est and most permanent usefulness, only when it fulfils this intention as far as possible.

Whether the words we have quoted, and others of a similar tenor, mark a change of opinion on the part of the director of the New-York station, or are only a clearer expression of convictions previously held, we do not undertake to say. In either case, we are glad to see the weight of this important institution cast in favor of the scientific conception of an experiment-station. The great need of agriculture to-day is not new varieties of plants, or improved breeds of animals; new methods of cultivating the soil, or improved systems of farming. All these, and many other like things, are good; but the two great wants are a better knowledge of principles, and greater intelligence to apply them. For the latter we must look to our agricultural schools: the former we should require from our experiment-stations.

We do not hold that an experiment-station should never undertake to originate or test new varieties of plants and animals or new agricultural methods, — often work of this general character will be demanded of it by the public, and will prove of great public utility, — but, in our view, it should not be allowed to be, or to appear to be, the chief end of the station. The two kinds of work are both important, but we question the advisability of attempting to unite them in one institution and under one management. Each requires facilities and talents peculiar to itself; and it seems doubtful, whether, as a rule, one institution will be able to provide good facilities for both kinds of experimentation, and still more doubtful whether it can find combined in one person the diverse knowledge and training required for their successful prosecution. With the growth of agricultural experimentation there might profitably be, we suspect, in the majority of cases, a subdivision of it into two overlapping yet independent classes. We should have, first, the experiment-station proper, aiming chiefly at a further elucidation of the laws and principles underlying agriculture; and, second, the experimental farm, devoted mainly to carrying out upon a farming scale the principles worked out by the experiment-station.

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.*

Psychical research.

YOUR issue of Oct. 17 contained two articles which are of good omen for the future of 'psychical research' in America. Of the first, the editorial article, I need say little. It is cordially welcomed by my colleagues and myself for its recognition of the far-reaching importance of an enterprise in the further development of which our society will, we hope, go hand in hand with yours. With the second article, on 'psychic force,' our agreement is less complete; but we still find nothing to complain of in the general attitude of the distinguished writer. He, too, recognizes the legitimacy of the inquiry, while clearly apprehending its difficulties. He describes with entire justice the two opposed classes between which psychical research has to clear a path, — the party of easy credulity, and the party of easy incredulity; and he points out with no more than proper emphasis the rigorous caution which every forward step demands. Fraud and superstition have naturally seized on what science has so systematically neglected; and those who now endeavor to take the subject up from the scientific side must accept the fact and its consequences.

So far, then, we are wholly at one with Professor Newcomb; but we cannot quite so readily follow him in his criticisms of our own doings. He begins by condemning one of our public appeals for information; but his strictures seem to assume that all the information which the appeal brings in will be regarded by us as a safe basis for conclusions. The appeal is, of course, merely a first step, for which it would be difficult to imagine any effective substitute; though I may mention that a very large amount of our information comes to us through private channels. The sifting and treatment of the evidence according to scientific canons must be a subsequent labor, the *rationale* of which could not be set forth, or even suggested, in the terms of a short advertisement. And of this labor no portion is more important than the one which we are glad to find Professor Newcomb so explicitly recognizing, — the application of the doctrine of chances. In all those branches of our inquiry where questions of *coincidence* occur, it is clearly essential to ascertain, as definitely as may be, how far the coincidences may fairly be ascribed to *chance*. We have taken, and are still taking, great pains to obtain this definite information. Very wide inquiries have been made; and the results, though far from complete, may still, I think, claim decidedly more validity, as a basis of computation, than Professor Newcomb's guess at what "any physician will consider quite within the bounds of probability." It would require more space than I can ask for, to comment on Professor Newcomb's numerical argument in detail. But I may remark that he seems to confuse the argument by classing all together what he calls 'dreams, illusions, visions,' etc.; at least, if he means to include in this heterogeneous group visual hallucinations of waking persons, which we regard as by far the most important phenomena from an evidential point of view. If any one, in his waking moments, experiences apparitions of human forms as often as once a week, which is the degree of frequency that Professor Newcomb's calculation assumes, it is obvious that the approximate coincidence of one of these apparitions with the death of the corresponding human being will be an insignificant accident. But we have not ourselves met with any specimen of this class. We have collected more than a hundred first-hand cases of apparitions closely coinciding with the time of death of the person seen; and it is only in a small minority of such cases that our informants, according to their own account, have had any other hallucination than the apparition in question.

