higher on the west side of the break than on the east side, and duplicated by the lateral and upward thrust for nearly two feet before it droops to and passes into the smutty coal of the break.

From what is exposed, it appears that a part of the upper hill, at least down to and including the coal and fire-clay, has, from some cause, moved on the underlying strata; and at the fault the coal-bed has been broken and forced upon itself for two or three feet. The coal next the mouth not partaking of the motion of that farther in the hill, I could find no detritus of the removed part of the top coal, 10 to 18 inches of which is wanting from the opening to the fault. This would tend to prove that the faulting might have occurred in carboniferous times. The exposure of the roof-shales is not sufficient to prove the absence of such detritus. The condition of the coal at the line of fault would point to a geologically recent date of disturbance. Jefferson county is outside the region of glacial drift. SAMUEL HUSTON.

Richmond, Ohio.

The Leadville porphyry.

In the American naturalist for November, 1882, I find the following note: —

"*The so-called Leadville porphyry.* — Professor Alexis Julien read a paper at the Montreal meeting of the American association, on this subject, in which he described the result of his examination of the rock in question, in thin sections under the microscope. He finds that it is not an eruptive rock, but is sedimentary. Its material consists of the *débris* of the erosion of plutonic rocks redeposited in the Silurian ocean. He concludes that the rock is not a porphyry, but must be called a felsite tufa. The importance of this conclusion in estimating the form of any metallic ores contained in this deposit is obvious, and will be invaluable to mining experts."

Having spent the better part of two years in a detailed study of the Leadville region, an abstract of the results of which was published about a year since, I feel it my duty to correct any misapprehension which may arise from the above statement. The paper to which it refers I have not yet been able to see, and cannot, therefore, tell exactly to which of the many varieties of porphyry occurring at Leadville Professor Julien refers. I have seen slides of his in the possession of a gentleman at Leadville, which I have reason to believe were made from specimens of the rocks to which I gave the local name of 'gray porphyry,' and which had been labelled by him 'felspathic gneiss.' To whatever porphyry he may refer, however, I have no hesitation in saying, that his microscopical determinations have led him utterly astray. On what ground he decides from the simple inspection of a thin section of a rock of this character, whether it is sedimentary or eruptive, I am unable to conceive. Microscopical lithologists in Europe, and their pupils in this country, hesitate to do this without the aid of field-observation; and, as far as I know, it is only a few Americans who have obtained their knowledge of this science independently of such adventitious aid, — and who therefore, in their own opinion, know much more than those who originated the science, — that feel themselves competent to decide on the character of a rock without any knowledge of its field-habit or mode of occurrence. The mischievousness of this assumption is illustrated in the present case, where an utterly mistaken statement is given to the public by one whose name and position should be guaranties of scientific accuracy. Quite aside from any microscopical evidence, — as regards which, it is unnecessary to say, I differ essentially from the above-quoted statement, — *all the Leadville porphyries are most distinctly eruptive.* They occur largely as sheets between sedimentary beds, it is true; but they also cross these beds, occur as dikes, and carry within their mass larger or smaller portions of the enclosing sedimentary beds, as caught-up fragments.

To the writer of the above-quoted article, I would say, that, though in one sense a mining expert myself, I fail to see any possible use which Professor Julien's conclusions, had they been correct, would have been to me 'in estimating the form of any metallic ores contained in this deposit,' even had the Leadville ores been contained in porphyry, which, as a rule, they are not. S. F. EMMONS.

U. S. geological survey, Washington, D.C.

Sand-tracery.

My attention was called last fall to the curious markings, formed chiefly by the agency of plants and wind, on the beach of Lake Champlain. Seeing a notice of similar phenomena observed on the seashore by a correspondent in the second number of SCIENCE, I would add the following, which tends only to confirm some of his statements: —

In passing over the smooth beach of Burlington Bay, one is struck, first of all, by the porous condition of the sands just outlying the portions within reach of the waves. Unacquainted with this appearance, he might attribute it to some sand-boring insect, did not a closer observation teach him at once that it was effected by the spray, and due to the bursting of air-bubbles. The sand sifts over these holes until they are entirely concealed, or only a small opening is left, out of which one might not be surprised to see an insect emerge at any moment. He would also notice numerous tracings referable to the tracks of small animals. These are frequently regular and clean cut, and resemble impressions which are seen in the triassic sandstones of the Connecticut river. Again: a little observation stands one in good stead, as it shows these to be made by dry frizzled algae, rolled onward by the wind, as was remarked in the letter above referred to, or successively raised and dropped, making still more deceptive impressions. A leaf is often trundled along by a slight breeze, indenting the sand in a very regular, though seemingly fantastic manner.

