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CONTAINING AN ACCOUNT OF THE ANNUAL MEETING

HELD AT

ANN ARBOR, APIL 2, 3 AND 4, 1913.

PREPARED UNDER THE DIRECTION OF THE COUNCIL

BY
RICHARD DE ZEEUW
SECRETARY

BY AUTHORITY

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LETTER OF TRANSMITTAL.

To Hon. Woodbridge N. Ferris, Governor of the State of Michigan:

SIR—I have the honor to submit herewith the Fifteenth Annual Report of the Michigan Academy of Science for publication, in accordance with Section 14 of Act No. 44 of the Public Acts of the Legislature of 1899.

Respectfully,
RICHARD DE ZEEUW,
Secretary.

East Lansing. Michigan, May, 1913.

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ORIGIN OF CONTINENTAL FORMS. IV.

DISCUSSION OF MR. TAYLOR'S THEORY.

There ore at least five main theories of the origin of continental forms:

1. Unequal radial contraction of a cooling earth.

2. Prof. Chamberlin's theory of the leaching out of basic materials from exposed land and their deposition in the sea.

3. Prof. Suess' theory of the foundering of portions of the earth's crust into the interior of the planet.

4. Mr. Taylor's theory of crystal creep.

5. The theory of the terrestrial loss of mass, developed from the suggestions of Rev. Osmund Fisher, which I have heretofore presented in outline before this section.

There are certain leading facts which any satisfactory theory must take into consideration. For example the following:

1. The present forms of the continents and the low density of their materials.

2. That the margins of the continents exhibit fracture.

3. That many of the fractured borders may be matched together so as to produce a consistent pattern.

4. That when they are so matched together the surface geology in several significant places also forms a consistent pattern.

5. That the Pacific border structures are set in a radial or rotating arrangement.

6. That the Atlantic fractures date from the end of Mesozoic time.

These facts I hold to be fatal to the first three theories mentioned, viz.: contraction, leaching and foundering, and, for the present, their discussion will be postponed in order to take up the theory of crustal creep. Not that unequal contraction and leaching are wholly excluded as possible factors, but they have not been dominant factors.

In 1908, Mr. F. B. Taylor presented in abstract before the Geological Society of America a paper on the "Bearing of the Tertiary Mountain Belt on the Origin of the Earth's Plan." It was published in full in the bulletin of the society in 1910. Briefly summarized, this paper presents the theory that during Tertiary time, possibly in part earlier, the continental sheets of North and South America, Eurasia and Australia experienced slow lateral motion which Mr. Taylor calls "crustal creep." This movement resulted in mountain building along the anterior borders of the moving sheets, thus explaining the peculiar distribution of the Tertiary mountains. North America is thought to have moved southwest away from Greenland, Eurasia southeast from Greenland but for the most part south, South America mainly northwest, and Australia northeast. Africa is thought to have moved eastward but only slight folding and slight elevation toward the eastward are cited. The author believes in crustal creep away from both poles, and is inclined to look for the explanation in some form of tidal force.

Compared with the theory of terrestrial loss of mass as I have developed it, following Fisher and Pickering, Mr. Taylor's theory presents some resemblances and some contrasts. Both recognize certain coastal affinities between the opposite sides of the Atlantic oceans. Both explain these affinities on the basis of former continuity and fracture with separation. Under both, the Atlantic ocean is thus a widened crack, rent, or rift in the earth's superficial materials.

Then come the contrasts. Mr. Taylor's motion is slow, that of the rival theory rapid. Mr. Taylor's is mainly Tertiary in time, my development of the other theory makes the principal motion determine the division between Mesozoic and Tertiary time. Mr. Taylor's theory goes a long way toward explaining the formation of the Atlantic and Arctic oceans, the other would explain the formation of all the oceans. Mr. Taylor's theory harmonizes with current geological sentiment, its rival is a startling and doubtless an unwelcome, reversion to catastrophic violence.

Mr. Taylor's theory has many elements of strength, resting as it does upon analysis of observed facts. Granting the Tertiary age of the mountain belts which border the Americas and, though less regularly, the mass of Eurasia, and granting, what seems clear, that these ranges have been produced by lateral movement due to pressure, we have exceedingly strong grounds for belief in widespread crustal creep of the general nature claimed. Granting further, that the folding is toward the oceans, and that coastal affinities exist, as claimed, on the borders which are not folded, the conclusion seems reasonable that there was crustal creep in the continental sheets away from Greenland and, toward the folded margins. The mountains were doubtless slowly, probably intermittently formed, so the creep must have been timed accordingly and not all accomplished in one comparatively brief action.

In the Tertiary mountain belt, two peculiar features are pointed out which strongly support Mr. Taylor's conclusions,—two "mountain knots," one in Alaska, the other in Peru, where convergences of crustal creep are advanced in explanation of excessive and tumultuous mountain building. These seem to be excellent evidence. Also, in addition to mountain ranges, he is able to point to numerous overthrusts, a few of considerable extent, the most notable example being one of a hundred kilometers in Scandinavia.

While the theory rests upon Prof. Suess' views of mountain structure, to which some may not give unqualified approval, still on the whole this is probably much more an element of strength than one of weakness.

Lines of rifting and parting to the east and west of Greenland are indicated by the coastal contours, less

upon the east side, Mr. Taylor's illustration of those on the west of Greenland, his figures 4 and 5, being especially striking.

I cannot admit that the coasts of Africa and South America fit into the outlines of the mid-Atlantic ridge, as he claims, but as I have previously demonstrated, these two borders fit into *each other* in a most surprising manner if we can bring ourselves to disregard that ridge, and a line of parting is indicated which argues as strongly for Mr. Taylor's theory as do the para-Greenland rifts. As I understand it, the mid-Atlantic ridge is not an essential part of his theory, he merely tries to accommodate it.

Among the "many bonds of union" which, as Mr. Taylor says, show that Africa and South America were formerly united, is the faunal and floral evidence in favor of a former land connection between Africa and Europe and northern South America, along some such lines as have been proposed by von Ihering, Ortmann, Scharff and others. While this instance is only one of several, it is particularly applicable in the present discussion and I have developed this phase of the subject in a separate paper.

But Mr. Taylor's theory is unsatisfactory in several ways.

For instance, I cannot see that there is any place in it for coastal matching together between Africa and North America. As I have demonstrated, and it is easy to verify, the convex northwestern coast of Africa fits into the concave southeastern coast of North America in a manner not less significant than the matching together that is possible between North America and Greenland on the one hand and Africa and South America on the other. There are no mountain structures available to take care of crustal creep in this particular instance. We may pass over related difficulties on small scale in Madagascar, Borneo and other islands, confining our attention to the major features.

In the Atlantic, if we could overlook the above objection, the general evidences of separation might be held to favor his theory almost as much as they do that of loss of mass, but, on a broader view, a seemingly insurmountable difficulty arises in the peculiarly reciprocal conditions in the Atlantic and Pacific. In the latter, we find the coastal evidences of movement set in a radial or rotating arrangement with the Australian focus as a centre and everything indicates converging crustal movement toward that centre as contrasted with the separations, the widening of the Atlantic rift, which lie opposite to it upon the globe. Mr. Taylor believes that, associated with a change in the earth's oblateness, there was outflow of mass from high latitudes toward low the world around, but the lines of motion that he figures, if depicted upon the globe would do about as well for convergence toward the Australian focus as my own. The flow of mass was not alone away from the Greenland horst and from the south polar regions toward the north, it was away from the whole Atlantic ocean north and south and toward the focus mentioned. His theory makes no provision for this peculiar distribution of-deforming force and it does not seem as if anything can explain it so efficiently as does the loss of mass in the region toward which the continental sheets moved. If there is nothing more involved than what he says, all of those peculiar features which I have pointed out in the Pacific hemisphere in this connection have to be disregarded.

There still remains the broad objection that crustal corrugations and overthrusts are inadequate to account for the amount of crustal movement indicated on the globe.

Suppose we take the case which is easiest for the theory, the 560 miles of separation between Greenland and Labrador. Draw a representative line southwest from Greenland, on a great circle, in the direction of the crustal creep of North America. Prolonged, this line strikes the Australian focus. It might be claimed that we have the whole distance from Labrador to the Australian coast in which to account for the 560 miles of creep. Reference to Mr. Taylor's figure 7, however, discloses the fact that he concedes that the foldings from the Australian coast to the Solomon Islands are attributable to pressure *from* that continent instead of *toward* it. As to the mid-Pacific ridges he is in doubt but leans toward a southwesterly, that is an Australian, source for the corrugating pressure. That would restrict us to the space between Labrador and the mid-Pacific islands. Now, since the central portion of North America is known to have remained free from major deformation through Mesozoic and Tertiary time, the Pacific border of this continent is about all there is left to fall back upon. Let us see what measure of crustal shortening may reasonably be assigned to that region.

The amount of crustal shortening in the Laramide range of the Canadian northwest was given by McConnell in 1887 as 25 miles in 50. That is, 50 miles of surface had been gathered into 25. This includes a thrust of Cambrian over Cretaceous of about 7 miles. Taking this into consideration, Dana, 1806, assigns to the Pacific border of North, "Laramide and other systems later than the Archaean, not over 75 miles."

In the Appalachians, Claypole, 1885, found 153 miles (in Pennsylvania) shortened to 65, a difference of 88 miles.

Willis, 1893, in Tennessee, found 72 miles shortened to 54, difference 18, 'which measure he considers "accordant" with Claypole's results in Pennsylvania.

For the Alps, Heim, 1874, found 74 miles of shortening, about the same as that given for our Pacific border.

In regard to overthrusts more particularly, although many are included in the above measurements, a few examples may serve to acquaint us with the general run of values; such are, one in the Laramide range (McConnell) 7 miles; one in, Utah, 4 miles and more (Blackwelder); in Montana one of 5 to 7 miles (Willis); Georgia, 11 miles (Hayes); in Scotland, one of 10 miles (Peach); numerous small ones in Europe and one in

Scandinavia of 62½ miles (Suess). The last is exceptionally large. Overthrusts are abundant in various regions but they are generally small.

To be liberal with the theory, let us grant that there is much more shortening hidden in the Pacific border of North America than has hitherto been supposed. Let us say that we will concede for that one region, in addition to Dana's allowance of 75 miles, Claypole's 88 for the Appalachians and Heim's 74 for the Alps. We secure a total of 237 miles, which is a fraction, over 42% of the 560 miles of crustal creep of North America which we have to account for. More than half, 323 miles, is still missing, so to take care of that we have still to introduce somewhere between Labrador and the Solomon Islands the shortening equivalent of four such mountain regions as the average, (79 miles) of the Alps, Appalachians, and Pacific Border of North America. Recourse must doubtless be had to enormous theoretical thrusts, of the existence of which we have no knowledge whatsoever. I have selected this particular case, because it seems more favorable to the theory than any other. Between the Labrador coast and the Solomon Islands, we have something like 9000 miles in which to find 560 miles of crustal puckering. We do not find it.

If we take the case of western Eurasia, we have not over 2500 miles in which to take care of over 1000 miles of crustal creep. Reference to Mr. Taylor's figure 7 shows how few mountain ranges there are between the British Isles and Africa-Arabia, even including those which run in the line of creep and which were formed by pressures at right angles to it. The great Scandinavian overthrust is not available for western Europe and the Alps are the main reliance. To get upwards of 1000 miles of crustal shortening in between Scotland and Africa-Arabia, or more properly between the English Channel and those southern limits, since England, has no Tertiary mountains, certainly seems more than can be reasonably granted.

