methods, and the new questions raised by investigation, many series of experiments will be undertaken, the outcome of which will definitely settle the question of the entrance of free nitrogen into vegetable tissues. If this question be answered affirmatively, agricultural science will not place bounds to the possible production of foods. If the nitrofying process does go on within the cells of plants, and if living organisms do fix free nitrogen in the soil in a form in which at least a portion of it may be nitrified, we may look to see the quantities of combined nitrogen increase *pari passu* with the needs of plant life. Thus, even intensive culture may leave the gardens and spread over the fields, and the quantities of food suitable for the sustenance of the human race be enormously increased.

In regarding the agricultural economies of the future, however, it must not be forgotten that a certain degree of warmth is as necessary to plant development as potash, phosphoric acid, and nitrogen. If it be true, therefore, that the earth is gradually cooling, there may come a time when a cosmic athermacy may cause the famine which scientific agriculture will have prevented. Fortunately, however, for the human race, the cereals, the best single article of food, are peculiarly suitable to a cold climate. Barley is cultivated in Iceland, and oatmeal feeds the best brain and muscle of the world in the high latitudes of Europe.

It is probably true that all life, vegetable and animal, had its origin in the boreal circumpolar regions. Life has already been pushed half way to the equator, and slowly but surely the armies of ice advance their lines. The march of the human race equatorwards is a forced march, even if it be no more than a millimetre in a millenium. Some time in the remote future the last man will reach the equator. There, with the mocking disc of the sun in the zenith, denying him warmth, flat-headed, and pinched as to every feature, he will gulp his last mite of albuminoids in his oatmeal, and close his struggle with an indurate in hospitality.

NOTES AND NEWS.

ACCORDING to the report of Gustavus Hinrichs of the weather service of Iowa, that state, since the middle of May, has been subjected to a drouth, the most severe on record. The most serious drouth preceding the present one prevailed during June and July of 1863, when for sixty days no serviceable rains fell in Iowa City; but rains had been sufficiently abundant till the end of May, and nearly five inches of water fell during the first ten days of August. In the early summer of 1886, the last good rain fell on May 13. After that time, there was no rain reaching half an inch until August 4, — eighty-three days without a serviceable shower! The total rainfall during that period was less than one inch, while the normal rainfall would be nearly ten and a half inches. But, notwithstanding this extreme drouth, it cannot be said that there is a failure of crops; because farming operations in that state are so diversified that a total failure is almost an impossibility.

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.*

Glaciers and glacialists.

THE number of *Science* for the 23d of July last contains a paper by Mr. Jules Marcou, in which he refers to my memoir on Professor Guyot (published by the U. S. national academy), and denies statements cited by me from a publication by Professor Guyot with regard to the latter's glacier discoveries. Mr. Marcou commences his criticism on the subject with the following paragraph: "At Princeton Guyot was long isolated from intercourse with Swiss naturalists; and at the close of his life, while suffering under the malady which proved fatal in 1884, he put forth claims of doubtful value. These are the facts." Then follow the facts as Mr. Marcou understands them.

Mr. Marcou's statement is wrong in important points. Professor Guyot gives an account of his own discoveries of 1838 in his memoir of Professor Agassiz, which was read before the national academy, the first part in October, 1877, the second in April, 1878. This is six years before his decease, while he was still engaged in his laborious topographical survey of the Catskills. The following is the paragraph from the Agassiz memoir: —

"In the spring of 1838 I had the pleasure of a visit from my dear friend Agassiz in Paris, where I then resided. The main topic of conversation was, of course, the glaciers. He put me *au courant* of Charpentier's views, as yet imperfectly published (his book having been issued only two years later, in 1840), and adding his own idea of a general glacier era, he urged me to turn my attention to these phenomena. I asked to be allowed to suspend my judgment until my own observations should justify my adhesion to so startling a theory, but promised to visit the glaciers that very summer. I did so, and an exploring tour of six weeks in the Central Alps rewarded me beyond my expectation. The glacier of the Aar, on which Agassiz began two years later (1840) his regular system of observations, taught me the law of the moraines. The glacier of the Rhone gave me the law of the more rapid advance of the centre of the glacier, and that of the formation of the crevasses, both transversal and longitudinal. The glacier of Gries showed me the laminated, or ribboned (blue bands) structure of the ice deep down in the mass of the glacier, and the law of the more rapid advance of the top over the bottom. On the southern slope of Mont Blanc, the great glacier of La Brenva, with its twin rocks, rising like two dark eyes from the middle of the ice (they are, indeed, called by the mountaineers the 'eyes of the glacier'), made me understand that the motion of the glacier takes place by a gradual displacement of its molecules under the influence of gravity, giving it a sort of plasticity, and not

by a simultaneous gliding of its whole mass, as believed by de Saussure. All these laws, deduced from a first, but attentive, study of the phenomena of the glaciers, were, at that time, — excepting that of the moraines, — new for science. They were expounded by me, and illustrated by diagrams, at the meeting of the geological society of France, in session at Porrentruy, the same summer of 1838; and I had the great satisfaction of seeing them fully confirmed by the subsequent observations of Agassiz and others, which furnished the precise numerical data then wanting for their complete elucidation. This paper, however, though duly mentioned in the proceedings of the geological society (*Bulletin*, vol. ix. p. 407), was not printed, owing to a protracted illness of its author in the winter following. But on the occasion of a claim by Prof. J. D. Forbes to the discovery of the laminated, or ribboned, structure of the ice, the portion relative to this subject was printed, and the whole manuscript, on a motion of Agassiz, was deposited, by a formal vote, as a voucher, in the archives of the Society of natural sciences of Neuchatel, the original draft being now in my hands. If I mention this circumstance, it is because the regrettable omission of the publication of my paper was the occasion of the unfortunate misunderstanding which estranged two such men as Agassiz and Forbes, and which I feel bound, in a measure, to explain."

