ON THE ORIGIN OF THE IRON ORES OF THE MARQUETTE DISTRICT, LAKE SUPERIOR.

By M. E. WADSWORTH.

[From the Proceedings of the Boston Society of Natural History,
Vol. xx, pp. 470–479, March 17, 1880.]

On the Origin of the Iron Ores of the Marquette District, Lake Superior.

By M. E. Wadsworth.

However distant the Lake Superior district may seem to us, it is eminently fit that its geological problems should be discussed before this Society. The earlier explorers were largely members of this body, and questions relating to the geology, mineralogy, and physical geography of that most interesting region have been debated from time to time here. Of the members of this Society who have been more or less actively engaged in the exploration of the Lake Superior district, we may mention both Agassizs, Desor, Foster, Jackson, Marcou, Marvine, Pumpelly, the brothers Rogers, and Whitney; while on the pages of the Society's publications over forty papers relating to the district can be found.

One of the most important and interesting questions relating to Lake Superior geology is the question of the origin of the iron ores. It is important and interesting not only from a scientific point of view, but also from an economic standpoint. If it be asked how its origin can be determined, we reply that we believe that every geological formation, whether chemical, mechanical or eruptive, contains within itself and in its relations to other rocks, the story of its origin and subsequent history. This story is to be read with greater or less facility, according to the amount of obliteration subsequent changes have produced upon the marks left by the preceding events; also according to our knowledge of, and skill in reading, these characters. It is necessary in such cases to proceed from the known to the unknown. We are to study the structure and relations of rocks,

whose origin is known; and when we find the same characters in other rocks of unknown origin, we are enabled from these marks to determine the origin of the rock of unknown history. Eruptive, chemical, and mechanical deposits are being formed to-day on the earth's surface. Their origin is known so far as it relates to their present position. Other formations have their origin known from historical record. The various known cases give the data for working back into the past history of the rocky crust of the globe. In this study it would seem that the characters and relations of a formation must prove its origin; and it is not allowable to assume because *some* material of a certain kind is being deposited in one way at the present day that *all* material of that kind must have been laid down in like manner, unless its characters and relations are the same. It is not proper for us to decide *a priori* the origin of any formation, until its conditions have been studied. Furthermore it is not allowable to take characters common to formations of unlike origin as proving the rock in question to belong to one instead of the other. We must choose as deciding points those features that are exclusively, so far as known, confined to rocks of one origin. If the diagnostic features are common to rocks originating in two or three dissimilar ways, their discovery in the rock in question only shows that it may have been formed in any of the two or three ways, but does not show which one.

Doubtless some one at this point, if not before, exclaims: "What nonsense! does not every geologist and petrographer know these things? Why should a paper open with such trite and commonplace remarks?" We beg the pardon of our critic, and urge in excuse the fact that these simple and obvious rules have been repeatedly violated in the study of the questions before us. Furthermore the history of geology is fraught with illustrations of the neglect of these and other, perhaps simpler, rules. In applying the principles above given in studying the question before us, it is necessary to study the rocks *in situ* and accept the evidence they there present.

Taking up the problem of the origin of the iron ore and its associated jaspilite it is proper to remark, in order to save time, that except in some few secondary and subordinate cases, they do not, so far as we have observed, present the characters of vein-stones. All writers, so far as we are aware, agree upon this point, and the evidence has been given in their writings; hence it is not necessary to discuss the question here.

It remains for us to examine whether the ore and jaspilite were deposited as sediments *in situ* or are of eruptive origin, since both views are held. That the ore and jaspilite are of common origin and are inseparable parts of the same formation is universally acknowledged. Their interdependence is such that the relations of one to the country rock give the relations of the other.

The question now arises, what are the grounds upon which the sedimentary origin of the ore and jaspilite has been urged by different writers.

The sedimentary origin is advocated on the following grounds:

(1) Bog iron ores are forming at the present day.

