lack at present, and at the same time to develop the resources of the country. In order to make the work of such a survey as useful as possible, it is proposed to make the results known through the agency of the daily press and other publications, to be issued as rapidly as possible. It is to be hoped that the practical and wise measures proposed by the association will be carried out, as they cannot fail to benefit the people of the State.

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.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

Is the Rainfall increasing on the Plains?

IN view of the recent discussion on this subject which has appeared in this periodical, perhaps it will not be amiss to add a few remarks to those of Mr. Curtis concerning the errors which may arise in climatic studies from errors in rainfall records. Rainfall records have probably been kept as long in New England as in any other part of the United States, and a number of them give indications of a secular change in the amount of rainfall. But Mr. E. B. Weston, Desmond Fitzgerald, and others who have had occasion to study some of these records, state that in certain cases the apparent change in the amount of rainfall was clearly due to the different methods pursued by different observers in measuring snow, and converting it to its equivalent in rain; and they think but little reliance can be put in the results obtained from a comparison of earlier with more recent records. Mr. Weston has also shown that gauges with different kinds of rims give persistently different results. If these early volunteer records are of uncertain value for studying climatic changes, are those which are now being gathered by our national Signal Service to prove more so? These latter records are in almost every case obtained from rain-gauges exposed on the roofs of houses; and hence the amount of rain caught becomes a function of the wind-velocity, a function of the wind-direction, and a function of other variants and variables, not least among which is a not uncommon change in the position of the gauge itself. Numerous experimental observations have shown that gauges exposed on roofs catch more rain when exposed on the side opposite to the direction from which the wind blows, and less rain when exposed on the same side from which the wind blows. The writer thought that the large errors which may arise from this source were fully recognized by the Signal Service officers and by the scientific public, so that it was unnecessary to call attention to them. But recently he has listened to two papers by well-known writers, dealing with changes in the amount of rain, especially in the West; and both these writers referred to the valuable records now being obtained by the Signal Service as furnishing a basis for future studies of this kind. The present writer inferred from these papers that the errors arising from exposure are not so fully known as they ought to be, and hence presents a brief study of the Boston rainfall record, which is only one of several similar cases which have come under his notice. For several years in succession the annual rainfall at the Boston Signal Service station has been reported below the normal According to the Bulletin of the New England Meteorological Society, in 1885 it was nearly three inches below the normal, in 1886 nearly five inches, and in 1887 nearly thirteen inches below. This seemed rather strange, since none of the numerous gauges around Boston showed such marked deficiencies. Thus, in 1887, when the Boston Signal Service station reported the annual rainfall thirteen inches below normal, the Harvard College Observatory, only three miles west of Boston, reported an annual rainfall twelve inches greater than that reported from Boston, and one inch greater than the average of twenty years' observations at the observatory. The observer at Lynn, Mass., ten miles north of Boston, reported an annual rainfall fifteen inches greater than Boston, and six inches above the average of thirteen years' observations at Lynn. According to the records of several gauges in Milton, ten miles south of Boston, the annual rainfall was from nine to twelve inches greater than at the Boston station. These stations are all so close to Boston, that it is rendered entirely improbable that there was in reality any great deficiency in the Boston rainfall; and the apparent deficiency seems clearly due to a change in the position of the Boston gauge about 1883 or 1884. Previous to this the gauge had been exposed on the roof of the Equitable Building in Boston, and these records were used in forming a series of averages or normals. Then the gauge was removed to a high tower on the Post-Office Building, and since then there has been almost a persistent deficiency of precipitation as compared with former records, or with the records of stations surrounding Boston. Moreover, the amount of rainfall caught is evidently a function of the wind-velocity, and decreases with increased velocity of the wind. Thus, during a gale on April 2, 1887, the amount of precipitation reported from the Boston Signal Service station was 0.22 of an inch; while measurements by a number of observers in and around Boston showed that snow fell to a depth of over a foot, and when melted gave an inch of precipitation as ordinarily recorded. Again, during the storm of March 11 to 14, 1888, the Boston Signal Office reported 1.24 inches of precipitation, while surrounding stations reported three inches or more.

It seems a pity that our Signal Service gauges should be so badly exposed, for these are looked to as the standards throughout the country; and there is no doubt that in the future, as in the past, there will be attempts to prove climatic changes from their records; but the writer feels that any one who has had experience with rainfall observations will look dubiously on any conclusions based on such records as exist at present. H. HELM CLAYTON.

Blue Hill Observatory, May 2.

Significance of Sex.

SOME recent publications on the subject of the significance of sexual reproduction, especially those of Dr. Weissmann (*Nature*, xxxiv. p. 629, 1886, and xxxvi. p. 607, 1887) and a short abstract of a lecture by Hatschek (*Annals and Magazine of Natural History*, i. p. 163, 1888), have induced me to draw brief attention to some speculations of my own on this subject, published several years ago.

