by the expulsion of heated materials from the interior of the earth by volcanic agency? This brings us to the second part—

From whence and by what agencies were the present altered and unaltered materials derived?

With respect to this question, I am still in accord with Mr. Thureau, for I am of opinion that the expulsion of heated materials from the interior of the earth by volcanic agency has occurred, and to this expulsion may be attributed the *immediate* cause of the opening of the Iron Blow fissure. My original suggestion, that the materials now forming the contents of the fissure does not "necessitate their having been formed originally in the way of 'volcanic mud,'" is incorrectly interpreted by Mr. Thureau as a denial of volcanic action.

This interpretation, moreover, is hardly warranted; for Mr. Thureau is well enough aware that elements such as barium, sulphur, iron and gold, now contained in the fissure are, and may have been, expelled from the interior of the earth as volcanic products by way of sublimation or heated solutions, or by both together or alternately. Mr. Thureau elsewhere admits this, for he states the discharges of the volcanic vents alluded to by him "leave a thin deposit or lamina in the 'cups' at the surface which, after hardening, was found on analysis to be chiefly charged with silica (quartz), and to also contain a sensible percentage of gold and silver." Now this deposit, it is clear by his own showing, was not composed of "volcanic mud" seen in ebulition as "a greyish semi-liquid mass . . . within the mouth of the 'fumaroles,'" but was essentially a *distinct chemical*

deposit formed from associated heated solutions. If, therefore, this be the process—as Mr. Thureau avers it to be—"which assimilates a great deal to what can be seen in its '*dead state*' at our 'Iron Blow,'" it is Mr. Thureau himself who overthrows his own argument, for it is not "volcanic mud" which he likens to the baryta of the Iron Blow, but the silica found as "lamina in the cups" which, without doubt, by his own showing, was formed as a *precipitation from solution!* Where, then, is Mr. Thureau's logic in finding fault with me for preferring to believe the same thing in my statement, quoted by him, viz., "It is probable that the four principal elements—iron, barytes, sulphur, and gold—were originally *precipitated together from solution?*"

That there can be no mistake that the contents of the Iron Blow were considered by him to be the analogues of the silica precipitated from solution, and not the "greyish semi-liquid mass," is proved by the following sentence:—"If baryta is substituted for silica (as matrix?) in the latter case, the question of origin as to both metalliferous deposits is not only, in my opinion, very suggestive, but forms the only possible true solution of the case."

I am, of course, extremely gratified to find in this clear expression of opinion that he thus agrees with me that precipitation from solution is "the only possible true solution of the case;" for while it refutes his "volcanic mud" theory, it more firmly establishes my opinion "that the four principal elements—iron, barytes, sulphur, and gold—were originally precipitated from solution."

Besides this, there is no evidence at the Iron Blow to show that the respective solutions were in any way associated with a "volcanic mud" corresponding to the "greyish semi-liquid mass within the mouth of the fumaroles" of America, of whose composition Mr. Thureau's description does not afford us the slightest enlightenment.

Strictly speaking, mud is a term more appropriately applied to *mechanical mixtures* of various hydrous aluminous silicates, and *such mixtures* are fundamentally different from the definite *chemical compounds*, *pyrites* and *barytes*, which form the characteristic contents of the lode at the Iron Blow.

Causes which produced subsequent modification of materials as originally precipitated.

This part of the subject does not concern me so much as Mr. Ward, who is well able to defend his own views. I may, however, be allowed to observe that Mr. Thureau's denial that the soft and pulverulent combination of iron peroxide and barium sulphate of a deep purplish colour, together with the still more modified massive blocks forming the cap of this

part of the lode, have been derived by subsequent decomposition of the parts more exposed to decomposing agencies, is a most unsatisfactory position for him to assume. It is not true, as stated by him, that the iron pyrites contain "no baryta to speak of." At page 218, "Royal Soc. Proc., 1886," the analysis given by Mr. Ward shows iron bisulphide pyrites, 83·0 per cent.; barium sulphate (barytes), 17 per cent., *i.e.*, actually 2·85 per cent. *less* than the decomposed pulverulent mass, which Mr. Ward, no doubt, rightly attributes to oxidation of pyrites.

