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HOW MANY ARCHÆAN ROCK-GROUPS HAVE WE IN GREAT BRITAIN?

BY CH. CALLAWAY, D.SC , M A., F.G.S., WELLINGTON, SHROPSHIRE, ENGLAND.

RECENT geological research amongst the pre-Cambrian rocks of North America, while it has settled some points, has unsettled others. A generation ago the terms "Laurentian" and "Huronian" were thought to have a clear and definite application. At that time, we in Great Britain knew of only one Archæan group, called Hebridean or Lewisian, and supposed to be the equivalent in time of the Laurentian. Later on, British geologists discovered a second pre-Cambrian formation, the "Pebidian" of Dr. Hicks, or "Uriconian" of the writer. This great volcanic system bore many resemblances to the published descriptions of the Huronian, and it was referred with more or less hesitation to that group. Meanwhile, Dr. Sterry Hunt was creating more systems in America. We heard of his "Norian," "Moutalbian," "Taconian," and "Keweenian," and every year we looked for new worlds from his prolific brain. Unfortunately, subsequent research in the United States and Canada has but very partially confirmed Dr. Hunt's results, and even our faith in "Laurentian" and "Huronian" has been somewhat confused. "Huronian" appears to be several things, and "Laurentian" in some localities is said to be an intrusive granite. Nevertheless, it appears to be generally admitted that in North America there are gneisses and granites which are older than any other rock-masses, and that in the same region there are volcanic formations which are younger than these crystallines, and more ancient than the Cambrian; so that the old notions on "Laurentian" and "Huronian" remain true in a general way. It would also seem that North America contains sedimentary rocks which are newer than the Huronian, and are yet pre-Cambrian. Thus it would hardly be rash to conclude that, on the western side of the Atlantic, there exist at least three Archæan roch-groups, a gneissic, a volcanic, and a sedimentary, and that they succeed each other in the order here given. Now it is interesting to remark that this description agrees with the latest results of research in Great Britain. We have first of all the gneisses and schists, which in Scotland are called "Hebridean," and "Malvernian" in England. We cannot say that these formations are the exact equivalents of each other, and it would certainly be rash to assert that they. or either of them, can be correlated with any rock-masses the other side of the Atlantic. Nevertheless, they are admitted to be the oldest rocks in Britain, and, in the opinion of the writer, they are separated by a considerable interval from the formation which comes next. This great volcanic system holds the place originally assigned to it in the Archæan series by Dr. Hicks and the writer. Its pre-Cambrian age has been admitted by Sir A. Geikie, director-general of the Geological Survey of Great Britain and Ireland, so far as the Uriconian rocks of Shropshire are concerned; but he assigns the Pebidian of St. Davids to the base of the Cambrian. In the opinion of the writer, the volcanic rocks of St. David's are truly pre-Cambrian; so that the name "Pebidian," originally given to them by Dr. Hicks, has priority over the more modern term "Uriconian." These rocks are of wide distribution, being found in North and South Wales, at Charnwood, near Leicester, in many parts of Shropshire, in the Malvern Hills, and probably at Howth, near Dublin. Evidence has recently been collected of a third pre-Cambrian system. Near Church Stretton, in Shropshire, is a chain of hills, forming Longmynd, built up of conglomerates, sandstones, and slates. Murchison called these sediments "Bottom Rocks," and he referred them to the Lower Cambrian. This view has been adopted by the English Geological Survey, and generally accepted. Recently, however, evidence has been collected which makes it almost certain that this formation is of pre-Cambrian age, and the present writer has given it the name "Longmyndian." The true basal Cambrian, a band of quartzite, occurs in close proximity to the Longmynd rocks, though not in absolute contact; and it is incredible that the Longmyndian, which is some miles in vertical thickness, should be a mere subdivision of the Cambrian, which is found in three of its four members within a few miles to the east. It would seem, then, that on both sides of the Atlantic, the Archæan (or pre-Cambrian) series consists of (at least) three members, gneissic, volcanic, and sedimentary, which follow each other in the same order, suggesting a similarity of conditions in both areas in the successive epochs of Archæan time.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