Take South America and Africa. The evidence is that their coasts match together and the inference is that they have moved apart at least 2500 miles between Guinea and Brazil and upwards of 1000 miles more than that further south. Mr. Taylor thinks they only parted from the mid-Atlantic ridge, which would give each an excursion of about 1000 miles. Now the case of South America is radically different from that of North America in that the Pacific ridges are not even a little available in explanation of the creep, and he is, I believe, compelled to rely to an undue extent upon the deformation of the western border of the continent. The width of the sheet diminishes toward the south so that the conditions become increasingly difficult. Whereas just below the equator, there is something like 2500 miles in which to place 1000 miles of shortening, further south there is only half that width. Unless the eastern Pacific bottom, and the ocean is very deep for a long distance off the coast of South America, can come to the rescue with an improbable amount of crustal overthrust, we must be prepared to concede to South America as much puckering as that assigned to the Pacific border of North America, 75 miles, the Appalachian 88, the Alps 74, all added together and the total multiplied by 4!

For Africa, it seems necessary to grant also 1000 miles of crustal creep (toward the east) and the whole can be accounted for only by very great foldings and thrusts which have not yet been found in the African land. It is true that exploration is not yet complete enough to settle such a point, but the great resistance offered by the Indo-African mass against the southward pressure of Eurasia, which resistance would lie necessary to produce the observed Tertiary foldings along the southern border of the creeping sheet, this great resistance of itself presupposes that same great stability and rigidity that is indicated by what is known of Indo-African geology. Such a rigidity is to be reconciled with an undue amount of hypothetical thrusting and shortening, if it is to be granted that Africa crept slowly away from the mid-Atlantic ridge in Tertiary time. In fact, Mr. Taylor does not claim that. Be hopes to avoid the difficulty by placing the (eastward) drifting of Africa, before the Mesozoic. How to show the untenability of that position in few words would be very difficult, but it becomes perfectly clear when these crustal movements are regarded broadly in the light of all the facts that I have heretofore presented, that Africa's principal excursion had to be simultaneous with those of all the other continents. I must refer to all the evidence I have compiled and reaffirm that the crustal sheet which is now Africa was up to the end of Mesozoic time structurally continuous with both the Americas whose coast-lines match with it like fragments of broken slate. The general stratigraphical arrangement of its borders corresponds, the surface geology corresponds, and the plants and animals, in general, correspond, (albeit roughly) up to the end of Mesozoic time.

So, as a conclusion from the foregoing investigation, Mr. Taylor's theory that the Tertiary mountain belt and crustal overthrusts explain the crustal movement indicated upon the globe may be said to be quantitatively inadequate. Just how inadequate it is, we have seen in a general way; known foldings and thrusts and probable foldings and thrusts (such as those that may be thought to be submerged in the sea and those that are yet undiscovered on the land) such foldings and thrusts are capable of accounting for only a small fraction of the separation that is indicated by coastal parallelisms. But it is no less important to note that *the theory is adequate for that small fraction*.

I conclude (1) that taken broadly Mr. Taylor's theory is a most valuable contribution to the study of the origin of continental forms.

(2) That it is true that slow crustal creep occurred in Tertiary time and resulted in mountain building substantially as described.

(3) That Mr. Taylor's theory is qualitatively insufficient to accommodate lateral crustal movement as shown by certain coastal parallelisms and by the Pacific convergences.

(4) That it is quantitatively insufficient to account for more than a small fraction of the lateral crustal movement which can be shown to have occurred.

(5) That Mr. Taylor's theory, far from being an argument against or a substitute for the theory of separation of mass from the earth at the end of Mesozoic time, forms a most acceptable extension of it.

What he really shows is how. throughout Tertiary time, the earth's superficial structures went on slowly completing an adjustment, approaching a, new equilibrium after the destruction of the old; how; the earth literally healed itself, filling in and closing up to a wonderful degree the huge Pacific depression.

See how his story corroborates the other! and see how loss of mass, in turn, supplies him with, one adequate first cause which his facts demand!

The various crustal sheets moved, crept, toward the Australian focus, and, if this was due to the existence of a depression there, then clearly, terrestrial gravitation was at the bottom of the matter. Nevertheless there must have been other forces at work too, agitating or releasing forces which became intermittently active through Tertiary time. For mountain building seems not to have been a uniformly continuous process; it was intermittent. It took force to break the continental sheets loose and set them free to move as he shows that they did move; new rifts with separation had to accompany and permit Mr. Taylor's crustal creep; and these events were intermittent, with periods of comparative quiet between. In explanation of this aspect of the problem, I can quote with full approval the closing words of his paper and say that "one is inclined to reject all internal causes and to look to some form of tidal force as the only possible agency."

Just as I have argued that the Tertiary age was ushered in by the removal of mass from the earth and that this separation of mass was caused by extraterrestrial gravitation, to be, for the present, no more explicit, so in the end, Mr. Taylor's crustal creep works out to the same result, *intra*terrestrial gravitation plus "some form of tidal force" as he puts it, which is to say, plus *extra*terrestrial gravitation.

Mr. Taylor has taken a most significant step in daring thus to give expression to a belief that geology must look to extraterrestrial force for the explanation of one of its major problems.

What matters it at this time in what precise form we may severally picture to ourselves this deformation? The grand conception is stated that the greatest deformation of the earth mass since the close of Mesozoic time, the corrugation of the earth's surface with the Tertiary mountain belt, was produced through the action of extramundane force! Even if it be shown, as I believe it can be, that the Tertiary events resulted from the (pre-Tertiary) removal of earth mass, this merely transfers a part of the cause backward in point of time, it does not eliminate it, and so, after all, the conception loses none of its suggestiveness.

HOWARD B. BAKER.

THE SAGINAW OIL FIELD.

BY R. A. SMITH, ASSISTANT STATE GEOLOGIST.

Some years ago Dr. A. C. Lane, then State Geologist of Michigan, wrote an article upon the oil and gas prospects in Saginaw Valley. This article appeared in the Michigan Miner of Saginaw, and created not a little stir in the state at the time, especially at Saginaw. Nothing came of it, however, and the article was forgotten until a reprint of the article fell into the hands of some Saginaw business men in the winter of 1912. They became very much interested in the oil and gas possibilities of Saginaw Valley as portrayed by Dr. Lane and soon interested other business men of Saginaw to the extent that a company, the Saginaw Development Company, was organized for the purpose of putting down three wells, two of which were to be sunk down to the Berea grit, and a third to a depth of 3500 feet or more, unless oil or gas should be struck in quantity before that depth was reached.

Representatives of the company came to Lansing to confer with the Geological Survey concerning all of the available information relating to oil and gas, or other mineral prospects in and around Saginaw. At their request, a compilation and interpretation of the evidence was made, which, though not at all conclusive, was deemed sufficient to warrant a thorough test of the Saginaw territory. Most of the evidence, indicating favorable structural conditions for the occurrence of oil and gas in quantity in the underlying rocks of the region, was derived from the numerous and comparatively shallow drillings for salt along Saginaw river. More or less indirect evidence was also furnished by drillings at Midland, Alcona, Caseville, Blackmar, Flint, Owosso, St. Charles, etc. The two deep wells at Bay City, especially that of the North American Chemical Co., were of great value in giving a general idea of the probable nature and thickness of the deeper lying formations.

The rock strata of Michigan lie one upon the other like a pile of very shallow warped basins, each successively higher basin being smaller than the one immediately below. This basin like structure is known as the Michigan Basin. Obviously it follows that in general the rock layers should dip gently toward the center of the basin which appears to lie somewhere in Midland and Isabella counties. Saginaw and Bay City are to the east of the center, therefore, one should expect the strata on the whole to dip westward. As the general dip in the eastern part of the basin is about 20 feet per mile to the west, corresponding strata at Saginaw should be considerably deeper than at Bay City.

Upon platting the salt wells, it was seen at once that the salt horizons of the Napoleon, instead of being deeper at Saginaw, are fully 200-300 feet higher than at Bay City. In section, the top of the brine horizon of the Napoleon sandstone is seen to rise gradually from a depth of 840-900 feet or more in Bay City to about 610 feet in the Wylie well near Bristol St. Bridge in Saginaw, where they again deepen rapidly to the southwest and west being found at 800-900 feet at St. Charles and more than 1200 feet at Midland. Southeast of Saginaw the Marshall and the Berea also appeared to be much higher. To the north of Saginaw, there is a pronounced upward fold in the Coal Measures as observed in the Ralston well (Sec. 4, T. 13 N., R. 4 E.). The Marshall and the Coldwater also appear to be somewhat higher in the Page Oil and Gas Co. well (Sec. 26, T. 14 N., R. 4 E.) than they are farther east along Saginaw river. At Kawkawlin, the brine horizons occur between 700-800 feet or considerably shallower than in Bay City. From the foregoing evidence, it seemed fairly certain that a pronounced anticlinal fold existed in the rock strata down to the Marshall at least, and presumably much deeper. Apparently this fold should run slightly west of north through Saginaw near Bristol St. Bridge to a point two or three miles west of Kawkawlin. To the southeast of Saginaw at Blackmar, the Marshall and Berea were apparently struck at 360 and 1545 feet and at Flint, 170 and 1200 feet respectively. As this is considerably higher than at points to the east or west of these places, the anticline appears to turn more to the southeast toward Blackmar and Flint, but the evidence is not definite or conclusive, as the records of the drillings at these places are very imperfect. As the Marshall appeared to be only about 360 feet deep at Blackmar, 610 feet at Saginaw and about 700 feet at Kawkawlin, the structure apparently pitched gently to the north. On the whole, the evidence was fairly conclusive that an anticlinal structure existed, but its exact position and direction were not so clearly indicated.

The first well was put down by the Saginaw Development Company near the SE. Cor. of the NW. ¼ of NW. ¼ of Sec. 27, T, 13 N., R. 5 E., Buena Vista township, Saginaw county, although from the evidence then at hand, this location appeared to be considerably to the east of the supposed anticlinal. In this well, known as the Mundy-Fifield, the Marshall was found at 780 feet or 40 feet higher than in the South Bay City well (Sec. 5, T. 13 N., R. 5 E.) but fully 170 feet lower than the supposed depth to the Napoleon near the Old Wylie well on Niagara St. about 800 feet north of Bristol St. Bridge. The Berea was struck at 2070 feet or 30 feet higher than in the South Bay City well. All of the underlying formations were correspondingly higher than the respective ones in the Bay City wells and much lower than at Saginaw, as was afterwards shown by later drillings. From the first it was obvious that the well was located well down the east limb of the anticline. The Berea yielded some brine and a little gas, but no show of oil. At the depth of 2246 feet the drilling was abandoned, but afterwards it was deepened to the Traverse oil horizon, which was struck at 2520 feet or 90 feet higher than at Bay City and 200 feet lower than at Saginaw. There was about 20 feet of the "sand" but no oil or gas. The well is now being drilled to the Dundee but the chances for finding oil and gas do not seem at all favorable. The well is located too far from the anticline.

Another well was started near the site of the old Wylie Bros, well on what is known as the Garey-Casamer lease. Here the Upper Marshall or Napoleon was found at 510 feet or 170 feet higher than in the Mundy-Fifleld well and 210 feet higher than in the South Bay City. The Berea was encountered at 1835 feet or 235 feet higher than in the Mundy-Fifield and 265 feet higher than at Bay City. These drillings not only conclusively proved the supposition that an anticlinal undoubtedly exists in the rocks down to the Berea but also that it even becomes more pronounced with depth. Of course, due allowance must be made for thickening of some of the formations.

Here, as in the Mundy-Fifield well, the Berea proved to be a small yielder of brine, perhaps 25 or 30 barrels per day, and of gas. Drilling was continued with the idea of going on down to the Dundee, supposed to be 900-1000 feet below. At 2305 feet the Traverse limestone was entered and oil of the finest grade was struck at 2317 feet in a sandy or cherty limestone. The so-called "sand," since called the "Saginaw Sand," was very thin, being probably not much more than two feet.