The following sketch may serve to show the lines on which our own reasoning in the matter will proceed. We are making a census, which, so far, shows that in this country the proportion of sane persons, in good health and awake, who within the last ten years have had a visual hallucination representing some living person known to them, is about one in three hundred. Now, let us make a supposition far below the actual mark, and confine the number of the acquaintances of each of these hallucinated persons to five. Let us further suppose that one of these five persons does actually die in the course of the ten years. This seems fair, on the whole; for, though in some cases more than one may die within that time, in others none may die. According to this estimate, then, the chance that the death will take place within twelve hours of the apparition will be one in $365 \times 2 \times 10 \times 5$; that is, one in 36,500: in other words, only one out of every 36,500 of the hallucinated persons will, in the course of ten years, hit off the coincidence by chance. But since the hallucinated persons are only a three-hundredth of the whole population, this means that the proportion of the whole population who will by chance have an apparition of a person known to them within twelve hours of that person's death is only one in 10,950,000. Now, we ourselves have a large collection of such recent cases, resting on good first-hand testimony; but let us put the number far below the mark, and say thirty cases. If, then, these thirty coincidences are to be fairly attributed to chance, the population of the country will have to be 328,500,000. But we cannot suppose that our appeal for evidence has reached the whole population; and we shall be making a sober estimate, if we reckon that within the given time ten times as many cases must have occurred as those we happen to have encountered. This brings the necessary population up to 3,285,000,000; and the number will be further immensely increased if we take count of the fact that many of the coincidences are extremely close, that the times of the two events fall not only within twelve hours, but within one. Thus the theory that chance would account for the cases could only be justified if the population of the country were several hundred times what it actually is. The *reductio ad absurdum* seems tolerably complete.

The case of dreams is of course very different. We are most of us constantly dreaming. A very large number of 'odd coincidences' between dreams and external events is certain to occur by mere chance, and the cases are rare where the correspondence is of a kind which strongly suggests telepathic influences. Here, therefore, Professor Newcomb's estimate is far more applicable; and we have always felt that dreams, by themselves, could not be expected to afford conclusive proof of telepathy. This, however, does not seem a sufficient reason for ignoring them; since, if the fact of telepathic communication be otherwise established, they may throw light which we could ill afford to neglect, on the nature of the mental and cerebral processes involved.

As regards 'haunted houses,' we readily admit, and have expressly pointed out, the far greater uncertainty of the evidence as compared with the best telepathic cases. But even here we differ from Professor Newcomb in seeing a distinction between the experiences which we deem of some *prima facie* importance, and the experience which he supposes when a person, lying awake an hour after midnight, hears some sound the cause of which is beyond his power to guess. Sounds are the very weakest sort of evidence. What strength the *prima facie* case has, depends, not on things heard, but on things seen; and seen, not by one person only, but by several independently and at different times, and, as the seers affirm, without any knowledge, on their part, that the house was supposed to be 'haunted.'

Professor Newcomb's concluding remarks, dealing with the *experimental* side of telepathy, deserve careful attention. But his objections here rest entirely on the hypothesis of visual and auditory indications consciously or unconsciously given by the 'agent' to the 'percipient;' and though it is difficult, I know, to convince persons who have not been present that sufficient precautions have been taken to eliminate this source of error, it must surely be admitted that such precautions are possible. As regards sight, no one will deny the possibility; and, as regards hearing, we think, that, if a careful watch is kept, the means of communication resolve themselves into slight variations of breathing. Such variations were never detected in our experiments, and in any case could hardly be supposed capable of rapidly conveying to the percipient's mind the form of an irregular diagram; and the difficulty would be increased in cases where the signs would have had to be *unconscious*, as in many of our experiments where we were able not only to vary the 'agent,' but to act ourselves as 'agents.' As for 'indications whether the subject is going right or wrong,' they must, of course, be prevented by taking care that the 'agent' shall not watch what the 'percipient' is doing. Most of the spurious 'thought-reading' of the 'willing-game' would be prevented, if the 'willer,' instead of the 'willed,' were effectively blindfolded.