1. Dr. Weissmann, in his admirable paper on the significance of polar globules (*Nature*, xxxvi. p. 607, 1887), after showing that there must be some very great benefits resulting from the introduction of sexual reproduction, says, "Such beneficial results will be found in the fact that sexual propagation may be regarded as *the source of individual variability, furnishing material for natural selection.*" Now, in an article on genesis of sex, published in the *Popular Science Monthly*, December, 1879 (xvi. p. 167), and republished in the *Revue Scientifique* for Feb. 14, 1880 (xviii. p. 220), the same thought is distinctly implied, though not distinctly expressed. The whole contention of the article is to show that the object of sex is the funding of individual differences in a common offspring, thereby improving the offspring; and, further, to show how much pains nature has taken to make individual sexual differences greater and greater in the history of evolution. In the last paragraph I say, "Such mixing produces more *plastic nature*, more generalized and therefore more progressive form."

This was written nearly nine years ago. Meanwhile the thought continued to develop in my mind. In a book ('Evolution and its Relation to Religious Thought') just now published, but most of which, and especially all on this subject, was written three years ago, the same thought is much more distinctly expressed. On p. 220 I say, "Why was sex introduced at all? There are doubtless sufficient reasons of many kinds, but the *fundamental reason connected with evolution is the funding of individual differences in a common offspring, thereby giving to the offspring a tendency to divergent variation.*" Again on p. 223: "*Complexity of inheritance*, like complexity of composition in chemical substances, *gives instability to the embryo and liability to variation to the offspring; and this in its turn furnishes material for selection of the fittest.*" This was written in the fall of 1884; but, being much pressed with other work at that time, I laid aside the manuscript, and only took it up again, finished it, and sent it to the publisher, about a year ago. I do not bring this forward now by way of reclamation, — for even if I had any right to make such, which I have not, I care little who brings out a truth, — but partly because I would not seem to borrow an

idea without due credit, and partly because I am gratified that a thought which has lain long in my mind is now confirmed by so eminent a biologist and so profound a thinker as Dr. Weissmann.

2. There is one point, however, in Dr. Weissmann's paper, to which I would take some slight exception. He says, "There is *no essential, but only individual differences* between the nuclear substance of the spermatozoön and of the ovum. There are *no such things as male and female nuclear substances*, but only male and female cells, carriers of immortal germ plasm." Now, if by 'essential differences' he means mysterious or occult differences, such as are usually attributed to sex, he is probably right; but surely sexual reproduction is a device of nature whereby greater individual differences of nuclear substances are produced than could have been gotten in any other way. Such extreme individual differences are called 'sexual' in the case of organisms: why not also in the case of nuclear substances?

3. In the other paper referred to, Hatschek, after criticising the views of Weissmann and others, goes on to give his own theoretical opinion; viz., "that in sexual reproduction we must recognize a remedy against the action of *injurious variability*." He then goes on to show that disease or injurious variation of any kind in an individual would be indefinitely continued by non-sexual modes of reproduction, but in sexual reproduction is quickly eliminated by crossing with other strong and healthy individuals. Now, precisely this view is very distinctly brought out in my article on genesis of sex. Referring to the reasons for the introduction of sexual reproduction, I say (p. 177), "The reason is probably this: *Among all the qualities, good and bad, strong and weak, inherited (by the offspring) from both sides, there is a sort of struggle for life, and a survival of the best and strongest qualities.*". The same thought is expressed in many ways in my book on evolution, already referred to.

JOSEPH LECONTE.

Berkeley, Cal., April 24.

Catching Fixed Forms of Animal Life on Transparent Media for Study.

IN studying forms of animal life that become fixed to foreign bodies during their early stages of development, it is an obvious advantage to the investigator if they can be induced to attach themselves to transparent media, such as glass or mica, so that they may be studied without disturbance under the microscope with transmitted light.

Several attempts have been made to secure young oysters on glass by Prof. John A. Ryder,[1] Dr. R. Horst,[2] and Lieut. Francis C. Winslow,[3] but with only very partial success. Professor Ryder once found larvæ attached to the glass sides of an apparatus in which artificial propagation was being carried on. Dr. R. Horst tried ground and plain glass, but secured only two on the latter. Prof. Karl Möbius, in the *Zoologischer Anzeiger* of Jan. 22, 1883, describes a successful attempt to catch fixed organisms on glass. He used microscope-slides, and secured annelids, hydroids, polyps, *Bryozoa*, *Infusoria*, diatoms, etc. Prof. B. H. Van Vleck informs me that he habitually secures fixed forms of low organisms on glass microscope-slides for study.