The well made two flows of some 40 to 50 barrels of oil altogether. The indications were that a 25 to 30 barrel well had been struck. The well was shot with 100 quarts of nitro-glycerin. As soon as pumping began, however, it was found that the casing was leaking, and after the first 75-80 barrels, the production of oil fell off rapidly until the well made only about three or four barrels of oil per day with about 25 barrels of water. After many attempts the water was finally shut off through the use of rubber packers, but the production was not materially increased. Finally the well was reshot with 20 quarts, of nitro, but the casing was loosened, so that water was again troublesome. The well can hardly be said to have had a fair chance to develop its possibilities.

The next well, No. 3, was drilled on the Jackson-Church property near the west end of Bristol Street Bridge on the west side of Niagara Street and about 600 feet west of south from the Garey-Casamer well. The Marshall, Berea, and the Traverse were approximately at the same depth, as in the latter well, but there was no porous sandy limestone or oil "sand" in the top of the Traverse. The "Saginaw Sand," as the oil horizon in the Garey-Casamer well had been called, appeared to have pinched out, and there was not the slightest show of oil or gas at this horizon.

The well was drilled down to the Dundee which was found at about 2900 feet. At 2935 feet there was a show of oil which did not show much increase until at 2945 feet where the greatest showing was made up to 2955 feet. The drilling was stopped at 3080 feet where some brine was struck. This was plugged off and the well shot

with 120 quarts of nitroglycerine. The first pumping is said to have yielded some 50 barrels of very good oil and then the production rapidly fell off to about 2 to 3 barrels per day.

A fourth well, about 1200 feet south of east from the Garey-Casamer, was sunk to the Dundee just at the east end of Bristol Street Bridge on the Cresswell property. The formations in all three of these wells in Saginaw were at approximately the same height. In the Cresswell well, a showing of oil was struck in the "Saginaw Sand," another about 100 feet lower in the Traverse, and a third toward the base of the formation. The oils in the second and third horizons were dark and heavy, and of much lower grade. None of these showings were deemed to be worth testing and the well was deepened to 3060 feet, the Dundee being struck at 2886 feet and a showing of oil found down to 2942 feet. The oil was high grade, being similar to that foound in the Jackson-Church well

Although the well has been drilled in for some time, it has not been shot owing to some difficulty over the responsibility for the public safety. The appearances seem to indicate a small pumper similar to the Jackson-Church.

Well No. 5. 3068 feet deep, was drilled on the Watson farm near the SE. Cor. of the SW. ¼ of SE. ¼ of Sec. 17, T. 12 N., R. 5 E., Buena Vista township, just about a half mile east of the city close to the intersection of the highway and the new electric railway from Saginaw to Bay City. There was a small show of gas in the Berea, but no showing of either oil or gas was reported in the Traverse. Oil was struck in the Dundee, but in such small quantity, that after standing six days, there was only a few gallons of oil in the well and about 180 feet of water. The well which was at once abandoned without shooting was plugged to protect the Marshall brines from contamination and also to prevent leakage of gas which was made in considerable though not commercially important quantity from the Berea. The latter also yielded some very strong brine, perhaps 25 to 30 barrels, per day. The brine constantly flowed over the top of the casing in a small stream.

The Upper Marshall or Napoleon was struck at 600 feet or slightly higher than near the Bristol Street Bridge drillings. The Berea was struck at 1835 feet or practically the same depth, but the Dundee was 2928 feet or about 28 feet deeper. The structure appears to be that of a structural bench or terrace for the upper formations and that of an asymmetrical anticline for the lower. In the latter case, the western limb is very much steeper than the eastern which dips but 28 feet from the Jackson-Church to the Watson well, a distance of 2½ miles. From the Watson east to the C. G. McClure well, recently put down to about 3000 feet near Gera in Sec. 8, Frankenmuth township, the rocks seem to be nearly flat, as the formations are reported to be at practically the same depth. Farther east the rocks must begin to rise up the side of the basin, as, at Beese, Tuscola County, the Napoleon is probably less than 500 feet from the surface. The broad flat depression in the Berea to the east of Saginaw evidently dips toward the north and becomes much more pronounced as the top of the Berea drops from 1850 feet in the Watson to 2035 feet in the Mundy-Field. The syncline in the Dundee becomes still more pronounced in the same direction.

Well No. 6 or the Green Point well was located on the Globe-Blaisdell farm nearly opposite from the East Saginaw waterworks plant. This well is situated a little south of west from the Garey-Casamer and about 2¼ miles distant. The formations were reported to be 100-150 feet deeper than in Saginaw and there was not the slightest show of oil at any of the horizons, even though the well was apparently drilled nearly to the base of the Dundee, where much water was encountered.

Well No. 7 was located near Lawndale some four miles northwest of the No. 2 well in the NE. Cor. of Sec. 5, T. 12 N., R. 4 E. This well was also located at considerable distance to the west of the supposed crest of the anticlinal and as drilling progressed, this proved to be the fact for all of the formations were reported to be practically at the same depths as in the Mundy-Fifield. The Berea, Saginaw sand, and the Dundee would therefore be found respectively at about 2070, 2520 and 3150 feet.

THE OIL HORIZONS.

From the data assembled before drilling began, it appeared that the Berea was the most promising horizon to test for oil and gas. At Bay City, there were strong signs just above the horizon and also, in the Blackmar well, considerable gas was yielded from this formation. A favorable structure seemed to be the only thing lacking for an accumulation of commercial size. It was a keen disappointment when the Berea in most of the wells yielded only a small quantity of gas, which, however, increased considerably with time. In some wells, the gas was not noticed for some time after the Berea brine was cased off. The latter, though apparently very strong was also deficient in quantity,—there never being a flow of much more than about 30 barrels per day. For some time, it was a puzzle why this formation did not yield a greater abundance of brine, as it was so thick and well represented. An examination of the samples showed that the Berea is a very fine and close grained gray to white sandstone. In some phases, the sand grains are too fine to be readily distinguished by the naked eye. It is this fine to exceedingly fine grain of the rock which not only limits the flow of brine but also that of the gas. A heavy charge of nitro-glycerine might possibly loosen up the rock enough so that a considerable flow of gas could be obtained from the Watson well or from some of the other wells. The head of brine might overpower it, however.

The so-called "Saginaw Sand" appears to be a sandy or cherty limestone. There seems to be some doubt as to the exact nature of the oil horizon, but all of the samples examined by the writer have proved to be true

limestones though often cherty or sandy, effervescing very vigorously and leaving a comparatively small residue of sand and chert. At any rate, the "sand" is thin and pinches out wholly to the southwest in the Jackson-Church well. In the Cresswell or No. 4 well, the sand was represented by two sands,—that is, two cherty limestones separated by a shaly layer. In the Watson well, there was a cherty or sandy pyritous limestone filled with black micaceous particles from 2325 to 3469 feet.

The Dundee oil horizon appears to be some 35 feet below the top of the formation and is a light gray to buff and brown granular limestone effervescing violently with dilute hydrochloric acid. A fragment of the limestone shot out of the Jackson-Church well was a gray granular porous limestone, the pores being readily seen by the naked eye. The oil horizon appears to be free from water, but, lower down at no great depth, there is an abundance as shown in the Jackson-Church, the Watson, and the Green Point wells.

EXPLORATION.

Of the eight wells now completed in the Saginaw field, including Mr. C. McClure's Gera well, only three (Nos. 2, 3 and 4) appear to be on the anticline and these three, perhaps significantly, yield oil and gas in considerable even if not commercially important quantities at two different horizons. The Cresswell hole showed four oil producing horizons. The other wells were all located from about half to three or four miles distant from the apparent crest of the anticline and made little or no showing of either oil or gas. The Watson or No. 5 perhaps may be excepted as this well yielded considerable gas at the Berea horizon. This well, however, is very close to the anticline,—so close that apparently it ought to have made better showings of oil than it did.

The three wells yielding the most oil are within a radius of 1200 feet, near the Bristol Street Bridge. The indicated anticline is 25 to 30 miles long so that the three drillings near its crest in no way have tested the possibilities of the structure. Since the drillings at some distance from the anticline have been so barren of any encouraging results, it seems most logical that future prospecting should be along the supposed axis of the structure. Oil may not be found in quantity along its crest but, most certainly, the chances are presumably greater in its immediate vicinity than elsewhere.

Lansing, Mich.

A GEOGRAPHIC STUDY OF THE GROWTH AND DISTRIBUTION OF POPULATION IN MICHIGAN.

BY O. W. FREEMAN.

The history of a nation or state is largely the result of its own and its environment's geography. A study of Michigan's history, and of the reasons for the distribution of its population at different times, shows that certain geographical factors have influenced the history and settlement of Michigan far more than any designs or purposes of man. This paper is devoted to a brief discussion of the factors which influenced the distribution of population in our state.

Michigan was the first of the North Central States to be explored and to have permanent settlements established. These early settlements were mere missions or trading posts, whose location was determined by the presence of the abundant waterways of the state, which served for cheap and easy transit of trading goods and furs, and accounted for the founding of Detroit, St. Ignace, Sault Sainte Marie, and Fort Miami on the St. Joseph River near what is now Niles.

Although explored so early, there was no influx of an agricultural population until after about 1818. There were several causes for this, chief among them being (1) Michigan's earliest settlers were fur traders; fur trade and agriculture never go together; (2) The rich easily broken lands along: the tributaries of the Ohio would attract farmers first; (3) Erroneous reports were abroad to the effect that Michigan's surface was largely swamps and pine barrens, and, until more careful surveys were made about 1820, this idea, fostered perhaps by the fur companies, may have helped to turn settlers elsewhere; (4) Distance to markets made farming unprofitable. These geographical factors then proved sufficient to prevent any increase in Michigan's population for over a century after the founding of Detroit. In 1820 almost the entire population of Michigan was restricted to a narrow belt close to Detroit and Lake Erie. In 1825 the Erie Canal was opened; this formed a water route to markets. By 1830 the districts around Detroit became more densely populated, and settlements spread toward the North and West. For a few years, the Kankakee-St. Joseph river route served to encourage the settlement of the extreme Southwestern part of the state and some adjacent portions of Indiana, although the area about them was an unexplored wilderness. An early fort near Niles, in addition to prairies of a few miles in extent, may have helped to decide settlers to locate there. At first then, the distribution of population was controlled by trade routes, and these outlets to market were simply natural waterways except the Erie Canal.

Michigan became a state in 1837 and her boundaries definitely fixed. By 1840 settlers had ceased to go North along Lake Huron, being little attracted by the marshes of the Thumb, but instead had spread west mostly south of a line connecting Port Huron and Grand Rapids. A

new means for the transit of goods, the railroad, now began to help affect the distribution of population so that inland regions, if they possessed good soil, could support a dense farming, population, as a route to market had now been provided. Three railroads were projected, to be built by the state. Since water routes to market had hitherto been so important in the settlement of Michigan, it is natural that no north and south lines were planned, but only east and west ones to connect quickly with lake ports. These roads were to connect Detroit and St. Joseph, Monroe and New Buffalo, and Port Huron and Grand Haven.

The next ten years were simply a story of steadily increasing population supported by agriculture in the southern four tiers of counties, and a comparatively slight migration to the North.

Up to 1850, soil suitable for farming and routes to market had chiefly controlled the distribution of Michigan's population. After 1860, however, the distribution of population became more irregular through the influence of certain other geographical factors, the results of which were not very evident until then. Among these are (1) Effect of topography and climate; (2) Effect of natural resources, i. e. soil; forests and ores; (3) Relationship to other geographical provinces.

