ETHNIC problems have a natural interest for the American people. Their great task is to fuse together the life of many lands, — to bring political and social union out of the widest diversities that the races of men afford. They follow a true instinct in giving time and public money to such problems. The bureau of ethnology is doing an admirable work in gathering the history of our departing aborigines. There is, however, another field of labor, — one not yet fairly entered on, either by private students or by the ordered phalanxes that are marshalled in the cause of science by the bureaus of the federal government. As the indigenous savages were forced towards the setting sun by the plough-driving Aryans, the shore was crossed by another savage race, the African, that has come to stay for all time in our fields.

There can be no question that the African in the United States presents us with the greatest and most interesting experiment that has ever been tried by civilized man upon a lower people. Around this race have gathered a host of problems of the utmost importance to pure science, and of infinite interest in that field of nature called sociology, into which science is with such difficulty making a slow and blundering way. Out of the very numerous inquiries that should be made in this field we may note the following, that are at the moment, perhaps, the most important because they concern matters that need to be studied at once. *First* among these is the question of the origin of our American negroes. There is a great deal that still can be gathered concerning this question. No close observer of the negro race in this country can fail to have noticed the wide diversity of type masked behind the deceiving uniformity of hue. *Second*, we have the problem of the physical and mental change that has come over this people since their removal to America. *Third*, the effects of climate in different parts of the United States upon these black races, — effects on shape, liability to disease, longevity, etc. What to do with and for the negro, and how to do it, is the question of all questions most immediately and imperatively before us. We best begin to deal with it by making a scientific study of him.

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.*

The initiation of deep-sea dredging.

In a recent number of *Science* (July 18), Mr. Rathbun is rather severe upon European naturalists for their supposed ignorance of the fact that the Gulf-Stream dredgings carried on by the Corwin, under the superintendence of the late Mr. Pourtalès, were commenced in 1867, the year before the first British expedition in the Lightning; and he speaks of Mr. Pourtalès' report of December, 1867, as having been 'utterly ignored' by European writers.

It is quite true that no reference was made to this report in the historical account of the subject which formed part of the preliminary report of the dredging operations of the Lightning, presented to the Royal society by Dr. Carpenter on Dec. 17, 1868; for the bulletin of the Museum of comparative zoölogy, which contained Pourtalès' report, had not then reached him. The correspondence between Dr. Carpenter and Sir Wyville Thomson, which led to the cruise of the Lightning (published as an appendix to Dr. Carpenter's report), was carried on in entire ignorance of the fact that Pourtalès had dredged down to a depth of three hundred and fifty fathoms a twelvemonth before. In fact, it was only after their return in September, 1868, that they heard for the first time of the work done by Mr. Pourtalès in May of that and of the previous year. But a short account of it, received from Prof. A. Agassiz, was quoted by Dr. Carpenter; and reference was given to a fuller notice of Mr. Pourtalès' results in Silliman's journal for November, 1868.

It will be seen, therefore, that Dr. Carpenter, far from ignoring the researches of Mr. Pourtalès in the Corwin, fully recognized their priority to those carried on in the Lightning during the autumn of 1868. He could not well refer to a document, which, though published a year previously, had not yet come into the hands of British naturalists, and consequently could not receive from them the credit which Mr. Rathbun says has been denied it. But Mr. Pourtalès' dredgings were noticed in the same number of the proceedings of the Royal society as were those of the Lightning; and I do not well see how their value could have been more fully recognized, considering what was then known about them in this country.

I freely admit, however, that in 'The depths of the sea,' the book to which Mr. Rathbun so pointedly refers (though without naming it), it is stated that the dredgings of Mr. Pourtalès were 'commenced' in 1868. This is one of several minor inaccuracies which are unfortunately to be found scattered through the work; and, however much they are to be regretted, it must be remembered that at the time it was written the author was in bad health, with his time fully occupied by his professorial duties, and by the preparations for the cruise of the Challenger, which commenced almost before the book was in the hands of the public. In fact, the later chapters, which contain the erroneous reference to the date of

Mr. Pourtalès' first dredgings, were written under very considerable difficulties, as I well remember hearing from the author himself. But the 'priority in scientific research,' which Mr. Rathbun claims for Pourtalès' work, had been accorded to it four years previously, at the earliest possible opportunity, in the Proceedings of the Royal society. So far as I know, this honor has never been 'denied' to one who would have been the last to claim it for himself. I fully admit, however, that the date of his earlier work has been incorrectly given in certain popular accounts of the subject; but this was done accidentally, and without the slightest intention of appropriating any credit for the work of British naturalists which was justly due elsewhere, as will be evident from what I have said already.

P. HERBERT CARPENTER.

Eton college, Windsor, Eng.,
Aug. 11.

The 'bassalian fauna;' 'Pentacrinus asteriscus.'