Furthermore, I have frequently noticed a curious print made by the pliant stem of an alga, which had become attached at one end. The remaining portions, being at the sport of the wind, describe concentric circles at every point of contact. I thought at the time how little imagination would be required to endow such simple examples of nature's geometry with the higher characteristics of plants and animals. Would it not be worth while for some one who has the opportunity and leisure to make a comparative study of these markings, and determine how many of such trifling phenomena have been exalted higher than they deserve? F. H. HERRICK.

Burlington, Vt., March 1, 1883.

WHITNEY'S CLIMATIC CHANGES.[1]

III.

THE second part of this article discussed the relation of a general change of atmospheric temperature to glaciation. We now come to consider its relation to desiccation.

Because all precipitation depends on evaporation, and because rate of evaporation di-

[1] Concluded from No. 6.

minishes with the lowering of temperature, Professor Whitney conceives that a general lowering of terrestrial temperature by reason of the dissipation of solar energy will make the arid regions of the earth more arid; and he therefore cites the drying-up of rivers and lakes in regions already exceedingly dry as evidence of a general lowering of temperature. By approaching the subject from a different side we may reach a very different conclusion.

If terrestrial warmth, instead of emanating from a single celestial body, were due to an equable radiation from the whole sphere of space, there would be no atmospheric circulation. The whole air would be saturated with moisture, and the whole surface of the earth would be wet; but there would be no precipitation, no evaporation, no streams. We may therefore consider saturation the normal or static condition of the air, and wetness the normal condition of the land. The actual inequality of extraneous radiation — the relative intensity of solar radiation — is a disturbing factor. It produces atmospheric circulation, thereby causing precipitation, and diminishing the humidity of the atmosphere so that evaporation becomes possible. Precipitation is the necessary condition of evaporation. By precipitation and evaporation, inequalities are introduced in the distribution of moisture upon the surface of the land. Where precipitation preponderates, the condition becomes moister than the normal; where evaporation preponderates, it becomes drier. Excessive aridity, therefore, as well as excessive humidity, is caused by solar heat; and every increase of solar radiation tends to magnify the contrast between moist regions and dry regions, making the moist moister and the dry drier.

If our author has fallen into error in his fundamental postulates, we need not be surprised to find that facts have proved stumbling-blocks to him, and that he has involved himself in numerous inconsistencies. It will be profitable to call attention to some of these.

On p. 341 he asserts that the recession of the glaciers of the Alps is part and parcel of a general phenomenon of desiccation; and this desiccation his theory ascribes to a general lowering of temperature. On pp. 240 and 296 he notes as evidence of this same lowering of temperature the extension of glaciers in Iceland and the increased abundance of icebergs in the north Atlantic. Thus the extension of glaciers in one region, and their shrinkage in another, are both assigned to the same degradation of climate.

Having asserted that the phenomena of the glacial epoch in Scandinavia had their origin in local causes, and that the cognate phenomena, not only in the Alps, but in the Pyrenees, the Vosges, and the Caucasus, were part of the same system of events, he nevertheless declares that the ancient glacial phenomena of the Himalaya, of New Zealand, and of the Sierra Nevada, are not of sufficient importance to call for special explanation. And yet the glaciers of the Himalaya and New Zealand have shrunk, since their greatest extension, more than those of the Caucasus and Pyrenees; and the system of glaciers that has disappeared from the Sierra Nevada was greater than that ascribed to the Vosges. If the lesser changes are worthy to have a cause assigned them, why should the greater be ignored?

It is stated that the precipitation on the Sierra Nevada was very great in tertiary time, and has since continuously diminished. At a very late geological date the valleys of the range were occupied by glaciers; and the explanation given is, that the precipitation was greater then than now. But no suggestion is offered in explanation of the fact that at an earlier period, when the precipitation was still heavier, there were no more glaciers than at present.

This instance may be classed with a number of others, in which phenomena consistent with his theory are looked upon as systematic, while those of an opposite character are regarded as temporary or unimportant. The rise of the lakes of the Great Basin, since the first observations thirty-five years ago, appears to him a temporary oscillation; but the fall of the Lake of Valencia during a period of fifty years is made one of the proofs of a general desiccation, and the subsequent rise of the same lake does not find mention. The recent recession of the glaciers of the Alps is referred to a secular and general cause; but the contemporaneous advance of the glaciers of Spitzbergen is assigned a local cause, while the advance of the glaciers of New Zealand is ignored. The semi-periodic blocking of the Rofenthal by ice is mentioned as a curious anomaly, apparently without any realization that it points to a substantial uniformity of mean conditions for a period several times longer than that of the glacial recession upon which stress is laid.