We know that the great ice sheets by the formation of the Great Lakes have profoundly influenced the climate of much of Michigan, and, besides serving as a source for fish, have caused by their use as trade routes the building and the growth of many cities, as Port Huron and Detroit. The ice sheet also was the cause for the state's swamps, hills, lakes, water power, and sandy plains, while, indirectly, it determined the location of clay, marl and peat deposits. The ice sheet also was very important in determining the character of the forest growth and the areas for farming, lumbering, shipping and manufacturing.

The "Soo" canal was finished in 1855, and we naturally And by 1860 a considerable population in the Houghton and Marquette mining districts. The settled districts of the southern peninsula spread north, most rapidly along the shores of Lake Michigan and Saginaw Bay. (certain sandy areas, for example, in western Allegan and Van Buren Counties were thinly settled. The same was true of the extensive marshes in the Thumb. Of late years, the favorable climate along Lake Michigan permits of fruit raising on these sandy areas, while the Thumb marshes after draining prove very valuable for the growing of sugar beets. Other swamps in Kalamazoo, Van Buren, Ottawa, and Kent counties are now equally valuable for the raising of celery and peppermint, and permit Michigan to lead in their production. The draining of marsh land, however, was not undertaken until after the supply of other lands more easily prepared for cultivation was exhausted, so that, up to about 1890, the population of such areas was rather small.

By 1870, the lumber industry, which before 1860 was of no importance, had grown until Michigan was the chief lumbering state, a preeminence which it maintained well into the nineties. In general, the best white pine was north of Saginaw and Grand Rapids and south of Alpena, therefore, in addition to these cities, others in between, as Muskegon and Manistee, sprang up directly dependent on the lumber industry. The population of these lumber sawing districts was chiefly located about the mouths of rivers, affording means of running the logs to the mills and a harbor for shipping the product on the Great Lakes. The salt industry naturally went with the lumber sawing, as the waste from the latter could be used to evaporate the salt. In the mean time, other cities were growing up in the state, there being in 1870 no less than fifteen in the southern peninsula compared with five in 1860.

Railroad building in Michigan is easy, there being no very great differences in topography. We find by 1880 many roads completed and others under construction. These opened up the interior regions by furnishing routes to market. This is especially noticeable along the line of the Grand Rapids and Indiana Railway, where the population along its line is very much denser than back from it. A study of a railroad map of Michigan shows that three out of the five northern built roads bend decidedly toward the west. This is true in the case of the Michigan Central, Detroit and Mackinaw, and Ann Arbor, but is not evident in the case of the Pere Marquette and the Grand Rapids and Indiana, as they were already built so close to the west coast that there was no room to bend. Is it not probable that the bending resulted from the presence of the rich iron and copper ore of the upper peninsula, and the desire to exploit it?

At this time, only Oscoda and Montmorency Counties in the southern peninsula were unpopulated. In the northern peninsula, the discovery of new iron ore districts along the Wisconsin border encouraged the settlement of these regions. Ten years later in 1890, the whole of the southern and most of the northern peninsulas were settled, and the number of cities had greatly increased, there now being eight in the northern and twenty-three in the southern peninsula. Two of these cities, Big Rapids and Au Sable-Oscoda (classing the last two towns as one), which were founded on the lumber industry, are no longer cities of over 5000 population since, after the exhaustion of the lumber, other industries did not follow.

Since 1880, especially throughout the southern four tiers of counties, there has been a steady decrease in the rural population, accompanied by an increase in the urban population. The first regions to suffer were the hilly districts, for example in Barry Co. The lake region of northwest Washtenaw and the adjoining part of Livingston county have also never supported a dense population for the same reason.

The chief changes by 1900 were an increase in the rural population of the northern part of the state, and a corresponding decrease in the southern, while all over the state the urban population steadily increased. This increase of the urban at the expense of the rural

population continued, there being in 1910 forty-nine cities of over 5000 in Michigan compared to forty-one in 1900. These new cities, moreover, except two in the upper peninsula mining districts, were all located where the rural population was on the decrease. Where railroad communication is poor, and the soil mediocre, as in Oscoda, Crawford, and Lake counties, the population is likewise small. The largest cities, in general, during the last ten years became much larger, but many of the smaller ones, as Port Huron, Manistee, Coldwater, and Ishpeming, lost in population.

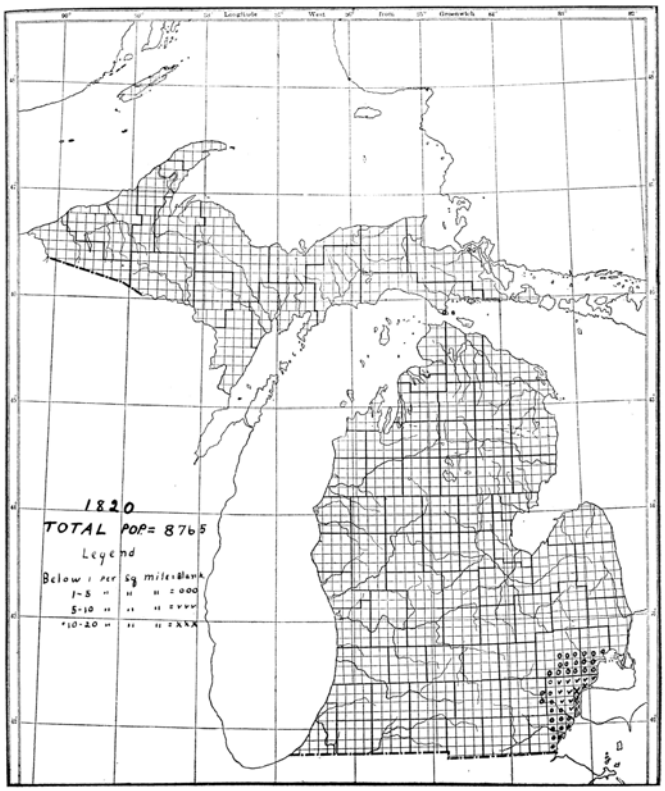
Forty counties in Michigan have lost in rural population. (See Map 45 K) Allegan county, if the manufacturing villages of Otsego and Allegan, which increased 2500 in the last ten years, are excluded, would also be included in this list, as the true rural districts lost considerably in population. Including Allegan county just one-half of the eighty-two counties in the state have decreased in rural population, this decrease dating in the case of the southern four tiers of counties usually as far back as 1880. Should the urban population, include all people living in towns of over 2500, as the census often does, this loss is still more evident, and would include Alger, Allegan, Charlevoix, Houghton. Isabella, and Presque Isle counties in addition, or all but thirty-seven counties in the state. Ottawa and Wayne counties only would show a gain. It will be noted that many of these counties show a considerable increase in total population, and the urban must be subtracted before it becomes evident that the rural districts have lost.

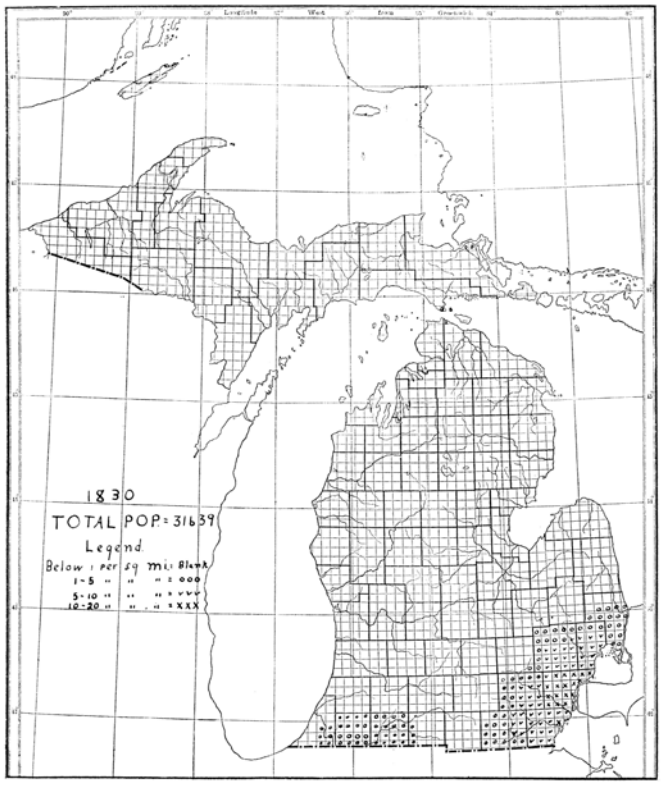
The cause of this loss in population of the rural districts, besides the younger generation leaving for the cities, seems to be due to a decrease in the average size and number of farmer families. In several counties, the number of acres per farm is increasing, while the number of farmers is decreasing. Since the rural population of Michigan increased only 2½% in the last ten years, while the urban increase was vastly greater, I think it highly probable that by 1920 Michigan's rural population will show a decrease as Ohio and Indiana already have done. The northern peninsula and the Northeastern part of the southern, and immediately around Detroit and Grand Rapids will probably show an increase; the rest, with some possible exceptions on the lake shore, will probably lose. The largest cities will grow still larger, but several of the smaller ones will undoubtedly lose in population.

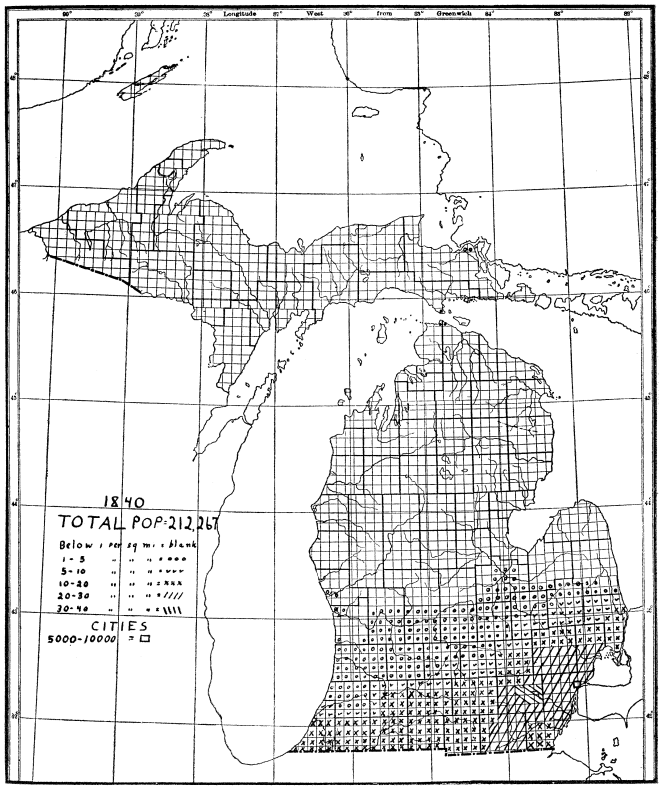
I find then that the distribution of Michigan's population has been determined by routes to market, climate, topography, and relationship to other neighboring provinces. Cities have been built and their location determined by the presence of natural resources, as Marquette and Saginaw; aided by water power, as at Grand Rapids. Others like Escanaba and the "Soo" are places of transfer of goods from rail to water routes. The size of Detroit is due to its location on great trade routes, especially by water, while Jackson and the other inland cities depend entirely on railroads for their location as manufacturing centers.

While the cities have increased greatly in population, many of the rural districts have decreased. The decrease has progressed from the oldest settled regions to the newer, and it appears probable for some years at least that this readjustment of population will continue.

Ann Arbor, Mich.



