NOTES ON THE MOUNT LYELL DISTRICT, TASMANIA.

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On taking a casual glance at the geologically coloured map of Tasmania we are struck with its similarity in appearance to some huge concretion, for we see a nucleus of greenstone, which is surrounded more or less by a ring of the upper coal measures, and these again by rocks from other epochs.

If instead of the geological map we take a topographical one we will observe the same onion-like structure. In the centre we find the Lake Plateau country, from which the river systems of Tasmania radiate. Encircling this are various ranges of mountains, and finally we have the sea coast, which has practically the same contour as the kernel, making due allowance for irregularities, which would be exaggerated by enlargements. Looking at the island as a whole, it is heart-shaped, and somewhat similar in outline to the continents, inasmuch as it is widest at the northern and narrowest at its southern end, its length being north and south. The geological and topographical features coinciding so closely, we are naturally led to the conclusion that they must in some way or other be intimately related. If we can further connect our ore deposits with the physical developments of the country we may gain something by our studies with which the strictest utilitarian cannot find fault.

Orography.

Mr. R. M. Johnston supposes that many of the Tasmanian mountains are due to the shrinkage of the earth's crust; but it has long since been proved that the elevations of our mountains are far in excess of that required by the cooling of the world, and although some slight puckering of the surface may be due to this cause, still I believe it will be generally conceded that other agencies are also at work, and that the presence of the ocean is the greatest of these.

We almost invariably find that the highest mountain ranges have been developed more or less parallel to the past or present sea coasts that are washed by the largest bodies of water. Thus in South America we find the Andes on the west higher than the mountains of the east coast; also that the western shores of America are washed by the Pacific Ocean, which is greater in area and depth than the Atlantic, which is on the eastern coast. Coming nearer home for an example, we find the highest Australian mountains on the east coast, the Pacific again being larger than the Indian Ocean. In Tasmania, however, matters are somewhat different, for there is an unbroken stretch of water from its west coast to South America; while on its east coast, some twenty degrees distant, New Zealand acts as a breakwater, and braces up the ocean, so to speak, relieving the east coast of Tasmania of much pressure from the main body of water. Therefore we are not surprised to find that the backbone of Tasmania is on the western side, especially when we find a map in Wallace's "Island Life" showing the deep water on the west coast at 2,600 fathoms, against 2,000 fathoms on the east coast. We not only have the horizontal pressure of the ocean, but also the vertical pressure, and the force is not a steady one, but varies with the tides, this movement being more effective in shifting the earth's crust than a dead pressure. The pressure of the immense bodies of water in the oceans are working as hard to-day in the building up, and indirectly in the pulling down, of our mountain systems as they have in the past, and it makes its pressure felt second-hand by the enormous weight of the rocks that they pile up, which in their turn exert a vertical pressure. We generally find that the actual coastal range is not so high as the next inland one, as if the force causing the mountain system had limited the area on which the range nearest to it could draw; also that the ranges following the main one are lower and further away from each other as they go inland. So in the case of Tasmania we find the West Coast Range comparatively low. Then comes the Main Range, including the Frenchman's Cap, Eldon Range, and the Cradles, which are among the highest mountains of Tasmania. These are followed up further inland by the King William Range. These ranges are like immense fossil waves rolling inland. They have a long slope on their western side and a steep one on their eastern, just as if they had been pushed up by the sea. Having taken a flying survey of the physical features of Tasmania we will now make a few remarks relative to the mineral wealth of the West Coast, and more particularly to the Mount Lyell district.

Geology.

The West Coast of Tasmania is rich in the variety of its minerals, among which ores of the following metals are found:—Tin, gold, silver, lead, zinc, copper, iron, chromium, nickel, bismuth, wolfram, iridium, antimony, manganese, and mercury; also other minerals of commercial value—*e.g.*, marble, serpentine, asbestos, lignite, etc.