(2) On account of the banding or lamination of the ore and jaspilite.

(3) The bandings show foldings and contortions.

(4) The jaspilite and ore are jointed and show cleavage.

(5) The associated rocks are sedimentary and on account of the alternation with schists, the ore and jaspilite, as well as the schists, must be metamorphosed sedimentary rocks.

(6) The presence of phosphoric acid.

1°. Taking up the evidence advanced in favor of the sedimentary origin of the jaspilite and ore in order, we meet first the formation of limonite in modern times. It is urged, since limonite is now formed, that all workable iron ore must have been produced in the same way. Iron ore is produced at the present day by volcanic eruption and by sublimation. Would it then be proper for us to claim that since iron is and has been placed on the earth's surface in modern times by eruption and sublimation, that all workable iron ore in past times was produced in one or both of these ways? Certainly, as fit and right as it is for others to hold, that because bog iron is and has formed on the surface, that hence all past ores must have been formed as bog ore. However, neither of these arguments appears to us to be sound, for the only correct method is to study the deposit in question and decide as to its origin from the facts it presents. It is to be distinctly understood that we are talking only about the deposits in the Marquette district that we have personally studied, and not about others there or those of other districts.

It seems that those who hold to the sedimentary origin of iron ores draw a line between those deposits which can be worked with profit and those that can not, in the present state of commerce. Nature, according to them, evidently drew the line between eruptive and non-

eruptive deposits of iron exactly at this point. It is well known, as remarked by Prof. J. D. Whitney, if iron ores were more valuable than they now are, numerous dikes of basaltic and other eruptive rocks exist that would be worked for the iron ore they contain. We hold that as it is, such dikes have been and are now being worked, only their origin is denied.

In the case of the eruptive rocks the iron is in the state of hematite and magnetite, while it is necessary, if we adopt the bog theory, that the limonite should be transformed into hematite and magnetite in some unknown way. In the eruptive rocks the iron ore, except in places, is subordinate to the remainder of the rock; and in the Marquette district the ore is likewise subordinate to the jaspilite.

Let us again enforce the principle: whether the Marquette ore and jaspilite were formed as eruptive or sedimentary masses can only be told by studying them and seeing if they have the characters of bog ore or of eruptive rock. In this way only are we able to connect their present state with their past. It is not proper to begin with any of the modern formations of iron and theorize back until we reach the older ones, claiming that we have thus proved the way in which the older ones must have been formed. If our logic and science are correct in this *a priori* method, we simply show how the ore *might*, not *must*, have been produced. The *must* comes only from the study of the state of the ore and its relations: facts to which all the theories *must* conform. Again, we remark, we are not to be taken as advocating the eruptive origin of all deposits of iron as others do the sedimentary origin of all, but merely of such as we think show evidence of the former origin. Such ores as show internal and external evidence of sedimentary deposition we accept as sedimentary.

2°. The banding and lamination of the jaspilite and ore do not appear to us to be proof of sedimentary origin, since a similar banding is strongly marked in the rhyolites the modern lavas approaching nearest the jaspilite, in dikes of felsite, in furnace slags, etc. Allowance, too, has to be made for the alterations that have been produced in the rock since its consolidation. This structure is common to both sedimentary and eruptive rocks, hence *per se* is of no value either way. The structure of the banding does often show the origin of the rock when it has been studied with care. Those advocating the sedimentary origin of the above-mentioned ore have rested their claim on the simple fact that the rock was "*striped*," and not on the character of the banding. We have studied the band-

ing and can find nothing in it that proves sedimentation or is inconsistent with that repeatedly seen by us in known eruptive rocks.

3°. The folding and contortion of the banding would take place in any rock whatever its origin, after it was in position, if subjected to proper conditions. A lava flow buried and subjected to the same compression and up-tilting, shows foldings and contortions as would a sedimentary rock in like position and subject to the same agencies. This we find to be the case in the older lava flows. Hence folding and contortion of banding in rocks, like the banding, is common to both sedimentary and eruptive rocks, and like the latter (banding) is no proof of either origin.