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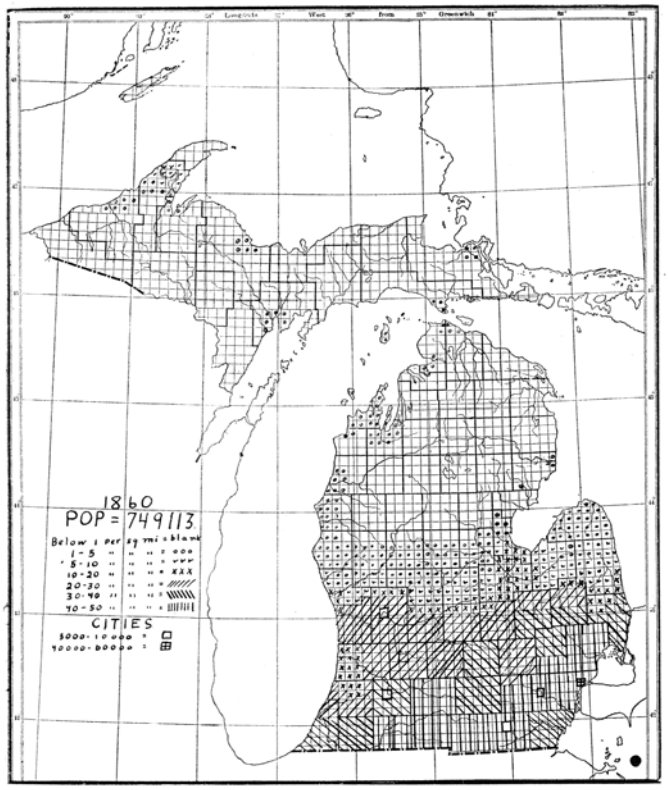
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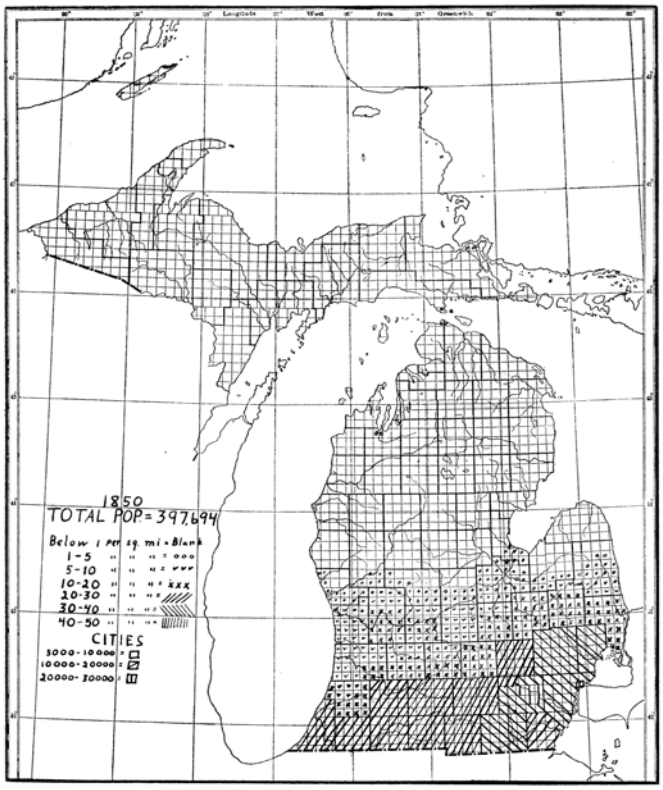
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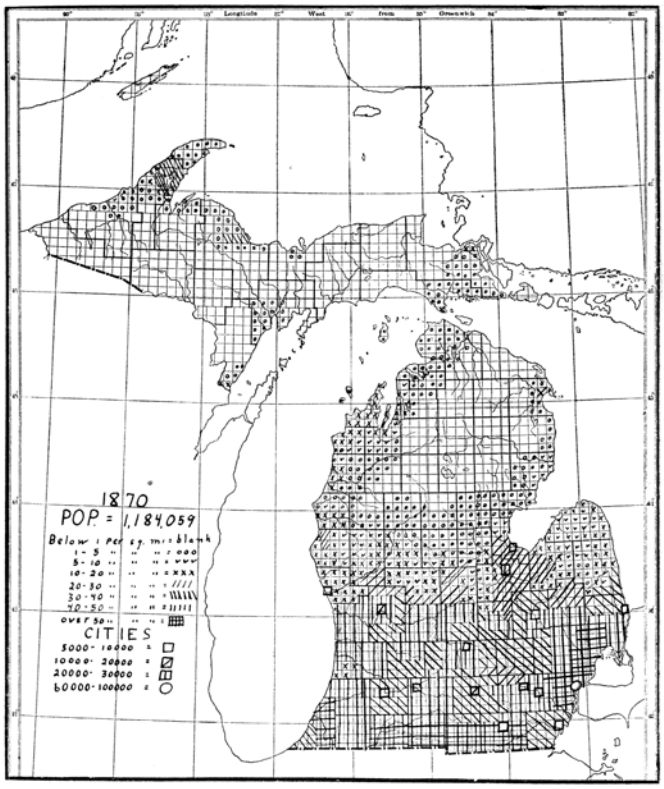
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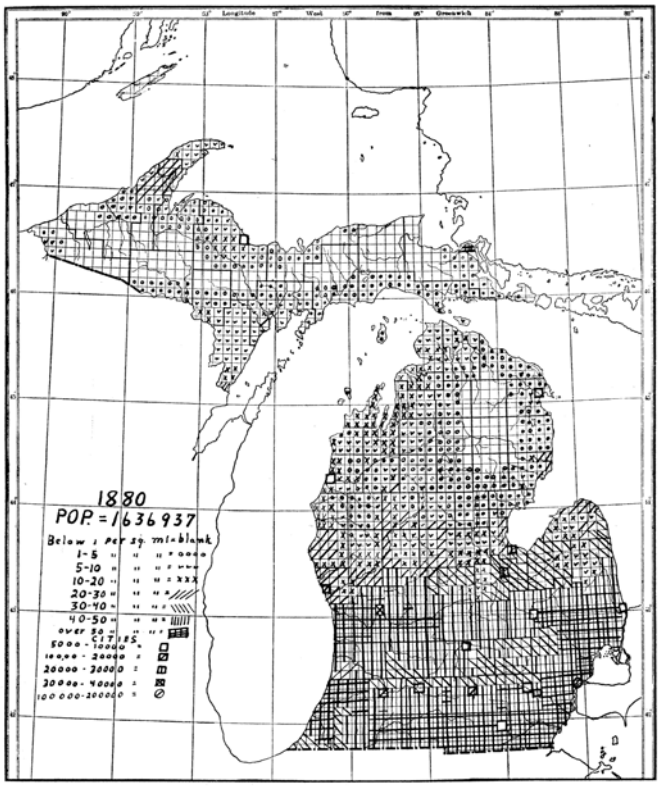
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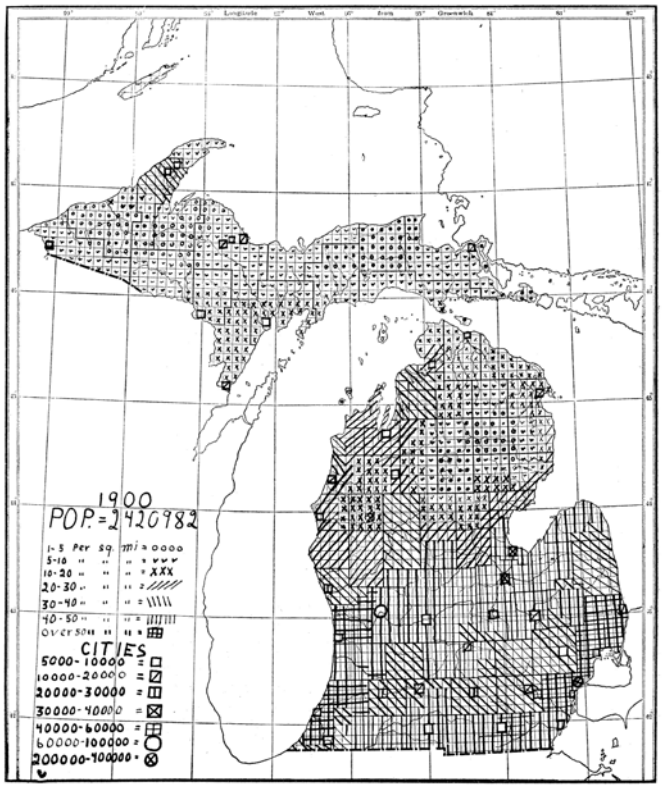
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CITIES
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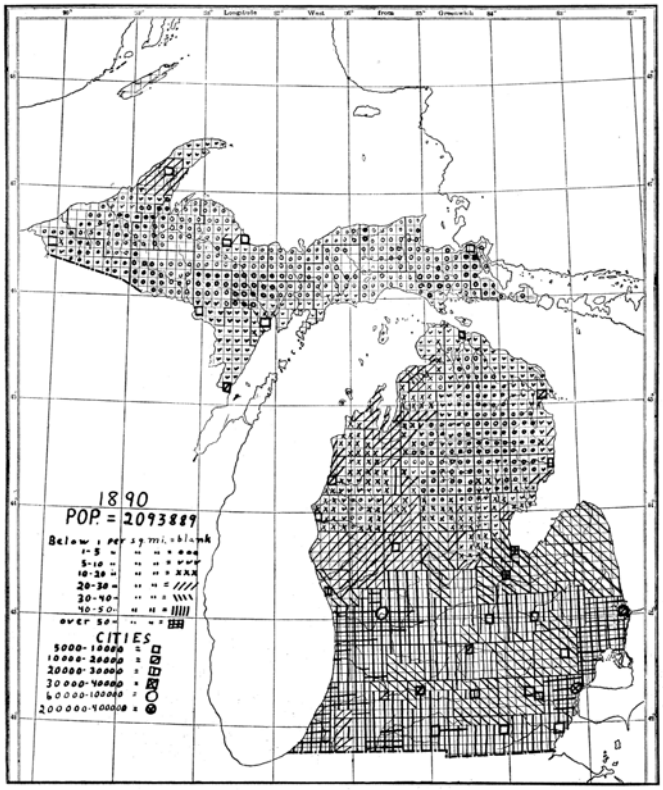
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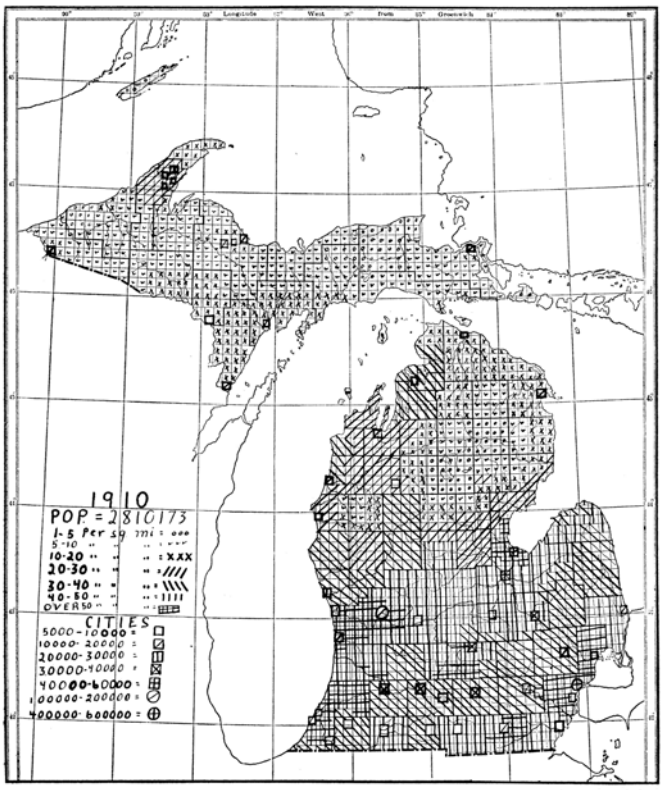
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GOLD DEPOSITS OF PORCUPINE DISTRICT, ONTARIO.

BY REGINALD E. HORE, MICHIGAN COLLEGE OF MINES.