Mount Lyell (named after the celebrated geologist, Sir Charles Lyell)—and its district is a most interesting one from a geologist's point of view, for here we have two or three different types of ore deposits. The most widely known, perhaps, is that called the "Iron Blow," on account of the hugh boulders of hematite (specimen 556) outcropping on the surface. Mounts Lyell and Owen (named after Professor Owen) are two peaks of the West Coast Range, divided by the Linda Valley, but connected on their western slopes by a saddle, which forms a watershed, the drainage on the east flowing down the Linda into the King River, and that on the west finding its way into the Queen River. That portion of the Iron Blow worked by the Mount Lyell Gold Mining Company is situated on the eastern side of the Mount Owen end of the saddle. The country rock about here consists of sandstones, green—(specimen 546) and red (specimen 551), bleached at the surface (specimen 553); various schists—ferruginous (specimen 550), talcose (specimen 548), hydro-mica (specimen 547), etc.; and limestone (specimen 552), which has a schistose structure. The general strike of these rocks is approximately parallel with the West Coast Range, and also the sea coast in these parts. The older rocks are overlaid with a quartz conglomerate (specimen 549), which caps the highest points of Mounts Lyell and Owen.

The Iron Blow Deposit.—Mr. G. Thureau believes this lode to be due to thermal action, but after carefully going over the ground three or four times I failed to find any facts which would confirm this theory.

Some people are pleased to consider this a so-called "true fissure lode," but this fissure theory, which has been applied to the majority of lodes, is fast dying out, for, assuming a fissure to be formed for the sake of argument, any man accustomed to mining is aware that conditions which would enable the walls of that fissure to keep open until the intermediate space was filled up with mineral matter by means of water must be very rare indeed. If the country rock has simply been loosened by some dynamical agency, or even if a fault caused a fracture and displacement of the rock, any cavities due to irregularities of the walls would be filled up with rubbed off portions of the sides which have been loosened by the movement, thus packing the space between the walls like mullock in a worked-out lode. There is, then, no fissure, and the whole affair must be relegated to an exaggerated form of loosened country, the interstices of which are filled with metallic minerals brought in by solution, which cement together the brecciated and powdered rock, sometimes even replacing it. However, the Iron Blow shows no signs of this, but, on the contrary, gives every indication of belonging to that class of deposit known as segregation lodes.

The two prevailing tints that the sandstones and schists assume when not bleached are green and red. Some of the bands of rock appear to have concentrated more iron than others, at the expense of their neighbours, and at times we get very dark shades indeed; in fact, in places it is completely turned into hematite, which retains the same laminated structure that the original rock possessed. This transformation can be traced in its various stages until we come to what we may consider the adult or perfect state, as exhibited in the Iron Blow. This deposit has not been actually traced for any great length, but a similar class of stone has been found here and there on its course so far north as Mount Sedgwick, but whether these outcrops belong to the same layer of altered schist or to parallel ones will not interfere with any interpretation of their segretative character. Although the Iron Blow is composed for the most part of hematite, it is not on account of its iron that it is worked, but because it is found to be the matrix for gold. Besides hematite we also find a fair amount of baryta (specimen 558), but, curiously enough, very little quartz. The footwall is red sandstone, with quartz, and the hanging wall iron pyrites, which we will speak of later on. The general strike is nearly north and south, though locally both strike and underlay vary considerably, which is just what one would expect in a disturbed country like this. The hematite may be divided into two classes, according to its hardness, these occurring in alternate bands, the softer portion looking as if it had been loosened by friction.

