a series of independent figures, designed to show the degree of development attained by each type at any epoch relatively to other epochs.

These charts and diagrams were thoroughly discussed; and the lecture closed with a few remarks on the genealogy of plants, illustrated by an arborescent figure showing one of the possible ways in which the present forms of plant-life may have been derived.

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.*]

Intelligence of the crow.

JAPAN is the birds' paradise, as fire-arms cannot be carried except by special permit. Though their punishment of criminals is sometimes extremely cruel, to shoot birds for sport or for scientific purposes would never enter the heads of this kind-hearted people. I noticed, in many parts of the country, that the crow felt a sense of security, meeting man boldly, conscious that he is a benefactor — and acknowledged as such — by killing injurious grubs, even though he collect a few grains of corn in the operation. He scorns to fly at your approach, and fears not a stick pointed at him, which he never takes for a gun. He is as familiar in Japan as he is shy in America and Europe.

Another instance of this bird's intelligence came under my observation as I was walking among the crumbling arches of Caracalla's baths in Rome, in April, 1882. When near the walls, a stone nearly as large as my fist fell at my feet. Fearing a recurrence of what I supposed was an accident of perishing masonry, our party went farther toward the centre of the area. A second and a third fell near us; and, looking up, I saw some crows circling above our heads, one of which dropped a fourth from his claws. It seems that we had been strolling too near their nests in the walls; and they took this method to drive us away, — a very effectual one, as a stone of that size, falling from the height of sixty feet, was an exceedingly dangerous missile, and perhaps only prevented from being fatal by the failure of the bird to make allowance for the impetus given by its own motion. The aim was accurate, and the discharge right overhead; but, as both we and the bird were moving, it fortunately missed its mark.

SAMUEL KNEELAND.

Paleolithic man in Ohio.

In SCIENCE of April 13, p. 271, Professor Wright remarks that "no paleolithic implements have as yet been found [in Ohio], but they may be confidently looked for." It has seemed to me possible, from my own studies of the remains of paleolithic man in the valley of the Delaware River, that traces of his presence may only be found in those river-valleys which lead directly to the Atlantic coast, and that paleolithic man was essentially a coast-ranger, and not a dweller in the interior of the continent. If we associate these early people with the seal and walrus rather than with the reindeer, and consider them essentially hunters of these amphibious mammals rather than of the latter, it is not incredible. I submit that they did not wander so far inland as Ohio, nor even so far as the eastern slope of the Alleghanies; and we need not be surprised if paleolithic implements, concerning which there can be no doubt whatever, — for recent Indians made and used stone implements that are 'paleolithic' in character, — are not found in Ohio, or even in Pennsylvania west of the valley of the Susquehanna River.

Unquestionable evidences of paleolithic man in America have been found in the valleys of the Connecticut, Delaware, and Susquehanna Rivers, and probable traces of the same people in the valleys of the Hudson, Potomac, and James Rivers. This is an extensive range of territory, and one not too limited as the probable area occupied by a primitive people.

If we could accept without qualification the assertion occasionally made, that America's earliest race was pre-glacial, the difficulties that beset the study of paleolithic man would quickly vanish. I am disposed to believe it, upon theoretical grounds, but have met with no satisfactory demonstration that such was the case. In a recent lecture before the Franklin institute of Philadelphia, Prof. H. Carvill Lewis remarked, "That man existed before the glacial epoch has been inferred from certain facts, but not satisfactorily proven."

Accepting the above conclusion, and coupling it with the assertion made by both Professors Wright and Lewis, that the melting of the great continental glacier occurred so recently as ten thousand years ago, we are compelled to crowd several momentous facts in American archeology into a comparatively brief space of time; and it becomes more probable that the fabricators of the implements found in post-glacial gravels came from some transatlantic continental area, and had not wandered far inland when met by southern tribes, who drove them northward, exterminated or absorbed them.

On the other hand, if the relationship of paleolithic man and the Eskimo is not problematical, and the latter is of American origin, then I submit that man was pre-glacial in America, was driven southward by the extension of the ice-sheet, and probably voluntarily retreated with it to more northern regions; and, if so, then in Ohio true paleolithic implements will surely be found, and evidences of man's pre-glacial age will ultimately be found in the once-glaciated areas of our continent.

CHAS. C. ABBOTT.

The copper-bearing rocks of Lake Superior.

Mr. Selwyn's courteous reply in SCIENCE, No. 8, to my letter in No. 5, calls for only a few remarks from me.

In his admission that I am right in asserting the existence of a great unconformity in the St. Croix region, between the basal sandstones of the Mississippi valley and the copper-bearing rocks, he yields the principal point for which I contend. It seems very unreasonable to me to extend the term 'Cambrian' over this unconformity; but, in the absence of any fossil evidence, I am relatively indifferent on this point. I only insist on the complete distinctness of the copper-bearing strata from the lowest sandstone of the Mississippi valley, and from the horizontal sandstone of the eastern end of the south shore of Lake Superior. Mr. Selwyn evidently does not appreciate that the St. Croix valley unconformity is not merely 'locally very great.' Our conclusions as to this unconformity are not based on any one local unconformable contact, but upon the fact, that, for a distance of over fifty miles in a north-westerly to south-easterly direction, the basal sandstone of the Mississippi valley lies horizontally athwart the courses of the tilted Keweenawan beds, overlying and burying the western termination of these beds, which are here disposed in synclinal form. Nor is the St. Croix Falls locality, described in the third volume of the Geology of Wisconsin, the only place in the St. Croix valley where the unconformity may be actually seen. Besides other places, it may be finely seen on Snake

and Kettle rivers, in Minnesota, where the Keweenawan beds are identical in all respects, even to the occurrence of interbedded porphyry-conglomerates and cupriferous amygdaloids, with those of Keweenaw Point.