I notice that Mr. Gill has "recently proposed the name 'bassalian realm' for the collective deep-sea faunas." I do not know whether it is proposed to define this name more strictly by assigning to it any particular bathymetrical limits; but it may be well to notice, that, in his presidential address to the biological section of the British association at Plymouth in 1877, Mr. Gwyn Jeffreys suggested the use of the name "'benthal' (from the Homeric word *βένθος*, signifying the depths of the sea) for depths of one thousand fathoms and more," while retaining the term 'abyssal' for depths down to one thousand fathoms.

There is another point to which I have long thought of directing the attention of the readers of *Science*, and I therefore take this opportunity of doing so.

The surveys of Hayden, Wheeler, and others, in Utah, Idaho, and Wyoming, have revealed the very wide distribution, in beds of Jurassic age, of a crinoid which has been called Pentacrinus asteriscus. Nothing is known of this form but a number of stem-joints (I speak under correction, and shall be pleased to hear that I am wrong); but most of the figures of these joints which I have seen (e.g., that given by White in the paleontology of Wheeler's survey) seem to me to indicate that the type should be referred to Extracrinus rather than to Pentacrinus. The essential characters of the stem-joints of Extracrinus are well shown in plate liii. of Buckland's 'Geology and mineralogy,' figs. 9–13; on tab. 101 of Quenstedt's 'Encriniden,' especially figs. 24, 27, 33, and 37; and also on plate xii. of the Austins' 'Monograph of recent and fossil crinoids.' The five interradial petals are quite narrow, and much less distinctly oval than in Pentacrinus, sometimes becoming almost linear, with rounded outer ends. The interpetaloid spaces are plain, and devoid of sculpture; while the markings at the sides of the petals are much more delicate than in Pentacrinus, having more the character of striae or crenulation than of coarse ridges. They are also much more numerous than in Pentacrinus, and are limited to the sides of the petals, not reaching the outer edge of the joint-face. Under these circumstances, I suspect that it is to Extracrinus, and not to Pentacrinus, that we must refer the joints which were described by Meek and Hayden as having lance, oval, petaloid areas, "bounded by rather narrow, slightly elevated, transversely crenulate margins."

Extracrinus was proposed by the Austins for the two well-known liassic fossils, Pentacrinus briareus and P. subangularis; but recent investigations have shown that the genus extends up into the great oolite (Bathonien) of Britain, France, and Switzerland. I have no knowledge, however, of any triassic species of Extracrinus; though Pentacrinus is well represented in the St. Cassian beds, and has been found associated with Encrinus in the 'wellenkalk' of Würtemberg.

It is therefore interesting to find that the triassic form of Pentacrinus asteriscus, which was obtained by the fortieth parallel survey from the Dun Glen limestone and the Pah Ute range, differs from the Jurassic specimens found in south-east Idaho and western Wyoming, almost precisely in those points which distinguish Pentacrinus from Extracrinus. According to Hall and Whitfield, the chief distinction of the triassic forms lies "in the more obtuse points of the star, and the filling-up of the angles between the points, and also in the broader form of the elliptical figures on the articulating surfaces of the disks." They suggest that the differences may possibly be of specific value; but, having carefully studied a large variety of stem-joints of Pentacrinidae, both recent and fossil, I am inclined to go farther, and to suspect that the triassic type may belong to Pentacrinus, but the Jurassic form to Extracrinus.

The two genera differ very considerably in the characters of the calyx and arms, as will be fully explained in the report on the Pentacrinidae dredged by the Challenger and the Blake, which will appear in the course of the winter. But, in the mean time, I shall be most grateful for any information respecting Pentacrinus asteriscus, in addition to that which has been already made public; and I need not say that I should much like to have the opportunity of making a personal examination, both of the triassic and the Jurassic specimens. P. HERBERT CARPENTER.

Eton college, Windsor, Eng.,
Aug. 11.

Points on lightning-rods.

The following passage occurs in J. E. H. Gordon's excellent "Physical treatise on electricity and magnetism," vol. i. p. 24: "It was held that the knobs [on the ends of lightning-rods] must be most efficacious, because the lightning was seen to strike them, and never struck the points. The fact that a point prevents the lightning from ever striking at all was not known."

This is not true. The highest rod on my house is some fifteen feet above the others, and about thirty feet higher than the surrounding buildings; and yet, notwithstanding the fact that it is tipped with a brush of five points, it was struck a few years ago. The points are gilded iron, and the topmost one was melted into a ball about one-eighth of an inch in diameter. The rods are all connected by horizontal pieces held about three inches from the tin roof by glass insulators, after the fashion of ignorant lightning-rod agents. The neighbors say that the sparks flew so thickly between the rods and the roof, as to resemble a sheet of flame. The shock was, singularly enough, so slight that it is doubtful whether it was due to the electrical discharge, or the deafening crash of thunder that instantly followed the splitting sound of the spark. A. B. PORTER.

Indianapolis, Aug. 23.

Photographs of the interior of a coal-mine.

One of the most interesting enterprises to which the preparations for the New Orleans exposition have