We now come to a very important point. There has evidently been some great strain about here, as indicated by cracks in the sandstone and conglomerate filled with quartz, and in the limestone, occupied with calcite; also by the fact that the conglomerate found on the tops of the mountains only occur in broken patches on the saddle. I am inclined to think that as the peaks of Mounts Lyell and Owen were formed the land piled up at those points was robbed from what are now valleys, the dragged out appearance of the country leaving one under that impression. The great weight of these accumulations could not be without effect on the weakened neighbourhood, and when settling down it would have the same effect as if one were to catch hold of opposite ends of a pamphlet and press them towards each other, if I may be allowed to make use of a homely illustration. If this took place after most of the Iron Blow hematite was deposited it would account for some of it appearing to be rubbed up along the lines of least resistance, which are coincident with the planes of bedding and cleavage. It would also account for another deposit adjoining the Iron Blow, though entirely distinct from it. On the hanging wall of the hematite, as before mentioned, is a large deposit of iron pyrites (specimen 555). If we examine the structure of this we find that when undecomposed it is inclined to be schistose, and has a remark-ably close, dense texture. The boundary between this pyrites and the hematite is very sharp, which is what we would expect if due to the cause I suggest; for if such a settling down of the mountains on each side took place after the hard hematite was segregated, there being little cohesion between the hematite and the softer schists, a rupture would most easily take place between them; and the hematite, not yielding so readily to pressure exerted end on to its grain, would break up into fine particles that could occupy any loosened portion of its body; while the country rock, being more flexible, would bend rather than break, and so remain porous until infilitrated mineral solutions filled up all the spaces. The country rock about the iron pyrites deposit occurs at various angles of strike, as if wrapping round it, and the talcose schist illustrates the lenticular nature I have tried to describe on a small scale. There are no signs of sudden rupture having taken place here, but everything points to a gradual motion, which in all probability is continued at the present day. Associated with the iron pyrites we find galena; also copper pyrites, with its resulting secondary minerals—viz., malachite, azurite, cuprite, and native copper. The pyrites deposit occupies a depression or gully running 25 degrees W. of N., the hollow evidently being formed by the decomposition of the pyrites, a large portion of it being dissolved out by water in the form of sulphate of iron, while that which is oxidised into gossan remains behind as a thin capping, until denuded by atmospheric agencies. I account for the pyrites being found so near the surface by the fact that the country here is very moist, a peaty water oozing out of the button grass, the reducing action of which would retard oxidation. I also credit this water with being the agent that reduces the silver and copper which we find in the neighbourhood. The water is so darkly coloured that one almost suspects Neptune of having upset his billy-can of tea. The mass of the pyrites is very poor in gold, but the gossan (specimen 559) or iron capping in places contains a fair amount. This is only reasonable, for a ton of gossan is greater in bulk than a ton of heavier pyrites, and since a ton of gossan must be the product of decomposition of much more than a ton of pyrites, the former will naturally contain the gold concentrated in it, unless washed away mechanically, but the vesicular nature of the gossan tends to prevent that, and the sulphate of iron formed would prevent it being carried away in solution chemically. The decomposed pyrites, when placed on litmus paper and moistened, shows a distinct acid reaction, and this stone, when passed through the battery, unless well leached first and washed with alkaline water, will eat into the plates and sicken the quicksilver.

The Linda Valley Alluvial.—Most of the creeks flowing into the Linda Valley are found to be gold-bearing, as is also the alluvial of the valley itself; in fact, it was by following up the auriferous debris to the foot of the Iron Blow that this now well-known deposit was discovered, and for this reason it is generally considered that the gold found in the low country was shed from the hematite. I have carefully examined some specimens found in the alluvial, which show gold clinging to its matrix. In some instances, it is true, we find the gold attached to hematite, but we also find it sticking out of quartz. However, specimens of either sort are rare, even at the head of the gullies, the gold being, as a rule, clean and in fine grains. Now, I believe that most of the gold and alluvial found in the Linda Valley—and for the matter of that, in the neighbouring creeks also—is derived from the conglomerate which caps the mountains hereabouts. My reasons are these :—