Mr. Ward nowhere states that the entire mass of pyrites has undergone decomposition. On the contrary, he refers to the exposed surface of one portion of the original lode. The very fact that the undecomposed pyrites analysed by him was stated to be taken from a section described as two chains wide is proof that this is so. Mr. Thureau's most extravagant allusion to the fissure collapsing in consequence of a partial decomposition is therefore too preposterous to dwell upon. Has Mr. Thureau ever known pyrites, long exposed in lodes to air and water, not to have suffered from decomposition? That both decomposition and recomposition in mineral veins are among the most common of all occurrences cannot reasonably be disputed. Geikie, surely, may be trusted in a simple matter of this kind. At page 597, "Text Book of Geology," he states:—"It has been noticed that the 'country' through which mineral veins run is often considerably decomposed. In Cornwall this is frequently very observable in the granite. Moreover, in most mineral veins, there occurs layers of clay, earth, or other soft, friable, loamy substances, to which various mining names are given. In the south-west of England the great majority of the remarkable minerals of that district occur in those parts of the lodes where such soft earths abound. The veins evidently serve as channels for the circulation of water both upward and downward, and to this circulation the decay of some bands into mere clay or earth, and the recrystallisation of part of their ingredients into rare or interesting minerals are to be ascribed." So much for decomposition. Mr. Thureau, curiously enough, makes no allusion to the remarkable strings and veins of solid barytes penetrating the decomposed part of the lode. He would find it a difficult task to account for these strings on the assumption that they were formed contemporaneously with the pyrites mass, or even with the decomposed portion of the original lode.

Mr. Thureau's inexactness is also conspicuous in his references to baryta. In the first part of his paper, referring to iron pyrites (bi-sulphide), he states that it contains "*no* baryta to speak of," and yet he had Mr. Ward's analyses

before him proving that it actually contained 17 per cent. of baryta, thus:—

IRON PYRITES.

(Section: 2 chains wide.)

	Per cent.
Iron bi-sulphide (pyrites)	83·0
Barium sulphate (barytes)	17·0
	100·0

The only difference of composition between the pyrites and the purple rock is due to oxidation of pyrites, thus:

	Per cent.
Iron peroxide	77·75
Barytes	19·85
Water, etc.	2·40
	100·00

It will be seen, therefore, that the derivation of the one from the other is not such an inconceivable matter as Mr. Thureau was led to imagine from his inaccurate interpretation of the data at his command.

Mr. Thureau again makes a curious reference to the baryta of this purplish rock, in his expression—"Now it is a *fact* that baryta is the 'matrix' of that purple rock." How baryta can be the "*matrix*" of the larger constituent iron peroxide (the latter being nearly four parts iron peroxide to one part baryta) is a puzzle to me.

The word *matrix* is usually employed by geologists to designate the rock or *main substance* in which a crystal mineral or fossil is embedded. According to this meaning of the word, Mr. Thureau is far from correct in stating that "it is a fact that baryta is the *matrix* of that purple rock."

MUD VOLCANOES.

As regards mud volcanoes, there are two well-known kinds, both of which differ widely in characteristics from the phenomena associated with the deposits of the Linda Iron Blow.

The first kind is not volcanic in the proper sense of the term, although variously named *mud volcanoes*, *salses*, *air volcanoes*, and *macalubas*. Geikie describes these as forming groups of conical hills formed by the accumulation of fine and usually saline mud. They are distinguished from true mud volcanoes in having their chief source of movement in the escape of gases due to underlying chemical changes, usually carbon dioxide, carburetted hydrogen, sulphuretted hydrogen, and nitrogen. The mud is usually cold.

The true mud volcano occurs in volcanic regions proper, and "is due to the escape of hot water and steam through beds of tuff or some other friable kind of rock. The mud is kept in ebulition by the rise of steam through it. As it becomes more pasty the steam meets with greater resistance; large bubbles are formed which burst, and the more liquid mud below oozes out from the vent."

These true mud volcanoes, in my opinion, neither in their mode of appearance, nor in their characteristic contents, show the slightest correspondence with the metalliferous fissure lodes of the Linda district.

I may mention that although my examination of the various lodes in this district was necessarily limited, they occupied my close attention for the better part of three days, at a time when they were well exposed by working operations.

Discussion.

Mr. W. F. Ward, Government Analyst, said:—

The point under discussion is the origin of the "formation" known as the "Iron Blow," the oxidised portion of which was described by Mr. Thureau as "volcanic mud or ash." Mr. Johnston, however, from close examination on the spot, and I myself, from the "internal evidence" yielded by specimens, etc., attribute to this a non-volcanic origin.

The materials of this formation are (1) barytes, sulphate of barium, or heavy spar, (2) iron pyrites, or disulphide of iron, (3) hæmatite, or sesquioxide or peroxide of iron.