During the summer of 1887, I was studying the development of the oyster (*O. virginiana* Lister) at Buzzard's Bay; and as I was anxious, if possible, to get them growing on glass, I tried several methods to accomplish the desired end. During my work I had very valuable assistance and suggestions from Dr. E. B. Larchar of Onset, whose disinterested aid I here acknowledge.

In a small pond-like estuary, bare at low tide, on the 14th of July, I stretched wire netting between stakes driven into the sand and raised about a foot from the sand. On this were laid panes of glass, fastened in place with clothespins. Other panes were suspended from the sides, some in the plane of motion of the incoming tide, others opposed to it. Lamp-chimneys were also suspended from the apparatus. I used some fifty panes and twelve chimneys, but met with almost total failure, securing only two oysters, one of which, however, grew to twenty-one millimetres in diameter. A few barnacles became attached to the glass.

One of the most successful spatting-grounds for oysters at Buzzard's Bay is a sand-spit exposed about four hours at low water. Here the incoming tide divides, one branch flowing to Onset, and the other to Buttermilk Bay. The force of the current is very great. On this bar, at the suggestion of Dr. Larchar, earthenware drain-pipes were partially sunk in an upright position, and loosely filled with broken glass. Six-inch and four-inch pipes were used, and at different elevations above the sand. I did not secure any spat in the four-inch pipes. A six-inch pipe sunk nearly to the level of the bar was successful, but in an unlucky day was filled up with sand, and the young oysters buried. Six-inch pipes, reaching about ten inches above the bar, were the most successful, and on the glass in them I got large numbers of young oysters. Suitable conditions for the success of the undertaking were therefore as follows: (1) a strong tide-way, which would bring plenty of free-swimming fry, and afterwards abundant food for their maintenance; (2) an area of quiet water within the pipe, in which the fry could settle and attach themselves to the smooth glass; (3) the absolute shutting-out of violent currents which would detach them from their precarious abiding-places.

I first found spat on the glass on the 25th of July, and on the 28th took out a pane 6 by $4\frac{1}{2}$ inches square, on which I have just counted eighty-two young oysters, as it is still in my possession intact. This was only exceptional in the size of the glass, as other pieces bore nearly or quite as many spat proportionally to their area. Spat were secured in the pipes until the 29th of October, when few were left, large numbers having dropped off by the natural dissolving action of sea-water upon the organic cement by which they are attached.

Besides oysters, *Anomias*, *Crepidulas*, and *Bryozoa* freely attached themselves to the glass, presenting admirable opportunities for studying them alive and undisturbed.

The nature of my researches required that I should get young oysters with shells as clean and perfectly preserved as possible. In May, year-old oysters were found very beautifully preserved on the inside whorls of a dead Busycon shell, where they were completely protected from all eroding action, and clean. To imitate these conditions, in the salt-pond above referred to, I suspended a large number of two-and-a-half and three inch flower-pots inverted, from galvanized wire stretched between stakes driven firmly into the sand. The pots were raised about six inches from the sand. They met with entire success. The pots in many instances were literally almost covered with spat. On the outside of the pots very little, and on the inside no, sediment was deposited, as the pots hung like suspended bell-jars, so that the oysters were perfectly clean and very finely preserved. Further, on account of the porosity of the earthenware, the oysters had less hold than on natural cultch of stones and shells, and were easily removed for study.

ROBERT T. JACKSON.

Cambridge, Mass., May 4.

[1] J. A. Ryder, On the Mode of Fixation of the Fry of the Oyster (Bull. U.S. Fish Com., ii. 1882); An Account of Experiments in Oyster-Culture, and Observations relating Thereto (Rep. U.S. Fish Com., 1882), Washington, 1884.

[2] R. Horst, On the Development of the Oyster, *O. edulis* L. (Rep. U.S. Fish Com., 1884), Washington, 1886.

[3] F. C. Winslow, Notes upon Oyster Experiments in 1883 (Bull. U.S. Fish Com., iv. 1884).

Answers.

31. BLONDE AND BRUNETTE. — A week or two ago a correspondent of yours complained of the lack of precision of meaning attached to the terms 'blonde' and 'brunette,' and the want also of words treating of intermediate shades of color as applied to the hair and complexion of Caucasian human beings. There is certainly a need of more convention on this point; for it seems strange that lower animals, and even inanimate objects (*vide* dry-goods, etc.), should be minutely characterized as regards color, whilst their lords or makers are not. It would seem as if a century and a half ago there was such better understanding about terms of color as this gentleman wishes to see established. In the *Spectator* one meets with proof of this. In one place I remember a lady (assumed) writer draws the distinction between herself and a friend in the matter of complexion as between an olive and a brunette; and the term 'a handsome black man' (to imply the latter hue in a man) is also met with in the writings of the last century.

ALFRED J. HILL.

St. Paul, Minn., May 3.