NOTES ON THE MOUNT LYELL DISTRICT, TASMANIA.

By Frederick Danvers Power, F.G.S.

In reply to my critics, I must say that I was rather surprised to find myself credited with being the author of new theories. It is possible that I am the first to apply the views expressed in my paper to Tasmanian geology, but I do not pretend to be the originator of what Mr. Stephens terms the "water pressure and wave theory," or of the anti-true-fissure-lode theory, although I am prepared to uphold them until disproved, or more suitable substitutes are brought forward. In writing my paper I took for granted that the researches of such men as Dr. W. B. Carpenter, F.R.S., and S. F. Emmons were known, and therefore considered it unnecessary to back up my opinions by quoting them and others, in the same way that it would be superfluous to quote Sir Isaac Newton when mentioning gravity.

My paper was intended to give but a sketch of the geology of the Mount Lyell district. To go into detail in the present state of the country would take months of residence on the spot. In making my views public I was aware from Tasmanian literature, as also by personal conversation, that they were in opposition to those generally accepted in the island, but by bringing the results of modern research to bear on local conditions, I had hoped to direct thought out of the groove into which it had fallen. Of course I did not expect to win those who had strong views on the subject over to my side at once, but I did expect them either to give valid arguments for their side of the question, or prove mine incorrect, especially when these sentiments are due to misconceptions on their part. Mr. R. M. Johnston, though expressing his strong opinion against my views, does not offer a single argument in favour of his, or against mine, unless his statement that most geologists have adopted the contraction theory to account for mountain formations can be considered one. In replying I am debarred from elaborating on my subject, as to give it full justice would require more time than can at present be spared. I must, therefore, be content with briefly vindicating my views. The two points on which we differ are :—

1. The main cause of mountain ranges.

2. The nature of most so-called true-fissure-lodes.

In the first place I do not consider that any one theory will account for all our mountain ranges. There are several labourers at work; but my contention is that the chief of these is the pressure of the ocean waters. The other agents are not ignored; due weight has been given to the contraction of the earth, rotation of our world, denudation, sedimentation, etc. Now, I do not for one moment suppose that as our earth cooled it possessed a smooth surface. Our knowledge of molten slags, both natural and artificial, tells us that there are several cooling centres, and these would at once set up currents, which, assisted by the evolution of gases, together with the lunar and solar attractions and the rotation of the earth, would form inequalities on the surface of the globe which served as the nuclei of our continents and oceans; the world would not then be universally covered with water, as I gather Mr. Stephens premises, for elementary elevations and depressions would be formed long before the earth was cool enough to allow water to condense on its surface. Our continental plateau once formed, and water made to occupy the basins, the pressure of that water immediately begins to act and increases as its quantity was augmented. At first, no doubt, the cooling of the crust would have a greater effect in raising land than the pressure of water. Volcanic action would also be a main feature, and the tendency of our rotating earth to throw bodies towards the east, which is made so obvious by our trains and steamboats at the present day, would have a greater effect on the cooling mass than on our more solid crust; but under the conditions then existing disintegration would be more rapid than at a later period, for the rocks would not be protected by vegetation to the extent that they were later on besides, violent storms, humid atmosphere, and the large quantity of carbonic acid in the air would soon work havoc among the hills, forming much sediment which would act on the comparatively thin crust in a somewhat similar manner to which the immense weight of the ocean does now.

Dr. W. B. Carpenter, C.B., F.R.S., in a paper read before the Royal Institution of Great Britain on 23rd January, 1880, entitled "Land and Sea considered in relation to Geological Time," says that the total volume of ocean water to that of land is as 36 to 1; that most land is in the northern hemisphere and most water in the southern; that the bed of the ocean is comparatively flat, not basin shaped; it descends suddenly from a comparatively shallow bottom to a very deep one; that we must distinguish between the real and ostensible borders of the ocean basin, an elevation of 100 fathoms would generally show the real continental platforms, and would join many islands to the mainland, while if the

existing land was depressed to the same amount very extensive areas of what is now dry land would be overflowed by the sea; that the movements of elevation which have occurred from time to time in various parts of the land areas of the globe have been the result of forces acting in different directions—vertical and horizontal; that extensive platforms have been raised by the former, while the latter will throw it into plications of which the elevated portions will form mountain ranges. "Also, the largest mountain chains characterise the borders of the greatest oceans, showing that the lateral pressure from the direction of the oceans was approximately proportional to the extent of the oceanic basin."