1. There is no earthly reason why gold should not occur in the conglomerate if it were formed from a gold-bearing country, and we know the West Coast to be auriferous in several places.
2. The alluvial at the head of the Linda Valley is water-worn, and composed for the most part of quartz pebbles. In such a position the quartz, if originating from close by, could not have travelled sufficiently far to be so rounded, besides, there is not much quartz in the neighbourhood, the rocks being mostly schists and sandstones.
3. The saddle at the head of the valley connecting Mounts Owen and Lyell has very little conglomerate left on it, though traces here and there can be seen that could not possibly have fallen down from the higher mountains. If the conglomerate originally on the saddle was ruptured and loosened by dynamical strains it would be the more easily disintegrated and washed down into the valley by sub-aerial agencies than the less disturbed and more solid rock on the mountains, which would account for our not finding much conglomerate left on the saddle; then, again, the pebbles composing the gravel of the alluvial in the valley are similar in size and nature to those found in the conglomerate.
4. Boulders of conglomerate can be seen in various stages of decomposition on the sides of the mountains and in the alluvial of the valley. I have washed the product from a decomposing boulder standing isolated on its way to a gully below, and have obtained a colour of gold. On making

enquiries from local miners, two of them—Jack Fehey and Steve Karlsen—informed me that they had also obtained gold from the conglomerate wash at different times.

5. That portion of the alluvial deposit which is composed of broken up schists is very poor in gold; that made up of red standstone drift is better, while that consisting of quartz pebbles is the best, and we seldom get a "duffer" dish from it. At Karlsen's face they average about five grains per lode, while at the Linda Valley claim they estimate four and two-eleventh grains per cubic yard of dirt.

The alluvial at the head of the Linda Valley is evidently not in its original position, as can be seen by the high angle at which it is found, for the gravel, sand, clay and lignite, all of which have different angles of rest, are tilted up to such a degree that proves they could not have been deposited in such a situation. Some people shirk the necessity of explaining the problem of the Linda Valley alluvial by saying that the whole mass has been turned topsy-turvy, but there are no signs of such a general mixing up: the tilting of the deposit from the original angle of deposition is all we require to make the whole affair clear, and such a movement in the earth's crust has evidently taken place here, as the bed rock is thrown out of its normal course. The richest portions of the alluvial is found to be the top and bottom gravels, and wherever certain small, heavy white pebbles, termed locally "Sailor Jacks," occur payable gold is sure to be found. At Delany's face they have obtained good results from the former, and at Karlsen's face first-rate gold has been won from the latter. Some persons suggest that the alluvial is simply conglomerate, which has slipped down bodily from the mountains; but this can hardly be the case, as the alluvial is quite loose, while the conglomerate is very compact; besides, the former is associated with lignite which we do not find on the heights above, thus proving that the gravel was deposited gradually where now found, and that the conditions then prevailing were favourable to a dense undergrowth. The presence of occasional patches of gravel in the lignite shows that the original organic mass grew in shallow places subject to floods, and it is only in such spots that we find gold in the lignite, this going to prove that the gold in the alluvial has been deposited mechanically, and not chemically, otherwise we would expect to find it richer near the lignite, which would tend to reduce it from solution, either by virtue of the organic substance itself, or else by the sulphuretted hydrogen given off by its decomposition. Drift wood may have contributed somewhat to these lignite deposits (specimen 561), but owing to their depth I consider most of the vegetation grew *in situ*; well preserved samples of King William pine (specimen 563), and leatherwood (?) (specimen 562), can in places be picked out, and sometimes the ligneous matter has collected iron pyrites about it (specimen 560). A little cement is now and again to be met with: this in all probability was caused by pyrites being formed, an opportunity being given to the iron present to obtain sulphur from the decomposing vegetable matter, and the more heavily charged water finding its way to the comparatively stagnant bottom would cement the pebbles together. In course of time, as the deposit was drained, this pyrites would become oxidised, as we find it at the present day in Karlsen's face. Many of the streams on the Mount Lyell side of the gully have been worked for gold, until the heavy conglomerate boulders have become too numerous to remove. Besides gold, native copper is also obtained.

