

I trust that the record which will be shown in the census report of cotton production, now in press, will form a convincing illustration of the legitimate uses of soil-analysis.

E. W. HILGARD.

University of California, Sept. 1, 1883.

#### Do humming-birds fly backwards?

The Duke of Argyll, in his *Reign of law* (p. 145), lays it down in italics, that '*no bird can ever fly backwards*.' He mentions the humming-bird as appearing to do so, but maintains, that, in reality, the bird falls, rather than flies, when, for instance, he comes out of a tubular flower. But this morning, while watching the motions of a humming-bird (*Trochilus colubris*), it occurred to me to test this *dictum* of the duke; and, unless my eyes were altogether at fault, the bird did actually fly backwards. He was probing one after another the blossoms of a *Petunia*-bed, and more than once, when the flower happened to be low down, he plainly rose, rather than fell, as he backed out of and away from it. I stood within a yard or two of him, and do not believe that I was deceived.

It may not be amiss to add that the Duke of Argyll's objections seem to be purely theoretical, since the '*Reign of law*' was published in 1866, and it was not till 1879 that the author came to America and saw his first living humming-bird.

BRADFORD TORREY.

Boston, Sept. 14, 1883.

#### Wright's ice-dam at Cincinnati.

I notice on p. 320 of *SCIENCE*, vol. ii. no. 31, an inaccurate report of what I said at the Minneapolis meeting, which does injustice both to Mr. Wright and to myself, and which I would beg to have corrected.

The reporter makes me speak slightly of Mr. Wright's discovery of the ice-dam at Cincinnati, as not sufficing to explain our Pennsylvania terraces. On the contrary, I expressed my admiration for the discovery as furnishing precisely the explanation we need for the *local-drift* terraces of the Monongahela, and the *rolled-northern-drift* terraces of the lower Alleghany, Beaver, and upper Ohio rivers.

The reporter probably mixed this up with what I said afterwards respecting the *rolled-drift* terraces of eastern Pennsylvania, which only reach a height of 800' A. T., in Northumberland county, and require some explanation, perhaps, quite unconnected with that which Mr. Wright certainly furnishes in a most satisfactory manner for the 800' to 1,100' A. T. terraces of the Ohio River basin.

J. P. LESLEY.

Second geological survey of Pennsylvania,  
Philadelphia, Sept. 15, 1883.

#### Erratic pebbles in the Licking valley.

While engaged in tracing the outcrop of 'Clinton ore' in eastern Kentucky, in the fall of 1882, I became interested in the pebbles, which in certain localities, and up to a certain height, were very abundant in the surface-soil.

Most abundant were rounded quartz pebbles, probably from the millstone grit. Somewhat less abundant were fragments of chert, showing little or no wear derived from the sub-carboniferous limestone. Still less abundant, though by no means rare, were some from the carboniferous, often containing characteristic fossils. They were confined, so far as I could determine, to the valley of the Licking and its larger tributaries. Vertically, they range from the river-bottoms to the top of the table, formed by the upper Silurian rocks, which borders on the Devonian

escarpment; so that these tables are quite uniformly covered with the material.

The distribution of the material is such as could only have been made while the valley was temporarily occupied by a lake. I was therefore led, though with some hesitation, to suppose that the glacier must have crossed the Ohio at Cincinnati, damming the river. I was not at the time aware of the labors of Mr. Wright in tracing the glacier across the Ohio.

Having now the certainty that there was a dam at the required point, I think I may have no hesitation in saying, that, during a portion of the glacial period, the valley of the Licking was occupied by a lake which overflowed laterally, and whose bottom became littered with materials brought from the mountains of eastern Kentucky by floating ice. They are most abundant where the ice may be supposed to have had freest access.

Terraces which might have been expected are wanting in the region in which my observations were made. Possibly they may be found in other parts of the valley, especially above; their absence in the region in question being due to the fact that only small portions of the region would have reached above the lake-level, which, by their disintegration, could furnish the material for terraces.