Now, the reason I chose to illustrate my views by South American and Tasmanian mountains in preference to the Urals, as Mr. Montgomery would like, was because I preferred to write of places I have visited, and not depend on maps more than necessary. And here I would remark that during my several trips to Tasmania, which have extended north, south, east and west, I have been careful to note the general direction of the main ranges, often climbing to lofty summits in order to attain my ends. However, if Mr. Montgomery prefers to discuss the Urals I am willing, and would direct his attention to the immense tract of low-lying swampy ground forming the Siberian plain to the east of the Urals, and north of the Altai, Yablonoi, and Stannovoi Mountains; also to the inland seas (Caspian Sea and Sea of Aral) and the numerous lakes forming a chain all the way north to the Gulf of Obe. Does not all this remind us that this land has at some time been submerged by the ocean, and would not require much change in its level to be flooded again? We thus see that though the Ural mountains are now away inland, they were once a coastal range, as were the easterly and westerly mountains above mentioned, which also run parallel to their former sea coast.

Tides and currents also have their effect on mountain building, not only on account of the sedimentation which they assist, but also by their own mechanical force. Thus we find the currents of the Bay of Biscay strongest opposite the Pyrenees; where the currents impinge on the Californian coast we find the Sierra Nevada; large currents curl round on the Chilian coast, and there we have the Andes; while coming nearer home we have the main current sweeping along the east coast of Victoria, close to our Australian Alps. The enormous weight of water in our oceans—36 times the volume of land—also tends to press out any inequalities that form on its bottom, and drive them onward towards dry land, where they are relieved of their pressure. To take another homely illustration, as if one were to flatten out a portion of a sheet of paper which

was bulged up in a confined place. In order to bring in a flippant remark about some "Jelly Theory," Mr. Montgomery goes out of his way to apply hydraulic data to solid matter! which proves that he does not understand the situation. A solid body like rock will, as we well know, yield to pressure to a certain extent, and while it is being bent up a considerable amount of friction is developed. If the active force is removed the raised land will not fall back into its original position, for although the weight of the accumulated rock may cause a slight sinking, still the weight is not sufficient to overcome the necessary friction that the greater force was able to do; not only this, but the rock when strained will give in places, so as to ease itself, thus obtaining a state of rest, although out of its original position. We therefore see that this is not a case of balancing a dead weight, but one of steady pressure, and that it is not necessary for the rock to be so mobile as to be affected to any great degree by solar or lunar influences, as Mr. Ward suggests. It is my aim to account for the plications which we see, and know to have taken place; not to prove the earth's crust mobile like a jelly, so that it can have its form changed *ad libitum* like a rubber ball. Assuming Mr. Mallet's calculation of the 600 miles excess of original circumference to be correct, it by no means follows that all this surplus land is heaped into mountains. There are other land surfaces to be accounted for besides these, but even if these 600 miles were heaped up into our present ranges, why should these almost invariably be near past or present sea coasts, and more or less parallel to them, unless the oceans had some great influence over their structure? Again, if due to contraction, it seems rather strange that some of our highest mountains should be built up of comparatively young rocks, which could not have been deposited when most of the earth's crumplings, due to contraction, were formed, *e.g.*, the Alps, in speaking of which Professor Archibald Geikie in his text book, p. 918, writes:—"It is strange to reflect that the enduring materials out of which so many of the mountains, cliffs, and pinnacles of the Alps have been formed are of no higher geological antiquity than the London clay and other soft Eocene deposits of the South of England." I have been accused of advocating new theories contrary to well-known principles. What I have really done is to make use of nature's laws to account for observed phenomena, and not to invoke miracles to my aid. These laws of nature are in force now, and can be seen at work any day. It is no use for us to try and ignore them, for they will exist whether we like it or not.