In the fall of 1909, important discoveries of gold were made in the Porcupine district, Ontario, about 100 miles northwest of Cobalt and 360 miles north of Toronto. Development work has proven some of the deposits to be large and of very profitable grade. Two mines, Dome and Hollinger are now producing on a large scale and several are contributing smaller amounts. About $2,000,000 was produced in 1912 and a much larger output is expected for 1913.

The ore is native gold in pyritic quartz. The quarts occurs[1]: (1) as single fissure fillings or veins; (2) a series of fissure fillings running nearly parallel—vein system; (3) quartz-ferrodolomite lodes in which quartz veins penetrate bands of ferrodolomite or iron-calcium-magnesium carbonate; (4) quartz masses of irregular form, chimneys, kidneys, etc., and some more or less parallel, thick, lenticular masses.

The quartz is partly coarse grained, but much of it is very fine grained and has evidently been crushed. Granulation is indicated by the appearance of thin sections and strain phenomena are common. Some of the quartz shows a ribboned structure with banding in direction of the strike of the veins.

The gold is mostly in fine particles, but much also is in coarse grains and in the ore is frequently visible to the naked eye. Most of the visible gold is in parts of the quartz near the wall rock or masses of enclosed rock, and assays indicate a similar distribution for the invisible gold. Pyrite is almost always present in both quartz and wall rock and is more abundant in the latter. The gold is intimately associated with the pyrite and sometimes intergrown with it. Much of the visible gold near pyrite, however, is not actually in contact with it. There is some calcite in the ore, but it is not present in large quantity. Tourmaline, generally in aggregates of small crystals, is often found in the quartz, Scheelite has been found in a few veins.

[1]Descriptions of four typical deposits were given in Canadian Mining Journal, Nov. 1, 1910, pp. 649-656.

MICROSCOPIC CHARACTERISTICS OF THE GOLD ORE.

The examination of numerous thin sections shows that there are certain differences in the ore from the several deposits, but these seem to be of a minor nature and the following description of gold-quartz from the Dome Mine may be taken as fairly typical of the Porcupine Mines.

The quartz is not uniform in grain and one may distinguish readily between the part that is coarse and the part that is fine. The coarser grains are commonly 0.5mm. to 1.0 mm. in diameter, while the finer are about 0.05mm. The coarse has numerous small cavities partially filled with liquid inclusions, and shows marked strain shadows; the fine has fewer inclusions and strain effects are not so marked. Fine grained quartz forms streaks running through the coarse grained. In some cases, two coarse grains are separated by a row of fine grains, which have apparently been derived from the former by crushing. In one specimen an area, 1.0 mm. in diameter, of fine grained quartz encloses an isolated coarse grain 0.2x0.4 mm. in diameter. The finely crystalline has apparently been formed largely by granulation of the larger grains. The small particles are firmly cemented together and there was evidently some solution and recrystallization though the cement is not distinguishable under the microscope.

In the fine grained portions there is a notable absence of large fluid inclusions and evidently some such fluid was able to move among the fine quartz particles. The presence of fluid inclusions in the quartz indicates that it was not when solidifying. The granulation of the quartz indicates that it was comparatively cold when crushed.

MODES OF OCCURRENCE OF GOLD IN DOME MINE QUARTZ.

1. Gold completely enclosed in one grain of coarsely crystalline quartz, e.g. One grain of quartz 0.5x0.8 mm. in the plane of the section completely encloses three isolated grains of gold. The small size of these gold grains makes it appear unlikely that they were not completely enveloped in the quartz, though there is a possibility that they were not. Another quartz grain 1.0 mm. in diameter enclosed three ragged grains of gold about 0.04 mm. in diameter and several gold particles 0.01 mm. or less in diameter.

2. Gold in spaces between grains of coarsely crystalline quartz, e.g. One U shaped area of gold 2 mm. long and 0.02 to 0.06 mm. wide forms a ragged band between coarse quartz grains. It forms a border for two-thirds the periphery of one quartz grain—hence the shape. In several sections, there is gold showing similar relation to coarse quartz grains.

3. Gold in crystals and grains of pyrite, e.g. One area of pyrite 0.5 mm.x1.0 mm. encloses several irregular patches of gold, most of which are less than 0.1 mm. in diameter. Four of these gold grains are completely within the pyrite, while a much greater number are partially enclosed by the pyrite and partially by quartz. A second and rectangular area of pyrite 0.5 mm.x0.1 mm. has along its middle portion five areas of gold. The string of gold particles continues from either end of the pyrite into clear quartz. Another specimen shows an area of gold 0.6x0.1 mm. which is four-fifths enclosed by pyrite, while the end projects into colorless minerals. The part of the gold not enclosed by pyrite is on one side in contact with calcite and on the other with a grain of quartz.

4. Gold grains in calcite, completely or partially enclosed. A twinned individual of calcite 1 mm.x0.5 mm. is enclosed chiefly by fine grained quartz, and one end is in contact with an area of pyrite 1 mm. in diameter. Around the edge of the calcite and in immediate contact with it, are nine distinct particles of gold. Within the calcite and arranged in a string roughly following a cleavage direction, are six grains of gold 0.02 to 0.03 mm. in diameter.

A second specimen shows an area of calcite 0.1 mm. x 1.0 mm., enclosing a number of small gold grains. This calcite is partially enclosed in fine quartz, but it also fills a fracture in one large quartz grain. This same specimen shows gold in quartz with no calcite in contact.

Another specimen shows a grain of calcite 0.5 mm. in diameter, which wholly or partially enclosed twenty ragged grains of gold. The gold is irregularly scattered through the calcite, but it is mostly at the edges. The calcite is surrounded by fine quartz 0.05 mm. in diameter.

5. Gold among grains of finely crystalline quartz, e.g. One very irregular area of gold, 1 mm. long and varying in width from 0.02 to 0.1 mm. is almost completely enclosed by fine grained quartz, the particles of which average 0.03 mm. in diameter. That part of the gold, not enclosed by quartz, is in contact with calcite. It is noteworthy that most sections, showing gold, show also fine (probably granulated) quartz and small amounts of calcite.

CHARACTERISTICS OF THE WALL ROCKS.

The gold quartz occurs in rocks of the Keewatin and Huronian series. The Keewatin is composed largely of igneous with some sedimentary rocks. The Huronian is largely sedimentary. Most of the deposits are in altered quartz porphyry, others in more basic rocks—porphyrites, basalts, etc.—conveniently called greenstones. Some are in conglomerate and greywacke-slate.

Mr. A. G. Burrows, who has mapped the area for the Ontario Bureau of Mines, gives the following succession of formations for the district:

"Pleistocene.-Post Glacial-stratified clay, sand and peat. Glacial—boulder clay.
Pre-Cambrian.—Later Intrusives—quartz-diabase, olivine, diabase, etc.
Igneous contact:
Cobalt Series—conglomerate.
Unconformity:
Temiskaming Series—conglomerate, greywacke, quartzite, slate or delicately banded greywacke.
Unconformity:
Laurentian—A complex of granites older than the Cobalt series. It intrudes the Keewatin, but its relationship to the Temiskaming is not definitely known; it may be in part older and in part younger than the Temiskaming series.
Igneous contact:
Keewatin—The series consists chiefly of basic to acid volcanics, much decomposed, and generally schistose; amygdaloid basalts, serpentine, diabase, quartz or feldspar porphyry, felsite, iron formation, rusty weathering carbonates, and other rocks have been recognized."

The Cobalt and Temiskaming Series referred to by Mr. Burrows are divisions of the Huronian corresponding to those made by Dr. W. G. Miller at Cobalt and by Mr. Robert Harvie[2] in the area east of Lake Temiskaming. Mr. Harvie calls the lower sediments "the Fabre series."

The gold is found in quartz occurring in the several types of rock comprising the Keewatin and Temiskaming series, but it is noteworthy that the wall rocks of all the ore bodies, while probably originally quite different, have remarkably similar composition. This similarity is due to alteration, by which the minerals have been replaced by sericite, carbonates, quartz and chlorite. The light colored wall rocks[3] are largely made up of the first three minerals. In the darker ones, there is much chlorite. Pyrite occurs abundantly and is commonly well crystalized. The wall rocks are commonly not highly auriferous except where penetrated by quartz stringers. Where thus silicified, the rocks sometimes contain payable quantities of gold.

[2]Geology of a portion of Fabre Township, Quebec Mines Branch, 1911.

[3]Microscopic description of several of the rocks is given by C. W. Knight and A. G. Burrows in the Bureau of Mines report, 1911. Mr. John Stansfield described rocks and ores from Vipond Mine in Canadian Mining Journal, Feb. 15, 1911. The wall rocks of Dome, Hollinger and Rea mines were described by the writer in Trans Cen. Min. Inst., 1911, pp. 173-178.

ORIGIN OF THE DEPOSITS.

We have in the Porcupine district pyritic gold quartz; deposits enclosed in rocks characterized by an abundance of ferrodolomite, sericite and pyrite. The nature of the ore and the wall rocks suggest that the gold was introduced into the fissures along with the chief constituents of the minerals mentioned. The solution,* which contained the gold, probably contained also in some form, iron, sulphur, silica, potassium, and carbon dioxide. From the solution, practically all the potassium and carbon dioxide escaped into the wall rocks and aided in the formation of sericite and ferrodolomite. Part of the iron and sulphur also escaped into the wall rocks and there formed pyrite crystals and contributed iron to the formation of ferrodolomite. Part of the iron and sulphur and nearly all of the gold and silica was deposited in the fissures themselves. It appears that the walls were more readily penetrated by some constituents than by the others,[4] and in this way much, of the CO_2, S, and Fe escaped. In proportion as these constituents escaped, the solubility of the gold in the remaining solution would be decreased and the deposition therefore aided by removal of solvent as well as by lowering of temperature. The pyrite, first formed in the veins, was comparatively poorly crystallized and was probably formed quickly. The pyrite in the wall rocks and some pyrite in the veins, that is probably of secondary origin, is in well formed crystals and evidently formed slower, or at least, under some more favorable conditions than did the original auriferous pyrite of the quartz veins. The gold and pyrite were not evenly distributed originally. Evidently in the first crystallization, they tended to segregate here and there, and the especially favorable place for deposition was near the walls or around masses of enclosed rock.

After the filling of the fissures with quartz, gold and pyrite, the veins were shattered and the quartz granules strained or crushed. In the crushed zones, a secondary set of minerals including sericite, chlorite, calcite, ferrodolomite and pyrite and some gold were deposited. These probably originated in the vein and wall rocks. The gold thus formed is in coarse grains which probably grew by slow accretion of small particles by a process continued over a long period. It is probable that this coarse gold grew at the expense of the fine gold contained in the quartz in its immediate neighborhood, thus leaving much very low grade quartz in the vicinity of the spectacular specimens. The coarse gold, to which a secondary origin is here attributed, while showy, is usually quite subordinate in amount to the fine gold, much of which may well be still in the form in which it was first deposited with the pyrite and quartz. Some fine gold, however, is probably secondary, and there are cases in which the amount of secondary gold is greater than the amount of primary.

There is nothing to indicate that the character of the deposits has been changed to any considerable degree since the glaciers cleaned away the surface rocks, and there is therefore no reason for believing that the ore will show any appreciable dependence on the present surface. The secondary changes which have taken place are not surface alterations.

*For discussion of transport of gold in solutions containing such constituents see Macaren Gold, pp. 105-107.

[4]Cf. Lindgren, Characteristic features of California gold quartz veins. Bull. Geol. Soc. Am., 1895.

RIPPLE MARKED HURONIAN QUARTZITE AT NIPISSING MINE, COBALT, ONTARIO.

(With six plates).

BY REGINALD E. HORE, MICHIGAN COLLEGE OF MINES.