The manuscript referred to in the latter part of this citation was sent to the Society of natural sciences of Neuchatel early in 1883, was read at the session of the society on the 12th of April, 1883, and published in its *Bulletin* of the same year. I have a copy of the published paper, which I received from the secretary of the Neuchatel society. It is in French, as first written, and its title page, and also the cover, bears the heading, 'Observations sur les glaciers des Alpes en 1838, par M. Arnold Guyot.' I had thus, in Guyot's memoir of Agassiz, and this publication by the Society of natural sciences of Neuchatel, the fullest authority for my statements, and also, in this and other ways, abundant reason for confidence in Professor Guyot. Moreover, his memoir of Agassiz bears evidence throughout that his friendship for Agassiz, as I know from long and intimate intercourse with him, was, to the end, that of a brother.

In the same memoir, Guyot says of Venetz and Charpentier — names mentioned by Mr. Marcou — and of Agassiz's great results:

"If to Venetz and Charpentier belongs the honor of having first proved the transportation of the Swiss erratic bowlders by the agency of ice, and the existence of great glaciers formerly extending to the Jura, to Agassiz we must accord the merit of having given to these facts their full significance; of having brought them before the world at large, and having made the glacial question, as it were, the order of the day. By his sagacity he found glacial action where it was never suspected before, pointed it out to the astonished and unbelieving English geologists on their own soil; found it in North America; traced it with undoubted evidence in the temperate regions of South America; and believed, though with hardly sufficient reason, that he had seen it on the vast plains of the Amazon. He proved the phenomena to be well-nigh universal." Thus Guyot does justice to his friend, and recognizes the earlier work of Venetz and Charpentier.

My academic memoir of Guyot closes with the following sentence: "As fellow-students, we have special reason to admire in Guyot — as he wrote of Humboldt — 'that ardent, devoted, disinterested love of nature, which seemed, like a breath of life, to pervade all his acts; that deep feeling of reverence for truth, so manifest in him, which leaves no room for selfish motives in the pursuit of knowledge, and finds its highest reward in the possession of truth itself.'" I know this to be a just tribute.

Mr. Marcou's remark condemnatory of Professor Agassiz's 'successor at Harvard college,' for "having denied, *in toto*, in a publication founded by Agassiz, — 'The memoirs of the Museum of comparative zoölogy,' — his [Agassiz's] great discovery of the 'ice age,' but having, more than that, ignored him altogether as the discoverer of the existence of ancient glaciers in the British Dominions, in New England and New York, in Brazil, in the Straits of Magellan, and in Chili," is essentially groundless. 'The memoirs of the museum,' referred to, contain, among its volumes, a work entitled 'The climatic changes of later geological time, by J. D. Whitney,' and this is the only ground presented by Mr. Marcou for the charge he makes. Mr. Whitney's work opposes accepted views on ancient glacier distribution, and therein opposes Agassiz, and nearly all geologists living; but he has not a word of disparagement for Agassiz, and gives no just cause of personal complaint. Mr. Marcou's charge against Mr. Alexander Agassiz has no other foundation, and is not true to the views he holds, and has always held, with regard to his father's work and discoveries connected with glaciers and the 'ice-age.' The memoirs of the museum of comparative zoölogy, founded by Mr. Alexander Agassiz, and not by his father, has been for some time sustained, and the museum work carried on, with the grandly generous outlay on Mr. Agassiz's part of several hundred thousand dollars; and he has never made the stipulation, which the objector seems to require, that the publications should contain nothing in opposition to his own, or his father's opinions.

JAMES D. DANA.

New Haven, Aug. 11.

Lacustrine deposits of Montana.

The examination of the Gallatin valley in Montana, by the writer, under the supervision of Dr. F. V. Hayden, during the summer of 1885, has developed some points of general interest in relation to the old lake basins of that region. Dr. Hayden was the first to demonstrate the fact that the western country, during the tertiary period, was covered to a greater or less extent with lakes, the waters of which, as the tertiary period progressed, gradually changed from brackish to fresh; until in pliocene time there were numerous fresh-water lakes scattered all over the area of the west, from the Mississippi valley to the Pacific coast.

The first of the basins described by Dr. Hayden was the one lying east of the Rocky Mountains, and extending from the Niobrara River to an unknown distance south of the Platte River. He estimated that this lake must have occupied an area of from 100,000 to 150,000 square miles. To the beds deposited in this lake the name of the Loup Fork group was given; and they were found to shade imperceptibly into an upper group, to which he gave the name of Post-Pliocene, the lower strata having been