As to the Animikie group, I have only to say, that I have not asserted its identity with the so-called Huronian rocks on the east shore of Lake Superior, spoken of by Mr. Selwyn, but merely its *probable* identity with the original Huronian of the north shore of Lake Huron, which neither I nor Mr. Selwyn have seen, and its certain identity with the iron-bearing schists of the south shore of Lake Superior. The term 'Huronian' has been so differently used by different members of the Canadian geological corps since the first establishment of the system, that much doubt must still remain as to whether there are two sets of schistose rocks north of Lake Superior, or not. This much, however, I regard as certain; viz., that the flat-lying Animikie rocks of Thunder Bay and northern Minnesota were once continuous with *some* of the folded schists lying north of them in northern Minnesota and Canada, — the Vermilion Lake iron-bearing schists, for instance, — although now separated from them by belts of gneiss and granite. The lithological differences between the Animikie rocks and the folded schists are often more apparent than real; while, in many respects, there is a very close lithological likeness. However, I do not expect, and indeed have no right to expect, acquiescence in my novel position as to the Animikie rocks until the evidence I have collected has been published. I am confident, that, with the evidence that I now have, in his hands, Mr. Selwyn would at least think the matter worth looking into.

With regard to the occurrence of volcanic ash in the Keweenaw series, I must acknowledge at once, that, so far as field-experience goes, Mr. Selwyn is far better equipped than I to judge of such materials, and that, not having seen Michipicoton Island, I am bound to accept his statement. I understood his first letter to indicate the occurrence of such ash in places which I had myself seen. Nevertheless, I bear in mind that a considerable school of English geologists has been long in the habit of calling almost any detrital rocks, not distinctly quartzose and associated with eruptive rocks, *volcanic ash*, when very often, at least, they might be simply derived by water-action from these rocks. Possibly there is some misunderstanding in our use of the term. Most of the detrital rocks of the Keweenaw series are volcanic detrital matter, in that they have been derived by water-action from the eruptive, massive rocks of the same series; but I used the term as applied to fragmental material produced by the volcanic action itself. I do not know of any *proof* of such an origin in stratified material, other than the vesicular character, and perhaps constant angularity, of the particles, which proof I have failed to find.

The discussion of such a question as the present one evidently cannot, however, be carried on satisfactorily in the pages of a journal; and I must ask my scientific *confrères* to defer their judgment until my publications on this subject, now in type, are issued. R. D. IRVING.

University of Wisconsin,
April 12, 1883.

Pairing of the first-born.

As regards the pairing of the first-born, my calculation of which called forth Mr. Hendricks's criticism, permit me to call attention to the following letter from Mr. Edmands, which I hope will set the matter straight. I applied to Mr. Edmands, because mathematics is not my *fach;* and I now have the pleasure of thanking him for the very kind attention he has given this matter. CHARLES SEDGWICK MINOT.

Boston, April 24, 1883.

As J. E. Hendricks remarks in SCIENCE of April 13, p. 278, "the chance that the first-born male will pair with the first-born female is as one to ten;" but Dr. Minot's argument in SCIENCE of March 16, p. 165, depends upon "the probability of both parents" being first-born, as stated at the beginning of the last paragraph on p. 165. If we first restrict the case to the offspring of first-born males, the chance that both parents will be first-born is evidently one in ten. But in the remaining ninety per cent of the race there would be no case of both parents being first-born. Taking the race as a whole, out of one hundred pairs, one pair would be both first-born, nine would have the male only first-born, nine the female only, and eighty-one (9×9) neither male nor female first-born. This does not touch the question whether Dr. Minot is justified in giving no weight to the eighteen cases in a hundred, where only one individual of the pair is first-born. J. RAYNER EDMANDS.

Cambridge, April 19, 1883.

Place the ten females in a row, and the ten males opposite them, with the 'first-born' opposite each other. The ten males are susceptible of $1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9 \times 10$ permutations, each of which furnishes a distinct system of pairing. Of these, $1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9$ are possible without disturbing the juxtaposition of the first-born. The chance of their pairing will therefore be,

$$\frac{1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9}{1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9 \times 10} = \frac{1}{10},$$

as stated by Dr. Hendricks in SCIENCE, April 13, p. 278. Mr. Minot's solution is correct only upon the supposition that *one pair, and no more*, will be formed. T. C. M.

JAMES CLERK MAXWELL.

The life of James Clerk Maxwell; with a selection from his correspondence and occasional writings, and a sketch of his contributions to science. By LEWIS CAMPBELL and WILLIAM GARNETT. London, *Macmillan & Co.*, 1882. 16+662 p., 3 portr., 4 pl., facsim., etc. 8°.

JAMES CLERK MAXWELL was born in Edinburgh on the 13th of June, 1831. He died Nov. 5, 1879.

The late Professor Benjamin Peirce once said in the hearing of the writer, that great geometricians did their best work before they had reached their fortieth year. This can hardly be said of the mathematical physicist; for the constant accumulation of new facts tends to make mature years the most fruitful in results to the student who still preserves his mental and physical activity. Commoner men doubtless, in time, make good the premature loss to the world of a genius. Those epochs, however, in a nation's history, in which men of