At the Nipissing Mine, an unusual method of surface prospecting has presented remarkably well exposed areas of Huronian and Keewatin rocks. In examining these areas recently, the writer found the well developed ripple marks shown in the accompanying photographs. Being very well preserved, the marks are of interest in themselves. As they occur in a series of rocks supposed to be largely of glacial origin, they have added interest.

Of the Huronian rocks at Cobalt the most characteristic type is a coarse conglomerate, sometimes called the Cobalt conglomerate. The unusual characters of this rock were pointed out by Dr. A. P. Coleman, who showed[1] that the material was probably of glacial origin. In a subsequent paper in the Journal[2] the present writer gave additional information in support of Dr. Coleman's view. It was stated that, with the conglomerate, there is well stratified material, and to this was ascribed a glacio-fluvial origin. At the time of writing that paper, the writer had not seen any ripple marks; but the evidence of deposition of some of the material by water was regarded as quite conclusive. More recently ripple marks have been found in the rocks broken in mining the silver ore; but until the hydraulic work was done at the Nipissing, it was not possible to present photographs showing the marks on rock in place. In prospecting the surface at the Nipissing Mine, a powerful stream of water, 4800 gallons per minute from a 3½ inch nozzle, is directed against the glacial debris and the rocks washed bare. The photographs show Huronian rocks thus exposed. Fig. No. 1 shows an end view of the quartzite bed which has the rippled upper surface. Fig. No. 2 shows a thin layer of shaly mud rock lying on the ripple-marked quartzite. Fig. No. 3 shows a large boulder in the fine mud rock lying on the quartzite. Evidently, there were remarkable changes in local conditions, for these large boulders encased in fine mud rock, or shaly greywacke, lie immediately on top of the bed of uniformly grained quartzite. Fig. No. 4 shows, at the left, blocks of the mud-rock which overlies the quartzite. Figs. No. 5 and No. 6 show closer views of the quartzite.

[1]A. P. Coleman. Lower Huronian Ice Age. Jour. Geol. Vol. XVI, No. 2, pp. 149-158. 1908.

[2]Glacial Origin of Huronian Rocks of Nipissing. Jour. Geol. Vol. XVIII No. 5, pp. 459-467. 1910.

THE ENVIRONMENT OF SOIL BACTERIA.

BY DR. F. H. HESSELINK VAN SUCHTELEN.

Ill physiology we recognize the influence of environment on the single cell. Not only if we take the high water content of the living substance into consideration, but also if we consider metabolism as the phenomena of life, the importance of water in the life process is made clear. Without water there is no life. By adding to or diminishing the water of the living substance within certain limits, we increase, diminish, and limit the intensity of life processes. The environment of bacteria is water, and soil bacteria form no exception. There, where there is little or no water at their disposal, the metabolic processes are reduced to a minimum. Spores, cysts, and other defensive organs are the results of the dryness of the medium. In a former publication,* the author has tried to make a numerical comparison between the water content of the soil and the activity of the soil bacteria. As an indicator the carbon dioxide production in soils was chosen, a metabolic product that is formed in nearly all life processes in comparatively large quantities, and in easily detected form. Without going into detail with these experiments, I will say that if the soil contained only 4.4% of water, the soil bacteria would be unable to attack the easily broken down dextrose which was added to the soil. I take this as an example illustrating the overwhelming importance of the water content for the biochemical action in soils.

From the foregoing it is clear that water is the medium of soil bacteria. Although one cannot make in proxi a sharp distinction between the quantity and the nature of the water, I should like to cite the experiments of Beyerinck† as evidence of the influence of the nature of the water, on the microorganisms. His experiments deal chiefly with unicellular organisms, and let us say here that the Oecological Method proved to be of especial value, in the case of the lower organisms, because they are unicellular and expose in comparison with their content such an enormous surface, on which the medium can act.

In his classical investigations Beyerinck showed the dominating influence of the nature of the water environment on the behavior of the microorganisms. How uniform was the material with which he started must be noted, for it was in many cases the mud of the canal in Delft. By influencing intelligently the life conditions of bacteria, such as oxygen and food supply, temperature and many other factors, he was able to predict and to obtain with mathematical surety the predominating flora.

In such experiments as this on the nature of the water, Soil Bacteriological Science finds its greatest promise.

If we ask ourselves, "what is the ultimate aim of the applied science of Soil Bacteriology," the answer must be: The aim of Soil Bacteriology (aside from the purely scientific interest) is to put the action of the soil bacteria in the service of Agriculture, to suppress the detrimental species and their action, and to encourage the beneficial bacteria like those which accumulate nitrogen, and such as make available those compounds of the soil, which are in a state not available for plant nourishment.

*F. Hesselink van Suchtelen: Centr. Bl. f. Bakl. II Abt. Bd. 28 S. 45.

†F. Stockhausen: Oekologie Auhaüfungen nach Beyerinck.

To accomplish this, or, in other words, to influence the microorganisms in the soils intelligently, presupposes the necessary knowledge of the environment of the bacteria, of those factors which are at our command, that can be varied as we desire, such as oxygen supply, water, reaction, etc.

Let us now consider the soil and let us treat it from the point of view of a medium for the micro-flora. Soil is composed of three states of material, solid material, water, and air; and these three states have a marked influence on each other. The soil bacteria are living in the soil water; but this soil water is influenced very markedly by the solid material and by the air. It is this that makes the soil a difficult medium to investigate. I might say here that I know of no medium that is so variable and complex as soil. If we consider milk in this respect, the air, and the solid substances, play only a very small role. It can also be said that the milk of different cows does not differ materially as a medium for the bacteria. On the other hand, we know how large are the differences in soils which must necessarily influence their micro-flora.

We encounter still another difficulty if we remember the fact that our medium (the soil) is very difficult to sterilize. Only by the action of powerful agents are we able to sterilize the soil, in fact, the changes, which are necessarily brought about by this sterilization process, are so marked that we doubt even if we may call this sterilized medium, soil.

This means that in the case of soils we are practically deprived of the opportunity of recording the action of single species of microorganisms. Further soil is especially characterized by enormous surfaces. To give an idea how great the soil surface is, I should like to cite the work of Alfred Mitscherlich* who came to the conclusion that the outer surface of one gram of quartz sand was 1.38 square meters and that of one gram of clay was 966.7 square meters. These enormous surfaces give us an idea of how closely the soil water can be in contact with the solid soil substance.

In regard to the permeability, I regret to say that we have but very little trustworthy data. The reason for this is evident, namely, that the many values obtained with air dried soil do not permit any conclusions for field conditions. We may say, however, that the permeability of the different soils is extremely variable.

From the foregoing, it is clear that, even in fine tertiary quartz sand, which has so great a surface, there is great possibility for action between the soil water and the absorbed substances on the soil grains.

On the question of, "What is the nature of these reactions between the soil water and the soil particles," the answer cannot be very satisfactory. Permit me, however, to draw your attention to some experiments which give us the right to suppose that these reactions are different from the reactions that occur in a beaker and test tube.

*A. Mitscherlich: Bodenkunde für Land und Forstwirte, 1905 p. 49-73.

Aside from the phenomenon of selective absorption which we know takes place in soils, we have at our command a number of experiments performed by the most distinguished chemists showing that the amount and kind of surface possesses marked influence on the reactions. I cite here the work of van't Hoff who concludes that both the nature and the amount of surface exposed have an influence. The inversion of sugar is affected by the nature of the walls of the containing vessel, and its reduction by Fehling's solution is effected by the walls and the amount of cuprous oxide formed in the reaction. In the case of soils where we have so large a surface and such thin films, absorption, surface tension, and other not-well-defined molecular forces may and will play their role.*

It follows then that the addition of an excessive amount of water to soils (drainage) changes the conditions, i. e. salts that were not in solution in the soil solution will be found to be dissolved in the drainage water, and we have therefore, the right to suppose that the drainage water is different, in a qualitative and quantitive respect, from the film water which surrounds the soil particles. It is, therefore impossible to make any conclusion, from the analysis of drainage water on the soil solution as it exists in the soil, because the dissolving process is probably not proportional to the amount of water added.

On account of the importance of the environment of the soil bacteria, a knowledge of the solution as it exists in the soil becomes most urgent. And here we may add that this subject does not only concern the lower forms of life, but in the case of higher plants also, the study of the soil solution promises fruitful results.

So I have directed my study towards this theme and have been seeking a method which would furnish me some soil solution. Here, again, we meet with some difficulties which I should like to mention briefly.

It is absolutely impossible to obtain a comparison between the soil solution obtained and the total soil solution, because every method for securing the soil solution can give only a percentage of the total solution, as the last traces of soil water are held back tenaciously by great forces.

The method finally adopted consists of the displacement of the soil solution by means of paraffine oil. There is something depressing in the impossibility of being able to verify our obtained results with the reality.

With the kind assistance of Mr. Itano some experiments have been made. Sulphuric acid of known strength was added to carefully washed, dry quartz sand. After this paraffine oil was poured on the sand and by means of a suction pump the acid was regained. The titration showed that the so-obtained acid did not differ from the acid which was used in the experiment. I am perfectly aware of the fact that this experiment has practically little bearing on soil conditions. The fact, however, that our regained solution had the same composition as the original employed solution does not mean that our method is not permissible.

There must be considered, then, the nature of the medium with which we displace the soil solution. We may congratulate ourselves on the choice of paraffine oil as a medium. With the most refined instruments that were at our service, we were unable to detect any change in the solution when it was brought into intimate contact with the paraffine oil. We found then, that the inactive paraffine oil did not change the electrical conductivity of the soil solution, while the chemical analysis also showed that there was no change brought about by the action of the paraffine.

*F. K. Cameron, The Soil Solution.

The third method which we employed was the measuring of the surface tension. We might expect that when only slight traces of the paraffine oil were dissolved in the soil solution, this would have its marked effect on the surface tension of this liquid. However, we were unable to detect any change in the surface tension of the liquid after it had stood for a long time covered with the paraffine oil. So far, the results obtained have demonstrated the permissibility of the use of the method employed. In regard to the amount of the soil solution that can be extracted by the application of our method, I must say in advance that even slight modifications even of the apparent details of our process caused large variations in the amount of water obtained.

If we record only the values obtained, by the use of those conditions which we knew to be most satisfactory, then we must record the amount of solution obtained as a percentage of the total water capacity.

But, at present, there exists in a few fields of soil physics such conflicting interpretations of the meaning of the term "water capacity." In the different text and laboratory books, we find the most diverse definitions and the most conflicting methods for the determination of this total water capacity. Because we suspected that this value would vary quite markedly with the application of the differently devised methods, we undertook some experiments which proved that our supposition was correct. The total water capacities of the same soil as determined by the different methods varied over thirty per cent. From the soils containing the maximum water capacity we were able to extract over seventy per cent of the total water. As an example, I will cite in this connection the data of an average extraction.

From eight kilograms of soil (clay) which contained 14.3% water (figured on the basis of dry soil) was obtaned 330 cc. of soil solution. It is evident that such results can not be obtained by the use of a simple

suction pump where the maximum difference of pressure is necessarily less than one atmosphere.

However, we have secured larger differences in pressure by using several hundred pounds of pressure by means of a hydraulic press.

We now have the soil solution and will analyze it. There are two ways in which we may investigate such a solution which require a short explanation.

I. The chemical analysis.

II. The physiological analysis.

A chemical analysis seeks through its results a determination of soil fertility. However one can not claim that this method has been successful. The only thing which we can say with surety about its results is that if a certain nutritive element is found to be not present in the soil, then it is lacking for the nutrition of the plant. The difficult problem between the relation of chemical analysis and availability still awaits solution.

The physiological analysis draws its conclusions from the vegetation itself. In other words, it is an attempt to put direct observation in the place of theoretical deduction. Since no definite results from the analysis of the soil solution have been so far obtained, and since one roust recognize that the latter has no scientific value as a determination of soil fertility, the author has applied not only chemical analysis to the solution but in connection with this also a physico-chemical analysis.