Mount Lyell Copper Deposits.—On tracing the alluvial copper to its source we are led to a zone of decomposed schists, forming a pug—grey, yellow, and red—some hundred feet broad. Where cut by the creeks this zone would appear to be equally productive throughout, as the tenacious clay retains most of the copper that is shed on to it; but when we clean up the bed of the watercourses we find the copper is confined to three distinct deposits, which run parallel, from two feet wide downwards. Where the pug is comparatively dry, a little below the surface, it flakes off in pieces corresponding to the cleavage plains of the shale; while the copper is found in sheets, as if occupying the joints and cleavage plains of the original rock. These sheets of metal, which at times are fairly thick, get broken up into small nuggets, shots, and spangles, which are more or less coated with the black oxide of copper. Below the native copper we come across cuprite, which occurs in beautiful crystals, mostly octahedrons, with their edges truncated by faces of the rhombic dodecahedron. No doubt the native copper has been reduced from this by the agency of the peaty waters. Still deeper than the cuprite we come across copper pyrites, which takes upon itself the same form as the rock which it impregnates; in fact, it might be termed a cupriferous schist. Instead of copper pyrites the rock may be charged in a similar manner with iron pyrites, or both. When the latter takes place the copper pyrites is found concentrated on one side and the iron pyrites on the other, and when the deposit crops out at the surface the iron pyrites resolves itself into the oxide, which is sometimes accompanied with pyrolusite.

In presenting the above remarks to the members of the Royal Society of Tasmania I have carefully weighed the ideas and opinions of others who have visited the locality I refer to.

If the glasses through which I have viewed the works of nature will not allow me to agree with the interpretation that other observers have thought fit to make, I cannot be accused of having drawn on my imagination to obtrude a pet theory upon your notice, for I have endeavoured to give a correct description of matters as they at present are found, and to point out how, by using such forces of nature as are to be seen at work at the present day, such results could be brought about, not forgetting that time is an unlimited factor of nature's that enables her to produce results slowly, yet surely, in the world's laboratory, which man, during his short space of life, can never hope to attain by the same means.

DISCUSSION.

Mr. R. M. Johnston, F.L.S., in reply to Mr. Power, said:—Of the many hypotheses advanced at different times to account primarily for the oscillations of the earth's surface it is frankly acknowledged by those who espouse one or other of them that all are open to objections of some kind.

There are three hypotheses, however, ably supported by celebrated physicists, which severally seem to find greatest favour among geologists, some espousing the one and some the rival hypotheses. These three hypotheses are mutually exclusive, and may be briefly described as—

1. The contraction or cooling globe theory.
2. The gradation or surface displacement theory.
3. Mobility of a hypothetic fluid theory.

Undoubtedly the first of these has obtained the greater acceptance among geologists, owing mainly to the able advocacy of Robert Mallet; and although it is admitted that there are serious objections to its acceptance (notably those urged by Mellard Read in his "Origin of Mountain Ranges"), it is even now variously modified, the most widely accepted among geologists as furnishing an explanation of the origin of the upheavals and subsidences of the earth's crust. Such is the opinion of Dr. Geikie in his last great work.

The rival theories, though ably supported, have even greater objections urged against them, and even one of the most formidable opponents of the contraction theory in Australasia, Professor Hutton, cannot urge that it is disproved by the demonstration of the truth of any other hypothesis. In his able address before the A.A.A.S. he states that "During the last fifty years investigation has rather been destructive than constructive, but progress has been made. Formidable obstacles have been removed . . . No doubt the outlook is still foggy, but the horizon is clearing, and we may hope that when we have fuller knowledge of the movements of the crust we shall find a clear explanation of their cause." Until that time arrives we must be content provisionally with that hypothesis which seems to account most reasonably with the greater number of known facts. The proof of a more perfect theory has not yet been made evident.