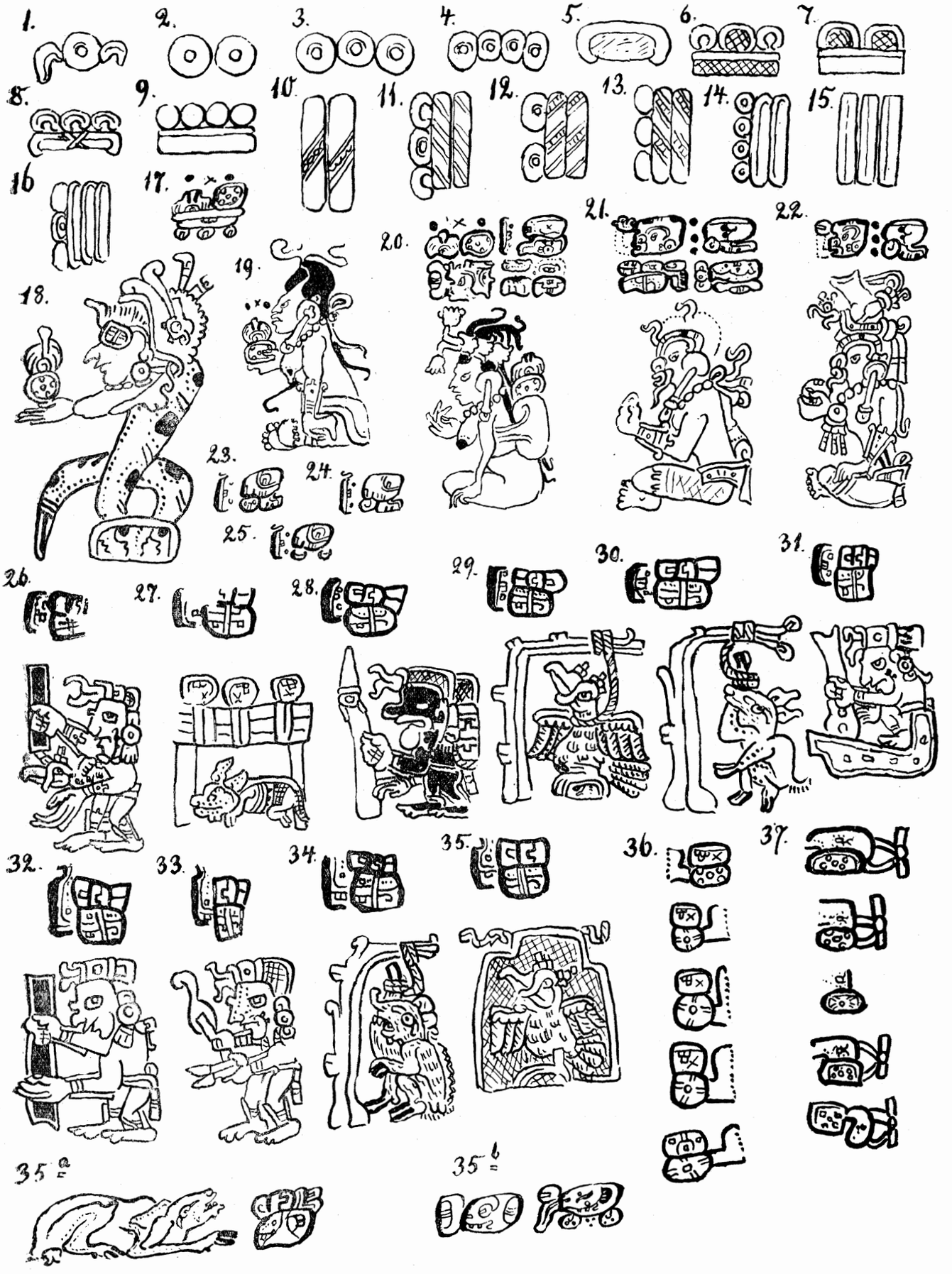
The editor will be glad to publish any queries consonant with the character of the journal.

Is the Maya Hieroglyphic Writing Phonetic?

In No. 505 of the *Science*, Professor Cyrus Thomas devotes a few more pages to the problem of the Maya hieroglyphic writing. "These," he says, "may perhaps be profitable to the subject, if confined to an earnest endeavor to arrive at the truth." The "additional evidence," introduced in this manner by Professor Cyrus Thomas, he has seen fit to precede by some remarks intended to invalidate the criticism I offered in this paper some months ago (*Science*, Aug. 26). My answer to these remarks is presented in the following lines, which, I trust, will also be profitable to the subject, although I do not claim to be the only scientific man that "earnestly endeavors to arrive at the truth."