To return to our local subject. If the ocean has not been the chief cause of the range which includes Mounts Lyell

and Owen, how do my opponents account for its parallelism to the sea coast, and its steep slopes away from the sea; and why have the mountains that dragged out appearance and broken surface? Will not the phenomena observed be accounted for equally as well, if not better, by the views I have expressed than by the simple contraction theory? The pressure of water on the bottom and sides of a vessel containing it is well known; the force of the tides and currents is also appreciated. The effect of the rotation of the earth tending to throw objects towards the east is felt by our express trains and ocean boats, as is also that of winds; the shape of waves of water or sand is familiar as having the steeper side away from the force producing them; where, then, is the absurdity of accrediting the ocean with being the greatest auxiliary to the contraction of the earth in mountain building? Mounts Lyell and Owen show no signs of having been caused by simple denudation; there is no indication of there ever having been sufficient water to cut out the Linda Valley. The conglomerate capping this range was deposited before the mountains were raised, and most of the gravel in the Linda Valley is due to the weathering of the conglomerate, which was cracked and broken as the peaks raised their heads at the expense of the surrounding neighbourhood, for an inclined surface has a greater superficial area than a horizontal one.

Concerning the so-called fissure lodes, I consider the term as generally used misapplied. In our text books we find the fissure theory upheld. My old professor at the Clausthal School of Mines, Herr Dr. Bergrath v. Groddeek, states in his work on Lagerstättenlehre, "Gänge sind ausgefüllte Spalten." V. Cotta in his book writes:—"Since, according to our definition, all true lodes are aggregates of mineral matter in fissures, fissures must necessarily have first been formed and then filled. Both operations may have been independent of each other, and even when this is probably not the case still the formation of the fissure was an entirely different operation from that of their being filled with mineral matter."

Without disparaging text books, from which we learn the A B C of sciences, we really get more information from original articles and discussions on the same by men who have made researches from all points of view. Thus Mr. S. F. Emmons, one of the ablest living authorities on ore deposits, states that:—"Many deposits formerly supposed to have been deposited in open cavities have really been formed by metasomatic interchange of ore with country rock, *e.g.*, lead ore in limestone formations, Leadville, U.S.A. Very many so-called fissure veins in crystalline rocks are formed by percolation water circulating along joints and shrinkage cracks, fault planes or zones of crushed rock, which have filled the

interstitial spaces and replaced the materials of the adjoining country rocks to a greater or less extent by the materials they held in solution, but are not the filling of any considerable open cavities." Mr. Richard Pearce in 1869 noted that the gangue material of the veins of Cornwall was the more or less completely altered country rock, and not foreign material brought from a distance.

Now, rocks may be so strained that faults result, but it is seldom that these faults leave open cavities that warrant the term fissure; for what would otherwise be a fissure is occupied by broken rock at the time of their formation. They are, therefore, not "true fissure lodes," such as may be seen in process of formation at the Steamboat Springs in Western Nevada, but fault lodes pure and simple, which show a brecciated structure, flucan, partings, slickensides, etc., all indicative of motion, and that usually gradual and under great pressure.

My idea of the genesis of the "Iron Blow" was that the hematite was segregated first in a layer of schist favourable for it; that after the peaks of Mts. Lyell and Owen were elevated they sank slightly, when the saddle connecting them was strained; the hematite being hard and strong was enabled to resist this force better than the weaker rock to its west, which, being loosened, located the site for the pyrites deposit. There is nothing peculiar in both the hematite and pyrites containing baryta, since they both, in my opinion, obtained their mineral from the same source, but there is no trace of any intimate connection between these two deposits, as there is a sharp line at their junction, and where we do find the pyrites decomposed it is converted into a gossan or hydrated oxide of iron, not hematite. Although the hematite deposit was not easily ruptured when pressed end on, yet the lateral thrust from the direction of the coast caused strike faults in it, as indicated by the soft rubbed-up portions where one part has slid over another.

Before condemning this opinion in an offhand manner, one should carefully weigh the pros and cons, utilising such knowledge as is possessed to enable a fair judgment to be arrived at. It is not very scientific to rest content with hypotheses because they happen to be generally accepted; we should be active in searching out the truth, and by upsetting one theory we advance one step nearer our goal, so that by opening the above questions fresh points should be gained, either by ridding ourselves of unnecessary theories or by adding fresh facts to our stock of knowledge.