Mr. Stephens, F.G.S., said that the author of the paper under consideration had evidently paid much attention to the geological formations of a portion of the Western Country, but, in dealing with the question of the history of the mountain systems and the general physical structure of Tasmania as a whole, he appeared to have relied too much on the information supplied by maps, which is necessarily very imperfect. That the chief agency at work in producing those undulations in the primary rocks which had brought into existence many of the mountain ranges and valleys of Tasmania might be traced to secular cooling and contraction of the earth's crust there could be little doubt, and the mountain systems of later date owed their origin indirectly to the same cause, though the valleys had, for the most part, been scored out by the ordinary processes of nature working on the surface of the country. In addition to what had been said in opposition to Mr. Power's water pressure and wave theory, it might be pointed out that until portions of the crust had been raised there was no deep sea, and therefore all that the weight of the water could do, if it had any effect at all, would be to moderate the downward pressure of neighbouring mountain ranges, which are always in a condition of continuous resistance to the forces which had elevated them. With reference to Mr. Power's remarks on the conglomerates of the West Coast, there could be no doubt that, as he suggests, some of the gold in the alluvial deposits has been derived from conglomerate formed from the waste of auriferous rocks. On the other hand it has to be borne in mind that, while there are many conglomerates interstratified with the primary rocks, and likely to contain gold in cases in which the cemented materials have been derived from auriferous veinstones, there are other conglomerates of more recent date which have been derived from bands of quartz rock, quartzite, and associated schists, such as occur in great force at Mount Arrowsmith, Rocky Cape, and elsewhere, in which no gold occurs to any appreciable extent.

Mr. A. Montgomery, M.A., Government Geologist of Tasmania, regretted the absence of Mr. Power, as he would have liked to have had from him further explanation of the new theories advanced. The subject matter of the paper had a very wide scope, comprising two new theories, one of the formation of mountain chains, and a second of the origin of lodes in general, and of the Mount Lyell lode in particular.

Each of these subjects was quite large enough to require a separate paper for its elucidation. Taking the new theory of mountain elevation first, the writer referred to Mr. Johnston's supposition that many of the Tasmanian mountains are due to the shrinkage of the earth's crust, and dismissed the whole contraction theory with the cursory remark that "it has long since been proved" that that explanation was insufficient. He thought that many other geologists would agree with him (Mr. Montgomery) in denying that any such thing had long since been proved. While it might be conceded that other agencies were at work, he did not think either that many would agree that the mere presence of the ocean was the greatest. The subject of the rate of the cooling of the earth and the contraction consequent thereon was a most abstruse one, and there had been great difference of opinion expressed about it. In his mention of the higher mountain ranges, the writer left out of account the great range of mountains extending from the west of Europe to the east of Asia, and right away from any large body of water. The theory enunciated was that the mere weight of huge bodies of water pressing on the crust of the earth forced up a part of the solid crust to a higher level, and he failed to see how such a thing could happen. Water could only raise water to its own level, and could not raise a heavier substance to a higher level. The theory brought forward to account for Mounts Lyell and Owen was a curious one, and he could not see the probability of it, unless indeed the change was ascribed to some grabbing influence from outside the earth altogether, that being the only sort of force that could rake up the material from the valleys and give the country a "dragged out appearance." This and the further reference to the effect of "the great weight of these accumulations" which are no sooner raised up than they begin to settle down again, presumably directly against the force that has just raised them, led one to think that the writer must have based his theory on the supposition that the whole crust of the earth was a jelly, and he suggested that this theory might be known as the Jelly Theory, to contrast it with those generally accepted. The "homely illustration" of the pamphlet was not a good one, as the forces there applied were two lateral ones in opposite directions, while that to be illustrated by it was one force acting vertically. The writer stated that the "fissure theory, which has been applied to the majority of lodes, is fast dying out;" but his impression was that 999 out of every 1000 geologists or men connected with mining would hold that theory as one that met the greater number of cases, and assert that true lodes were almost universally found to be fissure deposits. It was a fact that such packed spaces as alluded to by the writer as being sure to occur if lodes had once been fissures were common in lodes. The difficulties of imagining that a lode fissure could be kept more or less open disappeared when it was remembered that fissures were generally uneven, and that "faulting," even to a very slight extent, would result in a series of more or less connected but irregular cavities. That many lodes were lines of fault was well known. He instanced the case of the Tasmania reef at Beaconsfield, which crosses highly inclined and distinctly bedded strata almost at right angles. In the drive on the lode westward at No. 6 level a band of black rock is found on both sides of the drive for some distance from the shaft. On one side of the lode white sandstone then makes its appearance, but the black still continues on the other side, and it is not till a further distance of 104 feet has been passed over that the white sandstone comes in on it, thus proving a throw of the beds of the country rock of that amount.