We may suppose from analogy that the physico-chemical analysis of such a liquid may be of exceptional value. However, I must emphasize that in spite of all the various determinations I do not feel myself called upon to draw any definite conclusions from these analyses with reference to the exceedingly complex question of soil fertility. So far as chemical analysis is concerned we must keep before ourselves the all important fact, "corpora non agunt nisi soluta," in other words that only which is present in the soil solution can be taken up as a nutritive substance, but not every thing present need be taken up.

There still remain a few things which I should like to say. I will state the facts that were revealed by the application of our method. Complete results will appear in a publication of the near future.

This is not the place to discuss the details of the different analyses.

In many cases there was found in the soil solution a slime. This must be regarded as the first experimental proof of the presence of this substance in soil, and it is not impossible that much of the irregular behavior of the life in soil could be explained to some extent with a knowledge of this slime. If I may be permitted, I should like to call your attention to the possible effects of this substance on dessication, diffusion, and other processes.

(2) The specific gravity of the soil solution which influences the movement of the soil water was found to be higher than that of water.

(3) As to the viscosity of the soil solution which governs to a certain extent the rate of adjustment of soil water in the soil, we can say that it is relatively high.

(4) The surface tension, a property of liquids which is associated with adsorption and has an influence on the degree of capillarity, was found to be low in the case of the soil solution.

(5) In reference to the osmotic pressure of the soil solution, which on one hand is the indicator of the state of solubility, and has a bearing on the adjustment of the water in the soil, and on the other hand markedly influences the life in the soil, we can say that this pressure is low, a result which was to be expected from the comparatively high resistance of the liquid.

(6) Another thing noticed is the acid and basic binding capacity. This was found by the electrotitrimetric method. In general we may say that the neutrality was obtained by adding very small quantities of a normal 1000° alkali or acid.

(7) In regard to chemical analysis you will not be surprised to hear that all nutritive substances could be found in our soil solution to a certain degree. An astonishing fact, however, is the relatively large quantity of nitrites in some samples. With reference to the value of the chemical analysis of the soil solution, I refer to that which I have already said.

Our work can by no means be looked upon as complete, but I dare say that the results are promising, and that I feel happy to be able to present to the reader the preliminary results which have been obtained by the application of the methods of Mr. Itano and myself.

ORIGIN OF CONTINENTAL FORMS III.

BY HOWARD B. BAKER.

1. THE THEORY.

In previous papers I have outlined the theory that the present continents are the fragmentary remains of the crust of the Mesozoic earth. I have modified and developed the theory of the loss of earth mass first put forward by the Reverend Osmund Fisher and later by Professor Pickering, and have recorded certain conclusions prominent among which are the following:

1. The present geographical plan is the result, primarily, of the separation of mass from the earth.

2. This separation marked the close of the Mesozoic division of geological time as determined by the sedimentary series of southern France.

3. The separation was caused by extraterrestrial gravitation.

4. It was brief.

The theory as thus built up rests upon a large mass of geological data. General physical considerations, the evidence in favor of the former existence of extensive lands now lost, the fractured margins on the Atlantic ocean and the fact that opposite fractures may be fitted one to another upon the globe, these and many other lines of evidence go together to support the conclusions arrived at. At the same time the theory has biological aspects which are scarcely second in importance. It receives much support from both botany and zoology, and in return it offers to supply a new and a comparatively simple basis for pretertiary zoögeography.

2. SURVIVAL OF LIFE.

One of the numerous objections which the theory has to meet is the biological one that in a world-catastrophy of the nature claimed so much heat must needs be liberated that no life could survive.

In reply to this we may say that the geological evidence is so strongly to the effect that the continental sheets separated at the time mentioned and we are so sure that life did survive from the Mesozoic into the Tertiary that a purely a priori objection has less force than it would otherwise have. Moreover, we know that at the end of the Cretaceous period there was great and widespread extinction of species of both animals and plants.

To account for the survival of life we may observe that precipitation, altitude and atmospheric circulation would all combine to prevent unduly high temperatures over the lands of the earth. The smaller the land area the greater would we expect to be the destruction of life, and there is considerable reason for believing that such was the case.

Upon the first rifting in the old crust, under the tidal distortion of the planet, water would pour down upon the hot material below, being vaporized at once. This vaporization must have continued throughout the action and condensation of the vapor in the upper atmosphere would produce rain in abundance and this rain must fall upon the crustal plateaus.

The fragments of crust, standing two and a half or three miles high above the level of the hot denuded surfaces of the globe which are now the ocean bottoms would experience barometric changes which are difficult to predicate, but it seems clear that such altitude must have operated to lower the temperature very much as we observe it to do today.

With the denuded areas hot and upward currents being engendered in the atmosphere the continental plateaus being meanwhile high and cool, it follows that the return currents would descend upon the plateaus bringing them rain and cold from the upper atmosphere. It is even supposable that snow might fall on peaks in the interior of a continental plateau notwithstanding the enormous amount of heat actually liberated from the denuded earth. So we see that there are three good reasons why life might survive. We are quite sure that it did survive and the objection would thus seem to be not a fatal one against the theory.

3. LAND CONNECTIONS.

Land connections where none now exist have long been postulated to account for numerous and curious relationships between widely separated groups of animals and plants. As long ago as 1853 Dr. Hooker insisted upon connections of Australia and New Zealand with Africa and South America to explain the distribution of southern plants and from that time on such connections have been largely introduced in all parts of the world. Heretofore it has been necessary to postulate numerous and extensive vertical movements of the earth's crust often without any real geological basis and even in spite of geological opposition, and we must concede that such land connections as may have linked western Australia to South Africa, or South America to tropical Africa, or Spain to America, that these and similar examples are distinctly opposed to what is known of isostatic geology. So it becomes of interest to inquire what effect this theory of the loss of earth mass may have on the conception of lost land connections the world over.

Since in time the event is sharply localized to the end of the Cretaceous and the beginning of the Tertiary periods, and since it resulted in substantially the present arrangement of the continents, clearly its principal effect is upon pretertiary geography. It is capable of explaining many of the distributional peculiarities of Tertiary and later time, but surely prior to that it involves a radical change in the globe upon which we may attempt to depict the relations of land and sea.

So numerous and so difficult properly to weigh are individual instances of floral and faunal relationship that it seems best for me not to attempt to present and discuss any catalogue of such examples, striving rather to deal in summaries by comparing current opinions with the new basis afforded by the theory to learn to what extent they may modify each other.

The mechanical difficulties to be overcome have been found to be troublesome and the scheme herewith presented is put forward with some misgivings on account of its manifest crudity, but it serves to bring-out important points and is therefore in use pending the devising of a better method or a better carrying out of this one.

Briefly it is this: Mercator charts being found to be unsatisfactory on account of the distortion, and spherical projections being almost impossible to draw, the continents have been moved about upon a globe and in the rearrangement photographed. From the photograph the continental outlines have been traced so that we secure a result somewhat similar to a spherical projection. Upon this tracing as a basis the paleogeographic ideas of any author may be set forth.

Herewith are presented adaptations on this basis of the views of von Ihering and Ortmann, and, as well as I have been able to interpret, of Scharff. These maps are submitted without argument, being intended to introduce the method and to gain some idea of the general relations of land and sea without too great reliance on detail either of time or of conformation. The land connections are variously correlated by different authors. Ortmann would retain some form of connection between Europe, Greenland and America throughout Tertiary time. He would have South America united to Antarctica in the lower Tertiary, von Ihering would connect Brazil and Africa in the Eocene. Matthew terminates all trans-Atlantic connections before the Tertiary and cuts off South America from Antarctica by the middle of the Eocene.

Hardly a feature of zoogeography or of geology rests upon a less satisfactory basis than intercontinental correlation and until the element of time can be more positively determined this must remain one of the difficulties of the theory, but it is precisely in this matter of correlation that the theory offers a new basis and a new hope for the future.

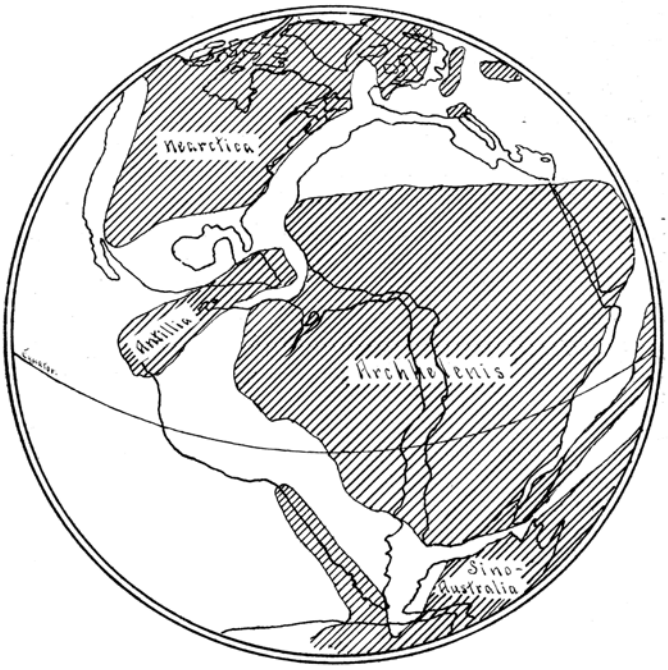

Fig. 1.—Lower Cretaceous. After Ortmann 1902.

Fig. 2.—Upper Cretaceous. After Ortmann 1902.

Fig. 3.—Land and Sea, “Eocene” time. After von Ihering 1907.

Fig. 4.—Adapting the ideas of Scharff, 1912, on the relationships of American life.

THE FACTORS THAT DETERMINE THE DISTRIBUTION OF BOLEOSOMA NIGEUM IN DOUGLAS LAKE, CHEBOYGAN COUNTY, MICHIGAN.

BY H. V. HEIMBURGER.

This paper is a brief account of a piece of work done during July and August, 1912, at the University of Michigan Biological Station.

By means of a careful survey of the lake, it was found that Boleosoma nigrum was confined to water less than thirty inches in depth. Thirteen localities were found, in eight of which both young and adult of the species were aboundant. In five habitats only immature specimens were found.

It is known that the breeding habits and the food habits of animals play an important part in the determination of the local habitat. The breeding habits of Boleosoma nigrum have been described by Hankinson and by Forbes. These authors state that the eggs of Boleosoma are attached to stones, mussel shells and sticks which lie loosely on the bottom. Spawning occurs in April and May.

I have found by examination of the contents of twenty-one stomachs that the food of adult Boleosoma consists, as was found by Hankinson at Walnut Lake, almost wholly of the larvae of Chironomidae; Chironomus prevailing. The young Boleosoma subsist very largely upon Entomostraca, but eat correspondingly larger proportions of Chironomus as they themselves grow larger.

The larvae of Chironomus and other midges occur plentifully wherever there is an abundance of decayed vegetable matter on the bottom. The eggs of midges are deposited on the floating leaves of such plants as Potomageton, Nymphaea, etc., whence the larvae find their way to the muck of the bottom.

An analysis of the habitats in Douglas Lake shows them to have the following features in common:

(1) Mussel shells, small stones and sticks on the bottom.

(2) Quiet water, protected from violent wave action, allowing a thin deposit of muck to accumulate in patches over the sand.

(3) Absence of thick muck deposit in the shallow water. There are always patches of sand, stones, etc., not covered by the muck. A deep deposit of muck is usually found in the deeper water near by.

(4) Masses of Potomageton or other aquatic plants with floating leaves, are found in the immediate vicinity.

These characters of the habitat are shown to be directly connected with the food habits or the spawning habits of Boleosoma and may be regarded as physical features that determine the distribution of the species.