The overflow was probably to southward, but I could not search for it. Could it be traced, the amount of erosion might give some data for an estimate of time.

G. H. SQUIER.

Trempealeau, Wis., Sept. 14, 1883.

#### Depth of ice during the glacial age.

In the issue of *SCIENCE* for Sept. 7, reporting my paper at Minneapolis, I am made to say, that, during the glacial period, the ice was indeed "600 feet over New England, and very likely of equal depth over the area to the west." I said 6,000 feet over New England. The evidences of glaciation are distinct upon the Green Mountains to a height of nearly 5,000 feet. The lower summits of the White Mountains, like Carigain (which is 4,300 feet above the sea), are covered with transported boulders; and there can be little question that some found by Professor Charles Hitchcock, within a few hundred feet of the summit of Mount Washington, were transported thither by glacial agency. Such is the evidence for New England.

For the region north of Pennsylvania and the Ohio River, direct evidence of such a great depth of ice is naturally wanting; but, according to Ramsay, glacial scratches are numerous upon the summit of Catskill Mountains in New York, at an elevation of 2,850 feet above the sea. In southern Ohio there are numerous places where the ice, within a mile or two of its farthest extension, surmounted elevations which are about 500 feet higher than the plains to the north of them. I see no reason why it should not have been as deep over the bed of Lake Erie as over the region to the north of the White Mountains, though there are there no glaciometers like Mount Washington to measure the height of the frozen mass.

G. FREDERICK WRIGHT.

Oberlin, O., Sept. 13, 1883.

#### The 'stony girdle' of the earth.

In your issue of Sept. 7, just received, you are kind enough to insert a synopsis of the two abstracts of papers which I sent to the Minneapolis meeting. Allow me the space necessary to make a correction and some brief explanations. We are required to furnish these 'abstracts' to suit a printed form of small note size, which is apt to lead to small chirography: hence I suppose the mistake in reading and printing the title.

It should read, 'stony girdle,' and was in inverted commas to show that the name did not originate with me. My special object was to call attention to its being, in a great measure, the same belt which forms the prime-vertical when the pole of the land-centre at Mount Rosa is brought to the zenith. The unfavorable comments to which you allude have force as a general rule; namely, that closet geology is not comparable to observations in the field. Yet *all generalizations* may be called closet geology, as being the result of a large number of facts collected in the field, and compared subsequently. As it would, however, be presumptuous in any one to offer generalizations who had not had somewhat extended opportunities for observation, I may be permitted to mention, as some justification, those I have enjoyed. In North America my observations, partly in special work, partly during travel, have ranged from Rainy Lake, north of Lake Superior, to Saltillo, in Mexico, and from the Atlantic states to the head waters of the Gila, in Arizona. In the eastern continent, I travelled from the north of Scotland to Cairo in Egypt, ascending Etna, and spending the vacations of three summers, during college-life, in Switzerland among its mountains, ranging subsequently from western France to the Crimea. In 1824 I saw the 'Perte du Rhône,' where that river disappeared for miles, and then re-appeared,—a phenomenon no longer to be seen, as the superincumbent rocks, some years later, caved in, and converted the subterranean into a subaerial bed for that fine stream.

In 1829 I visited the scene of the catastrophe at New Madrid; and while granting a local subsidence for the immediate cause, as claimed in the able paper by Dr. Macfarlane, of which you give an abstract, I am compelled to believe that the remote cause was due to a seismic movement, felt, as Mallet states, at least two hundred miles from New Madrid, and, indeed, affecting large and more distant areas about that time, as mentioned in Key to geology, p. 77.