I will glance briefly at the usual modes of occurrence of each, as showing in the first place that they are *not usually* "volcanic products."

1. "Heavy spar" occurs commonly in connection with beds or *veins* of metallic ore as part of the "gangue" of the ore.

It is found crystallised in the Cumberland hæmatite mines in the carboniferous limestone, and as much as 14 per cent. of sulphate of barium has been found disseminated in hæmatite from another district.

2. "Iron pyrites" is very widely distributed and abundant in rocks of all ages. By the decomposition (by the action of water and air) on the large scale of masses of pyrites, deposits of brown iron ore may be produced, sulphur being lost and oxygen and water taken up by the iron, and a very moderate heat suffices to convert this hydrated brown oxide into the red oxide or hæmatite by driving out the combined water.

3. "Hæmatite" occurs in many forms differing in texture and state of aggregation as: (*a*) crystallised, forming

"specular iron;" (*b*) fibrous, red hæmatite; (*c*) earthy, ochre, but all consisting essentially of peroxide of iron.

In the Cumberland deposits are found hard or "blast" ore, and soft, or "puddler's" ore, from its use in the puddling furnace: the hard, fibrous, and more common form often passing into the crystallised condition.

In Elba, hæmatite occurs crystallised between talcose (or perhaps hydro-mica) schists and crystalline limestone, and the crystals are frequently associated with iron pyrites. It is also found with other minerals as an abundant component of mineral *veins*, also in beds interstratified with sedimentary or schistose rocks.

On the other hand "specular iron" in some cases is a result of *igneous* action, is abundant around some volcanoes; and as pointed out by Mr. Thureau, scales of specular iron were found with 15 other minerals in "ash" from Cotopaxi.

To return to the formation, and quoting Mr. Thureau, we have "An immense bed or vein of solid pyrites filling the greater width of the fissure on its hanging wall, or about 225 ft. out of a total width of 280 ft. between walls of that chasm." Also "A soft purply pulverulent mass of oxide of iron about 56 ft. wide" on the *foot-wall*.

Now, as we have already seen, the pyrites decomposes sooner or later according to circumstances, and Mr. Thureau himself found "elongated and spherical nodules, which on examination were found to contain within hard crusts of sesquioxide of iron (hydrated), nuclei of pure iron pyrites . . . the nodules being in very close contiguity to the massive pyrites vein or bed;" these showing that, as might be expected, decomposition is still taking place.

To the analysis made by me in connection with Mr. Johnston's original paper, I appended a note that "there seems little room for doubt that the 'Iron Blow' is the result of oxidation of pyrites similar to that now associated so largely with it; the hydrated oxide first formed subsequently losing its combined water," and I was not a little influenced in forming this opinion by finding 17 per cent. of sulphate of barium intimately mixed with the pyrites, and 20 per cent. of that substance, *in similar condition*, intermixed with the peroxide of iron. This sulphate of barium Mr. Thureau claims to have "first discovered as the necessary adjunct to the gold." While, however, Mr. Thureau ignores or misquotes the evidence from the presence of this common constituent, and also deprecates forming opinions from the examination of specimens only, he yet advances as a most, if not *the* most, cogent argument in favour of "volcanic agency," the "almost non-auriferous" character of the scraps of pyrites assayed, as contrasted

with the high result of assay of one sample of the oxide of iron. In addition, he calls in to explain the presence of this always irregularly distributed metal gold, as I contend, quite unnecessarily, "a more drastic process of origination than simple and quiescent decomposition only," applying this only to the oxide of iron and not to the bulk of the pyrites which fills four-fifths of the same "chasm."

To return for a moment to the nodules of decomposing pyrites found in the Blow itself, to quote Mr. Thurean again, "these present, neither more or less, former gaseous bubbles surcharged with vaporous sulphuretted solutions of iron becoming rigid when cooled, elongated or rounded by compression." This form is almost certainly also due to decomposition which, acting more rapidly on edges and corners of irregular fragments, more or less rounds them off.

In conclusion, therefore, I maintain that ordinary processes of decomposition are sufficient to account for all the phenomena presented by the oxide of iron portion of the formation, and that there is no necessity to invoke "a more drastic process of origination strictly speaking volcanic."

The Secretary (Mr. A. Morton), read a letter received from Professor Liversidge, Sydney University, in which he stated that his impression formed upon Mr. Thureau's paper, and without having specimens before him, was that the Iron Blow was not of volcanic origin. It would be almost impossible to form a decided opinion without actual examination of the Blow.