4°. Joints and cleavage planes are well known to be common to both sedimentary and eruptive rocks, hence their presence cannot be taken as proof of either origin.

5°. Whoever advanced the view that since the associated rocks were sedimentary, therefore the jaspilite and ore must be, probably intended it for a bit of facetiousness, since he must have been aware that this principle would prove the great majority of dikes and veins to be sedimentary. A dike passing through slate must be sedimentary because the slate is sedimentary! Do we not find rocks intruded through sedimentary ones in every position, both parallel with the stratification and oblique or perpendicular to it? Can any geologist ever have been so ignorant of the mutual association of eruptive and sedimentary rocks as to have soberly advanced the above idea? How then can the alternation of one rock with another be taken as proof that they both originated in the same manner?

It is generally accepted that the old copper-bearing basalts of Keweenaw Point are lava flows. Now they are interlaminated with sandstones and conglomerates. Does this prove that the sandstones and conglomerates are lava flows, or again does it prove that the lava flows are detrital just as the sandstones and conglomerates are? However absurd this line of argument may seem now, it has been applied in the past to the above-mentioned beds on Keweenaw Point. How quickly would the fallacy be seen, if we should claim that the Calumet conglomerate was a lava flow because it was interlaminated between two lava flows? Would such a supposition be any more erroneous than the one advocated for the iron ore, which would make a lava flow on a sea beach, afterwards buried in detritus, to be of the same origin as the detritus above and below it?

It seems that many geologists and most chemists use this course of reasoning regarding all eruptive rocks, except those of recent formation or those whose origin is immediately obvious. Why not study thoroughly in each case, the internal structure and the relations of a rock to its adjacent rocks before deciding upon its origin?

6°. The presence of phosphoric acid could only have been taken as proof of sedimentary origin by those who had no knowledge of eruptive rocks, since it is well known to occur in many of the latter. This has been shown both by chemical and microscopic analysis. Even if this were not the case, the presence of phosphoric acid could not be made proof of the organic origin of the ores of the Marquette district, since they are exceptionally free from phosphoric acid.

We have now taken up all the evidence which we are aware has been used to prove the sedimentary origin of the jaspilite and ore. The characters used as proof seem to be such as are common to both sedimentary and eruptive rocks or are of no weight.

It does not appear that those who advocate the sedimentary origin of the ore have ever made any careful study of its relations, but have rested their claim principally on the "striped" appearance of the rock and the contortion of the stripes. Of the more recent authors who advocate the sedimentary origin, it does not appear that Dana, Hunt, Lesley, Newberry, and Winchell have ever studied the rocks in place, or, so far as their publications show, have ever been on the ground. Of the others, Brooks, Credner, Kimball, and Wright, all except Credner appear to be mining engineers, who at the time of their examination of the region gave no evidence that they had made a special study of the history and origin of rocks, or were even stratigraphical geologists. We claim that questions of the kind above discussed belong to the domain of the petrographer, one who is conversant with both petrology and lithology, and are not in the province of the chemist, mineralogist, stratigraphical geologist, palaeontologist, mining engineer, or lithologist as such.

It now remains to give the evidence in behalf of the eruptive origin of the jaspilite and ore.

The prominent fact to be urged on this side is that wherever the contact of these rocks with the country rock could be studied, that contact was always an eruptive one. The phenomena of the contact of eruptive rocks with other prior existing rocks are well known and not easily mistaken by one familiar with their study, however much they may trouble the chemist, mineralogist, stratigraphical geologist, palaeontologist, mining engineer or lithologist.