Professor Thomas is correct in stating that "a dot and two crosses with a month-symbol form a date in the bottom line of Plate 49, Dresden Codex." Nevertheless, I firmly believe I can maintain that "there does not exist a numeral designation with crosses *between* the dots." I have never seen it in the Codices. On the other hand, I found, for instance, on the sides of the Stela J of Copan (Maadsley, "Biologia Centrali Americani," Pl. 69–70) that the *one* dot of the numerals 1, 6, 11, and 16 always is framed by two ornamental signs, but there is never an ornamental sign between the *two* dots of the numbers 2, 7, and 12. Compare the Figs. 1–16 of the adjoined table. Moreover, I think, the analogy between the two hieroglyphs, Figs 29 and 30 (of my former paper), is obvious. Since in the one case the two dots and the cross are a part of the hieroglyph and not a numeral, I hope, it will not be a fault of veracity to believe the same in the other.

Professor Thomas says I am not correct in stating that Fig. 30 (of my former paper) is the glyph he interpreted "moisture." "True, the parts are similar," he says, "but the details and surroundings are different." In the adjoined table I reproduce the Fig. 30 of my former paper by Fig. 17, and Professor Thomas's moisture symbol by the Fig. 18. Certainly, the surroundings are different. In Fig. 17 the hieroglyph is placed on a dish, in Fig. 18 on the hand. And there are wanting in Fig. 18 the two dots and the cross that are seen in Fig. 17. But the parts are not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"similar,' but essentially the same. And that the whole hieroglyph is really the same, is proved by comparing Figs. 19 and 20 of the adjoined table, taken from the Dresden Codex, 18^a and 19^c. In Fig. 20 the hieroglyph of Fig. 17 is the first hieroglyph of the text. Its representative is shown in the hieroglyph carried on the back of the woman figured below. This representative of the text-hieroglyph exhibits the same elements in the same order as Professor Thomas's moisture-symbol held on the hand of Fig. 18.

Professor Thomas asserts that my statement that the first glyph shown in his Fig. 2, p. 46 (*Science*, July 22), is the same as that in certain groups mentioned by me, and Figs. 31–33 (of my former paper) are incorrect, as I had failed to include the prefix. The character of my first figure, he says, is the same, but the characters of my two other figures are different and give a different word. The first character Professor Thomas had interpreted *u-zabal*, "set the snare." Respecting the latter, he says, it is possible that the signification is suggested by *haɔab*, "a sword, weapon to wound with, a whip." This agrees, Professor Thomas asserts, "very well with what we see in the hands of the figures below, and also with the general tenor of the series." True, instead of naming one character and one series, I ought to have spoken of two allied characters and two allied series. But my objections to Professor Thomas's interpretation were chiefly based on the fact that each one of the two hieroglyphs is the leading character in a *series* of representations, embracing different actions, and not only the "setting of the snare." The first character is the leading hieroglyph in the series Figs. 26–31 of the adjoined table; the second one in the series Figs 32–35. It is obvious that — although there are represented different persons and animals — the general tenor of the two series is essentially the same. Both, undoubtedly, refer to capturing animals, showing the deity armed for hunting and different captured animals. Now, it can be proved that the leading character of the hieroglyphic groups of a series suggests the action in which the persons figured below are represented (compare, for instance, Codex Dresden 4^c and 7^c and the two leading hieroglyphs in Codex Dresden 12^c, Codex Troano 19^c, etc.). As, in our case, the general tenor of the two series is the same, the first of our characters (Figs. 26–31) will be intended to indicate the same action as the second one (Figs. 32–35). We must conclude, therefore, that the second part, which is common to the two hieroglyphs, is the essential one; and that the other, the so-called "prefix," is subordinate, referring to circumstances of minor importance, perhaps interchangeable. This conclusion will be proved once more by the fact that the second part occurs alone, and apparently with the same general signification (see Fig. 35^a, taken from Dresden Codex 60^a).

As to Professor Thomas's interpretation, the name *haɔab* he gives does not agree with his own alphabet. For the element in question, the knot or loop, seen on the top of the second part of the hieroglyph, according to Professor Thomas's alphabet, does not express the sound of the "letra herida" ɔ, that is to say, *ts'*, but that of *z*, or *s*. The word itself is not *ha-ɔab*, as Professor Thomas reads, but *haɔ-ab*, an instrumental noun derived from the verb *haɔ*, "to whip, to wound." Finally, it is obvious that the rendering, "sword, a weapon to wound with, a whip," does not more agree "with what we see in the hands of the figures below, and also with the general tenor" of the second series (Figs. 32–35), as it would agree with that of the first one (Figs. 26–31). I may safely abandon to the reader's judgment to decide whose interpretation in this case is the more based on "mere assumptions," Professor Thomas's or mine, and who has more earnestly endeavored to arrive at the truth.