These opportunities, in connection with the specimens and notes of reference brought home, permit a review of general geology, which I thought might enable me to present to the student of geography and geology some broad principles and truths into which the details subsequently obtained by him might be appropriately fitted: hence the paper read at the Boston meeting, showing that the eastern trend of each continent was distant one-fifth of the circumference of the globe from its adjoining continental trend; also that each continent presented a central focus, from which a circle with radius of  $36^\circ$  would embrace the land proper,—sometimes excluding a peninsula, such as Hindostan, sometimes including adjacent islands, as those of Madeira, Canary, and Cape Verd, as belonging to the main continent, Africa. The Montreal papers were designed to show the important seismic fissurings radiating from the pole of the land-centre; also the relation between solar and terrestrial dynamics, where seismic phenomena are transmitted along great circles coinciding with the sun's apparent path, or along belts of the earth's crust which are secondaries to the ecliptic.

The occurrences of the last few weeks seem to corroborate the generalization offered, inasmuch as Ischia is on the  $30^\circ$  fissure from Rosa, at no great distance; while Java and the Straits of Sunda, as well as Guayaquil, more recently disturbed, are on or close to the prime-vertical.

If these generalizations belong rather in the category of instruction for the student than of contributions to science, perhaps my twenty-five years of natural-science teaching may present some excuse.

Certainly, my great aim and desire are to arrive at important scientific truths, especially general laws in the dynamics of our globe. RICHARD OWEN.

#### Mr. Morse's papers at Minneapolis.

A number of errors have been made in the report of my papers which were read at the Minneapolis meeting.

In the paper on an apparatus for warming and ventilating apartments, the statement that the temperature of a hall was raised  $40^\circ$  above the outside temperature is incorrect. I said that the air, as it entered the room from the heater, had been raised  $40^\circ$  above the outside air.

In the paper on the methods of arrow-release, I spoke of the English method, which was probably that of the Saxon, and said that American archers followed the English. The Japanese never use thumb-rings, to my knowledge. The Koreans, Chinese, Manchu Tartars, and Persians use the thumb-ring.

A more serious mistake occurs in the report of my paper on the indoor games of the Japanese. I said very distinctly, that, in the game of chess, pieces captured could be used by the capturer against his opponent. In comparing the Japanese games with ours, I made no allusion to seven-up or whist. With every one I regard whist as next to chess in character as a highly intellectual game.

You will confer a great favor by publishing these corrections. EDW. S. MORSE.

Salem, Mass., Sept. 16, 1883.

#### Evidences of glacial man.

In SCIENCE, no. 32, p. 384, the statement is made, respecting Miss Babbitt's Minnesota finds, that "thus far, at best, the glacial workman is known only by his chips." What better evidence, I would inquire, is needed, if those chips are of artificial origin?

Is not this sufficient? Are not shavings and sawdust as good evidence of men working in wood, today, as are the planes and saws they use? From the very nature of the case, it is unreasonable to find as abundant and easily recognized evidence of man in drift-deposits as upon the surface-soils; yet this is what some of those present at the Minneapolis meeting of the American association for the advancement of science seemed to require.

In the case of the 'paleolithic' implements of the Delaware River valley, other evidence than the chipped stones has been found. The human tooth, lately described in detail in the Proceedings of the Boston society of natural history, is, of itself, evidence of man's presence at the time the gravels, in which it occurred, were laid down. Other human remains have also been found.

A word, too, with reference to the implements. These are nearly all as unmistakably artificial as the most finished arrow-head. Objects of identical character are found among the relics of the recent Indians, and are not questioned. Why, then, should a similar class of objects, found in gravel-deposits that antedate the superincumbent surface-soils, be questioned?

There is no doubt overshadowing the existence of man in the Delaware valley as long ago as the close of the glacial period: his presence, then, is not merely 'a theory advanced by Dr. Abbott,' as you suggest, but a fact susceptible of actual demonstration.

Professor Mason, in his address (in the same issue), asks, "What is the real import of such discoveries as those of Dr. Abbott and Professor Whitney in establishing the great antiquity and early rudeness of