The jaspilite and ore are found to break in various directions across the lamination of the associated rocks, to indurate them at the line of junction, to send stringers and tongues into them, to cut the laminae in every direction; in short, to behave always like an eruptive rock and never like a sedimentary one. No theory of deposition in fissures and cavities will account for these relations, since the internal structure and contact relations are not such as occur in this case. Further, it would be necessary to invert the strata several times in order to fill the cavities that exist in the same pit, since they hold every relation to the horizon; and lastly it would require the schists to be a formation of prior age to the iron ores, one that had been deeply buried, metamorphosed, and then elevated before the deposition of the latter.

It seems that Mr. Brooks, a mining engineer, whose authority has been generally followed upon the question of the sedimentary origin of the jaspilite and ore, found himself obliged to admit that in the Lake Superior mine there occurred masses which "appear like dykes of ore, squeezed out of the parent mass, which we may suppose to have been in a comparatively plastic state, when the folding took place; or they may have been small beds, contained originally in the chloritic schist, and brought to their present form and position by the same causes which produce the cleavage in the schist." (Geol. of Mich., I, 139, 140.) We thus see that Mr. Brooks arrived at a point where he was compelled to admit that the ore was in dikes, or at least had been in a plastic state. He further acknowledges that he is "unable to give any intelligent hypothesis of its structure." It seems that the Survey was abandoned at this point, the mines having been previously studied which offered the least obvious difficulties in the way of his theories. Had his work continued there is no knowing what views he might have held.

The efforts to prove the jaspilite and ore to be sedimentary required the assumption that they have, since deposition, been rendered plastic, that is have been protruded into other rocks as eruptive rocks are. This admission involves certain things that appear not to be well established.

1°. We are not aware that it has ever been proved by any accurate, thorough observations by properly trained observers, that a sedimentary rock has ever been made *plastic* by natural causes; in other words, proof is wanting that a sedimentary rock has ever been found with the characters of an eruptive one. The burden of proof yet

rests on those who hold that a sedimentary rock assumes the characters of an eruptive one. Hence we have here an unproved theory employed to prove another theory.

2°. It requires that the highly refractory magnetite, hematite, and siliceous jaspilite, all of which it is denied could have been fused as an eruptive product, should have been rendered plastic and fluent, while the easily fusible argillites and chloritic schists as well as talcose, sericite, and other schists, sandstone and quartzite, neither became plastic, nor showed any signs of it. While rocks of both basic and acidic character, some even of nearly pure silica, are found in contact with ore and jaspilite which show eruptive relations, the former exhibit nothing of the kind. Surely selective metamorphism can do no more than this!

So far as our microscopic examinations have gone the iron ore exists in octahedral crystals when crystallized, whatever may be its present state of oxidation. Also it is difficult to find in our collection any specimens free from magnetic properties. It is to be remembered that Messrs. Brooks and Credner held that the ore was all originally magnetite, and that it in part had been subsequently changed to hematite. The microscope would seem to sustain their conclusion. It is to be remarked that the present magnetic state of the ore appears, in some cases at least, to be directly dependent on the presence of later eruptive rocks. We found that at or near the contact of the ore with such rocks, it (the ore) was strongly magnetic, but at a distance only slightly so. If the ore was originally all magnetite, it certainly was in the same condition in which it abundantly occurs in various eruptive rocks; if originally hematite, it was in the same state as it exists in less amount in some eruptive rocks, particularly acidic ones. So far as chemical objections exist to the presence of magnetite and hematite with siliceous minerals, we can simply say that no eruptive rocks exist but the same objections lie against them. Since all lithologists are aware that magnetite and hematite occur in modern lavas, it seems probable that the difficulty rests with the present knowledge of the chemists and not with the lavas.

We rest our conclusion that the jaspilite and iron ore in the Marquette district are eruptive upon the fact that they possess characters which eruptive rocks exhibit, especially in relation to other rocks, and which no sedimentary rock, proved to be such, has been known to have. They offer no characters inconsistent with those

that known eruptive rocks have, but they do exhibit those, as said before, that no stratified rock has, so far as our present knowledge, not theory, goes.