Professor Thomas acknowledges the correctness of my statement that the sign of aspiration found in Brasseur's "Landa" is not in the original text. "Nevertheless," he says "we have to thank the Abbé for a happy suggestion. . . . I may add that Dr. Seler has gone farther than Brasseur, as he has given us in his 17^a a character which appears to be new, — at any rate, I have been unable by a careful search to find it in any of the codices." I refer Professor Thomas to the Figs. 23–25 of the adjoined table. These, and some other variants, act as leading hieroglyphs in a series of twenty-nine hieroglyphic groups, accompanying as many figures of the rain-god. My Fig. 23 contains the element in question, with exactly the same characters as I rendered them in Fig. 17^a of my former paper. This Fig. 23 occurs three times in the series, in Dresden Codex 30^c, 31^c, and 39^c. Professor Thomas, therefore, has not carefully searched. To call a notorious falsification "a happy suggestion," and to stigmatize a correct statement as a conscious falsification (I say it with due regard to courtesy), we are not wont to consider as an earnest attempt to arrive at the truth.

Professor Thomas argues that I had criticised his article without having thoroughly read it, because, in the fourth character of his Fig. 4, I overlooked, he says, the little item on the front of the face. Had I but looked to his Fig. 3, I would not have fallen into the error of considering the two as the same. I regret to say that the writer of the Dresden Codex has fallen into the same error, since he mentions the deity, seen in the Figs. 21, 22, of the adjoined table, in Dresden Codex 5^a by the first hieroglyph, Fig. 21, in Dresden Codex 13^b by the first hieroglyph, Fig. 22, both differing from another in "the little item on the front of the face," nearly in the same way as the characters of Professor Thomas's Figs. 3 and 4 (*Science*, p. 45) differ from another.

Professor Thomas himself, in most cases, has overlooked the notorious existence of variants of writing and the replacement of one element by another. He says, "To assume that the Fig. 29 (of my former paper) is a variant of Fig. 30, is certainly straining a point to the utmost tension." I could show to my opponent more curious variants. As to the mutual replacement of the element *Kin* and Professor Thomas's "letter-glyph" *b* — that, in my view, renders the sound *Kan* "yellow" — I refer him to Figs. 36, 37, of the adjoined table, the first showing the leading hieroglyphs of Cort. 21, Tro. 35^d, the latter those of Codex Tro. 24* 23^{*a}.

Professor Thomas concludes his objections against my criticism with the following phrase: "I must confess that his (Dr. Seler's) eyes are sharper than mine, if he can find any figures in either of the Codices representing a god or any one else beating a drum. This, like other of his assertions in regard to the significance of other figures, appears to be 'merely hypothetical.'" My reply to this apostrophe is the Fig. 40, taken from Dresden Codex 34^a, which, for the benefit of the reader, I have contrasted with two Mexican paintings, Figs. 38 and 39, taken from Codex Borgia 55, and Codex Land. 39. In the two Mexican paintings, a goddess is seen and a god, the latter beating a drum, in Fig. 39, curiously held between the legs. No scholar versed in Mexican prolographic style, will deny that the instrument seen in those paintings is really the drum, the *tlalpan-neuetl*, made of wood and covered with a tiger-skin. Compare Fig. 42^a, the well-known musician of the Mendoza Codex. Now the god of Fig. 39 has his exact counterpart in one of the persons of Fig 40. Here, in the very middle of the scenery, we have the head of the sacrificed (or the dead deity) exposed on the top of the altar-pyramid. On the left side a fire is burning, and below it an offering of maize is placed on a dish. To the right hand other offerings are seen, consisting of a meal of maize and turkey, and of a meal of maize and certain other game. Four persons sit around, playing different instruments. On the upper part of the left side, a black-colored person holds the *chicauaztli*, the well-known rattling staff of the Mexican paintings (see "Compte Rende, VII. Sess. Congr. International Américanistes," Berlin, 1888, p. 661–664, and "Veröffentlichungen aus dem Königlichen Museum für Völkerkunde," I., p. 147, 152). Below him a woman beats a drum of curious form. The music is seen rising from the end of the instrument. To the right hand of the altar, in the lower part, a man is playing a flute. Here, also, the music is seen rising from the lower end of the flute. The upper figure, on the right side, with one hand shakes the rattle and with the other beats the drum, held between the legs exactly in the same manner as with the god of the Codex Land. (Fig. 39). Another series of musicians occurs in Codex Tro. 24* 23^{*d}. Here a person, exhibiting a black-colored skin, like that of Fig. 40, is seen with the *Chicauaztli* in the one hand, and a rattling-ring (?) in the other (Fig. 41), while another deity (Fig. 42) is beating a drum. On