One of the most curious features of the book is its assumption of the possibility of detecting evidence of a secular change of climate within the brief period of human history. To one who has the geologist's conception of geologic time the idea is so extravagant as to be fairly

grotesque. Let us consider it a moment. Silurian fossils have been found, not only in arctic and temperate regions, but within the tropics. By a slight exaggeration of the possible conditions of animal life we may admit that the general climate of the earth was then 50° C. warmer than at present. The lowest estimate that has been offered from the geologic or the astronomic stand-point for post-silurian time is five million years, which gives us a fall in temperature of one-thousandth of a degree in each century. Can it be that Professor Whitney thinks a change in temperature of one-thirtieth of a degree was sufficient to degrade Arabia from a centre of civilization to a desert? and to rob successively Persia, Greece, and Italy, of the prestige of empire? Has a change of one-hundredth of a degree so modified the climate of Greenland as to nearly depopulate it? Can it be that the same change has perceptibly modified the distribution of cultivated plants in France? Has a change of the two-thousandth part of a degree caused the Alpine glaciers to recede several thousand feet? and the Lake of Valencia to lay bare broad tracts for cultivation? And, finally, was it worth while to make a serious investigation of the thermometric data of the past century in the hope of detecting a change of the thousandth part of a degree?

TERRACES AND GRAVELS.

In one place or another our author states correctly all the fundamental principles of the action of rivers in erosion and deposition; but a strange fatality attends his application of them.

It is a conspicuous fact, that running water, under some circumstances, erodes its bed, and that, under other circumstances, it builds up its bed by deposition. The conditions which directly determine the performance of the one or the other of these functions are *load* and *velocity*. We may define the load of a stream as the ratio of its transported *débris* to the volume of its water. With a given velocity a stream is able to transport a certain load: an increase of load leads to deposition; a decrease, to erosion. Conversely, to transport a given load a certain velocity is required: an increase of velocity leads to erosion; a decrease, to deposition. Under ordinary circumstances the load of a stream at flood-stage is not subject to great variation; so that the determination of deposition or erosion is usually due to velocity. Velocity is a function of grade and volume. An increase in the angle of slope increases the velocity and tends to make a stream erode; a decrease in the angle of slope tends to produce deposition. An increase in volume gives a greater velocity and tends to induce erosion; a decrease in volume diminishes velocity and tends to induce deposition.

It follows from this, that a stream which flows with so little velocity as to form a deposit in its valley may, by an increase of volume, be made to excavate its channel more deeply, and thus abandon its old flood-plain, leaving a portion of it as a terrace on the side of its valley. If, therefore, a stream be found bordered with terraces, and if there be good reason for the belief that the inclination of the valley through which it flows has not been changed, it is proper to infer that its volume was formerly smaller. By drawing the opposite and erroneous inference, Whitney has been led to see evidence of swollen streams — and therefore of excessive precipitation — where, in reality, none exists. In point of fact, river-terraces are nearly always produced by orographic changes; and it may be doubted whether there are any localities where the effect of orographic movements can be so far eliminated as to permit fluctuations in precipitation to be inferred from river-terraces.

If Whitney had escaped this error, it is possible that he might not have been drawn into a study of geologic climate; for it enters into his original discussion of the auriferous gravels. He there infers that the pliocene rivers were large, because they deposited their load high up on the flank of the Sierra; and that the modern rivers are relatively small, because they have carved cañons in the same region. It may, indeed, be true, that the pliocene precipitation and streams were relatively great; but these facts, so far as they have any bearing, point in the opposite direction.