It is not to be overlooked that the bog theory of their origin demands the following hypothesis, if we adopt the prevailing views. The rocks on which this material was deposited, must have been at that time metamorphosed or indurated to some extent. Hence they must have been originally depressed under the slowly accumulating weight of sediments down to the zone of aqueo-igneous fusion or near it. They must then have been as slowly elevated, denudation removing the superincumbent material. When restored to their original level or near it, the limonite was slowly deposited on their upturned and crumpled edges. Again the sediments accumulated and gradually these rocks with the limonite sank down to the zone of aqueo-igneous fusion or solution, the former passing through it. This selective zone picked out the limonite and siliceous mud associated with it, producing upon them all the marks of plastic eruptive material, which was squeezed in and through the overlying and underlying rocks, the latter remaining unaffected. Denudation again resumed its sway, and slowly and silently these rocks, buried four, five, ten, twenty or thirty miles beneath the surface, again appeared. These likewise are denuded and have deposited over them a conglomerate composed of their metamorphosed debris. Again down they went to or near the same zone when another elevation took place, and the Potsdam sandstone was deposited. We should then have three distinct ages in the "Huronian," all separated by vast intervals of time, instead of one period. Let us then imagine what this sedimentary theory demands in time and realize that this depression and elevation takes place through the influence of slowly deposited sediment on an earth "more rigid than glass and only less rigid than steel." Imagine the power required to elevate this vast area, the cubic miles of sediment to be deposited, and rock to be removed three times over.

On the other side the eruptive origin requires simply that the schists and sandstones should be deposited as usual, and the jaspilite and ore erupted as overflows and intrusive masses. No metamorphism is required or distinct age demanded, for as soon as cooled the rock would have nearly its present condition, and if poured out upon a sea shore would, as soon as solidified, be acted upon by the waves, forming the observed conglomerates.

We are perfectly willing to leave it to geologists to decide which is the simpler hypothesis and which demands least expenditure of time and energy. It is, however, not a question of simplicity, but a question of observed facts and evidence. We are perfectly willing to accept any theory that will explain the facts observed, but in the present state of knowledge we know of no view except the eruptive one that will explain the things seen by us, and we believe that the eruptive origin will explain every fact pointed out by those holding the view that the ore is sedimentary. We believe that the facts observed in studying the region under discussion, in a different manner from that employed by other observers there, sustain the views of Messrs. Foster and Whitney rather than those of more recent investigators.

It is not to be lost sight of, that as it was once fashionable to decide that rocks were eruptive, without evidence; so now it is likewise fashionable to decide that rocks are sedimentary, with like want of proof. As once any statement regarding the eruptive origin of any rock passed unquestioned, so now the similar wild statements about sedimentation pass unchallenged. The reasons for the position taken are rarely asked so long as the popular belief runs in the same direction. The day seems not so far distant as might be supposed, when it will again be as necessary to challenge the statements of those holding plutonic views as it is now those holding neptunian ones. The popular belief in any subject continually oscillates between different opinions like a mighty pendulum, passing and repassing the point of truth. But, strange fatality, if it stops at this point, all is stopped, the works are dead. When truth is reached or discussion ends, stagnation ensues. Again, when the pendulum vibrates, woe be to the man who swings not with it. In all candor we ask geologists to stop and think if the pendulum has not swung decidedly out of the perpendicular on the sedimentary side? Ease up a little, brethren, but do not swing back too far.

Besides the eruptive bosses, sheets, dikes, and wedge-shaped masses occurring both as overflows and intrusions, in the Marquette district, sedimentary deposits exist, as well as others formed from the decomposition of the ore and jaspilite *in situ*. For a description of these, figures of observed occurrences a fuller discussion of the subjects touched upon here, and many others, an historical account and general bibliography of both this and the Keweenaw Point district and a microscopic study of the rocks, the reader is referred to the Bulletin of the Museum of Comparative Zoölogy.