Mr. R. M. Johnston expressed general approval with the criticism of Mr. Montgomery, and expressed regret that Mr. Power had not confined his attention to one of the many important subjects introduced and treated it in a more scientific manner.

Mr. W. F. Ward, A.R.S.M., Government Analyst of Tasmania, said:—Mr. Power, in his notes on the Mount Lyell district, takes exception to the supposition that "many of the Tasmanian mountains are due to the shrinkage of the earth's crust," although he admits that "some slight puckering of the surface may be due to this cause." In so doing he joins issue with the great majority of geologists; for, as Geikie, in summarising the causes of upheaval and depression, says:—"With modifications, the main cause of terrestrial movements is still sought in secular cooling" (and consequent contraction). Confining our attention to the Tasmanian mountains, we may say that the highest peaks are one mile in height, the diameter of the earth 8,000 miles; the whole of this cannot therefore be called more than a "slight puckering," and it would on a 2ft. globe be represented by the thickness of thin foreign note paper. As regards the alleged inadequacy of the shrinkage to produce this comparatively slight effect, Mallet, the great authority on this branch of the subject, estimates that the *diameter* of the earth when liquid was at least 189 miles more than it is now; this implies also a shrinkage of the circumference to the extent of nearly 600 miles, so that we seem to have a very ample margin, sufficient to account not only for Tasmania's comparatively small hills, but for such enormous rock masses as the Himalayas and the Andes. Not satisfied apparently with this, however, Mr. Power invokes other agencies for the formation of mountains, and says:—

1. "The presence of the ocean is the greatest of these."

2. "We have not only the horizontal pressure of the ocean, but also the vertical pressure, and the force varies with the tides, this movement being more effective in shifting the earth's crust than a direct pressure."

3. "These ranges are like immense fossil waves rolling inland. They have a long slope on their western side, and a steep one on their eastern, just as if they had been pushed up by the sea."

The nearest approach to Mr. Power's suggestions which I have been able to find, is that of some American geologists, viz. :—"That the removal of rock by denudation from one area, and its accumulation in another affects the equilibrium of the earth's crust, and causes subsidence where deposition takes place, while the denuded area being relieved of weight rises;" this, however, has been fully met by proof that "should the removal and deposit of a few thousand feet of rock so seriously affect the equilibrium of the crust as to cause it to rise and sink in proportion, it would evince such a mobility in the earth as could not fail to manifest itself in a far more powerful way under the influence of lunar and solar attraction." If, then, the pressure of a few thousand feet of rock be incapable of raising the level of neighbouring districts to any appreciable extent, still less can the pressure of water, which has only about one-third the specific gravity of rock, be capable of slowly raising land, and sending it rolling inland "like immense fossil waves." If such were the case still greater would be the mobility and the influence of lunar and solar attraction. With regard to the Iron Blow I have found some difficulty in following Mr. Power's theory; but in view of the close neighbourhood of the iron pyrites and the hematite, of the fact that each is schistose in structure, that each contains much barytes, and that the hematite is on the footwall of the vein or deposit, I am still of the opinion expressed some time back in reply to Mr. Thureau: that the Iron Blow itself is the result of the decomposition of some of this large deposit of iron pyrites. I will only allude to the question of "fissure" veins to point out that Mr. Power quotes no authority against and suggests no substitute for "this fissure theory," as he terms it. In conclusion I can only agree with Mr. Montgomery that Mr. Power, notwithstanding his disclaimer, *has* drawn on his imagination for his theories.