If, however, we dismiss the idea that the behavior of these rivers was dependent upon their volume, we can find a more plausible explanation of the phenomena by referring them to change of inclination. If the inclination of the western flank of the Sierra was exceedingly gentle in pliocene time, it would be natural for its streams to form deposits on the lower slopes; and if afterward an elevation occurred, increasing this inclination, the habit of the streams would be reversed, and the cañons we see would result. That such a change in inclination has actually taken place is rendered probable by other considerations. In the first place, the western face, which is far broader than the eastern, is, as described by Whitney and others, an inclined plain, interrupted only by the narrow cañons of the

modern streams. Its plateau character is not given by a continuous stratum of hard rock parallel to the general surface, but has been produced by the uniform erosion of a system of plicated strata. Such uniform erosion could only have been accomplished by streams flowing at a low angle. Second, the eastern boundary of the range or plateau is a line of faulting; and the orographic movement producing the range consisted of a displacement along this fault-line, and a consequent inclination of the plateau-like mass to the westward. That this movement belongs to late geologic history is strongly indicated by the fact that it is incomplete. Some unpublished observations by Mr. I. C. Russell show that a part of it has occurred since the date of the quaternary lakes of the Great Basin; and the Inyo county earthquake brings it down to 1872.

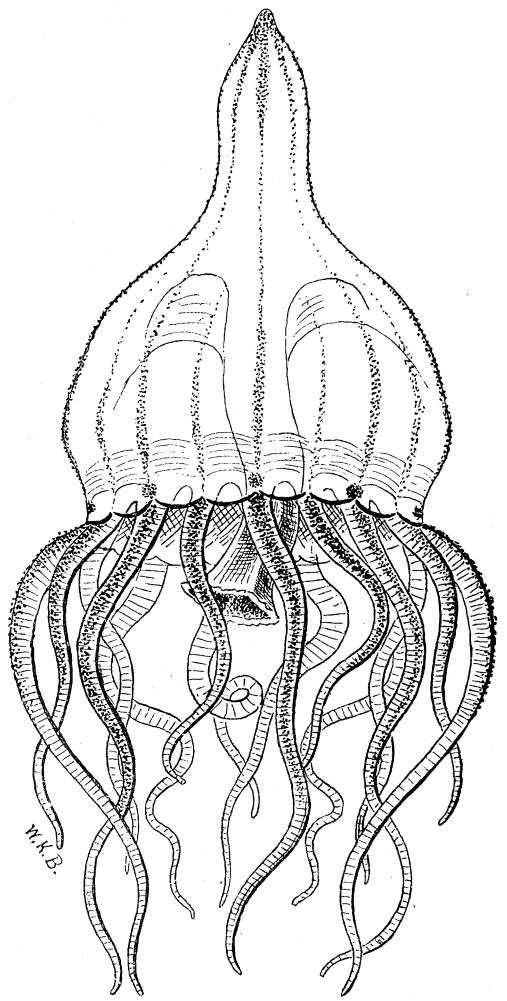
If a rise of temperature is not favorable to glaciation, if a fall of temperature does not make deserts drier, and if river-terraces are not indicative of waning precipitation, it might seem that our author's theory is badly off; but the case is not hopeless. The paleontologic evidence, and the doctrine of the dissipation of solar energy, remain; and if he will now devote himself to the investigation of the glaciers that are known to have recently increased, to the dry countries in which civilization and wealth have supplanted barbarism and poverty, and to the rivers that are engaged in filling up the valleys they once excavated, he may yet find in recent history the evidence he seeks of a secular change. G. K. GILBERT.

DEEP-SEA MEDUSAE.

Report on the deep-sea Medusae dredged by H.M.S. Challenger during the years 1873–76. By Prof. ERNST HAECKEL. London, 1882. 105 + 154 p., 32 pl. 4°.

THE expedition obtained only eighteen Medusae from deep water; and some of these, such as the beautiful Margelid, shown in plate 1, are undoubtedly surface-forms. But the value of the collection must not be estimated by its size: for some of the species are very primitive forms, or ancestral types, and are therefore of the greatest scientific interest; while others present unique and remarkable modifications of structure to adapt them to their life on the bottom.

Among the latter are the Pectyllidae, — a new family established by Haeckel, to include three genera of Medusae, obtained by the Challenger at a great depth in the Arctic Ocean, the Antarctic the Indian Ocean, and the Mediterranean. They bear a close resemblance to the Trachynemidae; but they are furnished with great numbers of ambulatory tentacles, which are wonderfully like the sucking-feet of echinoderms, terminating, like these organs, in expanded sucking-disks. As Haeckel has obtained living specimens of the Mediterranean species, and has thus been able to supplement his account of the anatomy by observations of the living animal, we have an interesting account of its habits in confinement. He says that it usually lies on its back, extends a portion of its sucking-feet stiffly out around it, and thus attaches itself to the bottom of the glass: the other sucking-feet play freely in the



Tesserantha connectens in profile, ten times the natural size. Outline-sketch from Haeckel's Deep-sea Medusae, Pl. 15, Fig. 1.