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° 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° 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° above the outside temperature is incorrect. I said that the air, as it entered the room *from the heater*, had been raised 40° 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

the American savage?" Speaking for myself, I would suggest that his question contains its answer. My discoveries have established the glacial age of man on the Atlantic seaboard of America, and at that time his culture was that stage known as 'paleolithic.'

CHAS. C. ABBOTT, M.D.

Trenton, N.J., Sept. 18, 1883.

THE ALPHABET.

The alphabet, an account of the origin and development of letters. By ISAAC TAYLOR, M.A., LL.D. 2 vols. London, *Kegan Paul, Trench, & Co.*, 1883. 16+358; 398 p. 8°.

MR. TAYLOR has produced an admirable work on the interesting subject of alphabetic writing. It abounds in wealth of collected material, down to the very latest discoveries (some of them of the utmost importance). By lavish and well-chosen illustration it puts this material before the apprehension of the reader or student with the most desirable clearness; and its digest and criticism of former opinions is made with impartiality and independence of judgment, while the author adds abundantly of new views, and arguments to support them. No other existing work of a like character can bear any comparison with it; and it deserves to have, as it doubtless will attain, a wide circulation and popularity.

In the main, these volumes are filled with the history of our own alphabet and its relatives, or of the ancient Phoenician with its descendants and probable ancestor, since other systems of alphabetic writing are comparatively insignificant in number and in importance. The Chinese characters are not alphabetic, although one or two derivatives from them (as the Japanese *kata-kana*) have that character. The cuneiform mode of writing ended its career in an alphabetic system, the Persian; but all the peoples using cuneiform passed over, more than two thousand years ago, to the side of the Phoenician. There have been other hieroglyphic schemes, in the old world and the new, that made advances, no one can say just how far, toward alphabetism; but they are long since perished without descendants. All these, together with such theoretic basis as he chooses to lay for the science, Mr. Taylor despatches in the first chapter (seventy pages) of his first volume; the rest is devoted to our alphabet: the various kindred Semitic forms of it being treated in the former volume, and the Indo-European forms, with the few outside stragglers, in the latter, under the divisions of Greek, derivatives of Greek (Italian, Coptic, Slavonic, Albanian, Runic, Ogham), Iranian, and Indian. The method is not to be condemned, although we might have desired a more ample theoretical introduction. The fundamental principle of alphabetic history is distinct, and briefly statable: all writing begins necessarily with the depiction of scenes and objects, or is purely pictorial; it everywhere tends to pass over into a depiction of the names of objects; and, when it has fully reached that condition, it has become alphabetic. There can be no such thing as an alphabet not starting from a pictorial stage, any more than a spoken language without an initial imitative root-stage. But while in language we can only get back by inference to such a state of things, because the beginnings of language are so remote from us, in writing we find the pictorial stage abundantly represented.

Whether that stage is discoverable in the actual history of our own alphabet, is a question not yet absolutely settled. Every step by which our familiar letters go back to the primitive Semitic alphabet, usually called by us Phoenician, is traced out with the utmost distinctness. The Phoenician is purely, though defectively, alphabetic. It must, then, have come from a pictorial original. Three such systems of writing are found in its neighborhood, — Egyptian, cuneiform (the perhaps sufficient, though rather scanty, evidences of whose hieroglyphic origin are given by our author), and the recently discovered and still obscure Hittite. Did it come demonstrably from one of these, or has it an ancestor now lost to us? As is well known, De Rougé's work, published less than ten years ago, attempted to show its derivation from Egyptian, from hieratic characters, of known hieroglyphic originals; and his view is widely, though by no means universally, accepted. Mr. Taylor is a firm believer in it, and sets it forth with much clearness and force. We find ourselves unable fully to share his conviction. De Rougé endeavored to prove more than was reasonable, and found it so easy to prove all he undertook, that his very success casts a shade of unreality over the whole comparison. We may allow that his identifications are both possible, and, as a whole, plausible quite beyond any others yet made. Yet whereas the derivation of the Greek or of the Arabic alphabet, for example, is past all doubt, and he would rightly be passed by as a time-waster who should attempt to re-open the question, no reproach can attach to the scholar who, unconvinced by De Rougé, should try to find another and better solution of the problem, as some are actually doing. Mr. Taylor overstates the desirableness of acquiescing in the