the top of the figures I reproduce the leading hieroglyphic that accompanies the figures and undoubtedly refers to the general tenor of the series. The curious form of the instrument of Dresden Codex (Fig. 40) occurs also on Plate 24 of the Codex Tro., together with another more regular form (see Figs. 43 and 44 of the adjoined table). And considering the former (Fig. 43) and the other figures of the instrument represented above, I think, nobody will doubt that also in the figures of Codex Cortes 21a and Codex Tro. 35b (Fig. 46 of the adjoined table) the writer intended to represent a drum. We shall the less doubt of it, as this action here is accompanied by hieroglyphs (Fig. 45), the one of them exhibiting the same characteristics as those accompanying the musicians in Figs. 41 and 42. We have, thus, in the known Maya Codices at least five well characterized representations of persons or gods beating a drum. My mentioning, therefore, of a god beating a drum was not "merely hypothetical," not a "mere assumption," but based wholly on proofs.

I shall not go into further details; nor shall I attempt to criticise the "additional evidence" brought forward by Professor Thomas in his last article, or to discuss the probability of that

curious enumeration of historical facts occurring every 177 days, for the space is limited. Only, by the way, I note that Professor Thomas interprets phonetically *Xaman* as "north," the character that, in reality, designates *nohol* "south" (see the evidence adduced by me in "Zeitschrift für Ethnologie," XXIII., p. 104). His third sample of the use of his "letterglyph" *b* is one of those interesting hieroglyphs that change the so-called "prefix" according to the four cardinal points. Compare Figs. 47, 48 of the adjoined table, the former taken from Codex Dresden 29, 30c, the latter from Codex Tro. 31, 30d. These varying elements undoubtedly are indicating the names of colors, as each of the four cardinal points was distinguished by a special color. And the so-called letterglyph *b*, with all probability, has to be considered as expressing the element *Kan* "yellow" (see "Zeitscrift für Ethnologie," XXIII., p. 108, 109). The explanation Professor Thomas gives of the five dots, seen under certain hieroglyphs, as rendering the sound *ho* "five," will receive a curious illustration by the varied form these dots exhibit, for instance, in the Fig. 35b, taken from the Dresden Codex. It does not appear, with all, that the samples of interpretation presented by Professor Thomas in his last paper are more satisfactory than those of his former one. It will be seen, indeed, that there is no reliance in the simple fact that, applying a certain key, the parts give apparently appropriate results. In a similar way there could be proved and has been proved that the Mexican and Peruvian languages are derived from Sanscrit, and that the descendants of the lost tribes of Israel survive in the Southern Sea. The right, Professor Thomas claims, to apply such a key has to be proved in the first place. I am awaiting if, in the paper he is preparing for publication by the Bureau of Ethnology, he will be able to do so.

DR. SELER.

Steglitz, Germany, Dec 18.

Irrigation Surveys.

I HAVE just had the pleasure of perusing your issue of the 16th, with its review of Irrigation Work by the General Government. Allow me, in returning my thanks for the comprehensive references made, to make some brief corrections:—

In the first place, then, the expenditures of the Geological Survey as to "irrigation" work, have been that of two appropriations — in all $350,000. This is wholly outside of printing, which is paid for under other appropriations. The cost thereof will not be less than $15,000. Besides these two direct sums of $100,000 and $250,000, with the printing of Part II. in Annual Reports 10 and 11, the Survey for work in the Arid Region, topographic and hydrographic, has had two more annual appropriations of not less than $100,000 in all. The terms of the appropriations were designed unquestionably to continue indirectly irrigation work which Congress had declared should not be continued by the Geological Survey. Its irrigation work, then, has cost much nearer $500,000 than it has $235,000. Its results are two finely printed volumes — one of 123 pages and the other of 395. In the latter are 80 or 90 pages of matter previously printed — the larger part of it, indeed, having been twice printed by committees of the Senate and House. The reprint in Report Eleven is of Major Powell's testimony and argument before the House Select Committee on Irrigation, 51st Congress, which in substance and effect is the same that Director Powell made to the Senate Committee at the same session. So, in effect, it has cost nearly half a million dollars to publish 419 pages of "original" reports. There are no topographical maps of significance as yet issued.

Now, the Department of Agriculture, under its office of Artesian and Underflow Investigation, and of Irrigation Inquiry, received and expended between April 15, 1890, and May 1, 1892, just two years, the munificent sum of $70,000. During that time it made and has reported on two engineering, geological, and economic examinations of the Great Plains region, between 97° and 105° of longitude, and two reports besides on Irrigation proper. It prepared and issued six volumes in all, — a report on Artesian Wells, and the three parts you have noticed of the closing report on Artesian and Underflow Waters, also Progress Irrigation Report for 1891, and the volume referred to as "miscellaneous" by the review. As the work is in part only my own, though I edited all of it, I can justly challenge the value of it all in quality, as much as I may claim it exceeds the report in quantity, as compared with the Geological Survey. The three reports (six volumes or parts) embrace in all 1,694 pages, and some 58 valuable profiles, maps and geologic sections, besides more than 100 other special illustrations. The report (four parts) you reviewed has been printed to the number of but 1,733 copies for the use of Congress, and it has cost something less than $4,000. The other reports cost in all about $2,500 — a total estimate of $6,500. Since that publication, Congress has appropriated $6,000 more for Irrigation Inquiry. How much of this has been used I do not know; some of it I am aware has been wasted and I make the remark advisedly, as much as I regret to say anything except in approval of the Department of Agriculture.

The account stands then:—

A. Ten thousand copies (5,000 each volume under a general provision of law) of two reports, and some other reprinting by the U. S. Geological Survey, with a number of reservoir sites reserved on the public lands, most of which have been restored under later law by the Land Office to the Public Domain; the cost of all, at least, $465,000.

B. Eight reports in all by the Office of Irrigation Inquiry, Department of Agriculture, — three of the Engineers, three of the Geologists, and the same number of the Agent in charge (myself) — in all seven parts or volumes, containing the matter in brief, already stated, all this, too, in cost has been less than $80,000.

The Weather Service volume (chiefly Mr. Glassford's work) is above criticism and that of the U. S. Census Office in its "Irrigation Division" work is only an adjunct to the U. S. Geological Survey, unduly fostered by the Secretary of the Interior and the Superintendent of the Census to enable Director Powell to do that which the 51st Congress by withdrawal of a specific appropriation had forbidden him doing, viz., continue the work of irrigation survey and inquiry. The agent in charge was formerly an hydrographer in the Survey and was transferred to the Census. He has done better than it could have been anticipated he would from his first bulletins, but the work has cost far more than it is worth. That, too, from the value of the conditions and not the ability of the agent himself. Of course, it will be noticed most because it has the benefit of the expensive printing and publishing of the Census Office.

This whole irrigation inquiry has been characterized by a wasteful scramble to get in or on it. The State Department has published a volume thereon; the Treasury's Bureau of Statistics has dabbled therein in its volumes on "Internal Commerce"; the General Land Office has had its shy; the Weather Service is discussing "Earth Moisture," etc., and the Army Engineer Office got in a little one on Egypt. The Department of Agriculture only did what it was ordered and of late months not all of that.

RICHARD J. HINTON.
Member Am. So. of Irrigation Engrs.

Washington, Dec. 26.

Geographical Variation in Birds

In ornithology geography is the father of trinomial nomenclature. Climate is one great factor in variation, and topography has not a little to do with making the climate; but geography is unquestionably *the* cause of variable climate, else would the polar regions be tropical instead of frigid. Topography is at best local.

The variations of a species of birds, which make of it several sub-species, are due to its geographical distribution. These varying individuals do not take the name of "forms," as in entomology, but are set apart as true sub-species, each with a more or less well defined habitat of its own. But there is a serious difficulty in ascribing any sharp line of difference between the forms which intergrade on the outskirts of the geographical range, and a corresponding difficulty in ascribing any definite geographical limit. It is not seldom that individuals of one sub-species are found far within the range of another sub-species.