But we find ourselves once more wholly in sympathy with Professor Newcomb, when he insists that the experiments must be repeated again and again, under the strictest conditions, before we can reasonably expect thought-transference to be accepted as an established scientific fact. So far from resenting the demand for more evidence, we are ourselves unceasingly reiterating it. The responsibility for such novel observations cannot be too widely spread, and glad indeed shall we be to shift some of it to American shoulders.

EDMUND GURNEY,
Hon. sec. of the Society for psychical research.
14 Dean's yard, Westminster, S.W.,
Nov. 4.

Mr. Gurney's letter suggests many interesting reflections on the probabilities involved in questions of telepathic phenomena, and I hope for an early opportunity to engage in a further discussion of the subject in the columns of *Science*. This will naturally involve the consideration of the points raised in his letter. Meanwhile there are two numerical data;

and, if he would favor me with them, I should feel much flattered, — firstly, his estimate, from the census results, of the number of persons of the age of fifteen and upwards, resident in the British Islands, whose statements he would consider *prima facie* entitled to full credence (to guide him I may remark that I see no reason why the number should not be from ten to twenty millions); secondly, his estimate of the probability that one of these persons, taken at random, would not be above amusing himself or herself at the expense of a society so eminent as that of which Mr. Gurney is the honorary and honored secretary. These numbers will come into my discussion, and I should much rather have them from an authority conversant with the subject than attempt to guess at them myself.

SIMON NEWCOMB.

Change in the color of the eye.

The experience of Mr. T. F. McCurdy (p. 452) is, I imagine, a not uncommon one. It certainly finds illustration in my own family and in myself; the iris, which was quite black in childhood, having for many years visibly lightened, until it is more correctly described as gray, with shades of hazel. The fading-out of black eyes with age is a matter of common observation; and the change, judging from the facts within my own knowledge, is more apt to occur where the individual takes after a grandparent who had the dark eye, and where the immediate parents had blue or gray.

C. V. RILEY.

Washington, D.C.

Specimens illustrating Lehmann's 'Origin of the crystalline schists.'

It may not be uninteresting to the geological readers of *Science* to know that the writer has recently received, through the kindness of Professor Johannes Lehmann of the University of Breslau, a very valuable suite of rock specimens illustrative of the latter's new and important work on the origin of the crystalline schists, noticed in *Science*, No. 86, p. 327, and in the *American journal of science* for November, 1884, p. 392. These specimens are sixty-three in number, and were collected, partly by Professor Lehmann himself, and partly under his immediate supervision, in the granulite area of Saxony, and in those parts of Bavaria which he has made the subject of his special study. They exhibit in an excellent manner nearly all those phenomena ascribed by the author of the above memoir to metamorphism by pressure, especially, however, the changes which certain massive pyroxene rocks of Saxony have undergone in becoming hornblendic schists exactly analogous to similar alterations traced by the present writer in the rocks near Baltimore.

To all students of metamorphism and of structural geology in highly crystalline regions, this work must be of absorbing interest as undoubtedly the most advanced of its kind; and, in spite of its superb atlas of most satisfactory photographic illustrations, its readers may be glad to know that this suite of original specimens is in the petrographical laboratory of the Johns Hopkins university, where it will always be accessible to such persons as may be interested in examining it.

GEO. H. WILLIAMS.

Baltimore, Nov. 25.

Bot-flies in a turtle.

Some days ago Prof. T. Robinson of Howard university called my attention to a box-turtle (Cistudo carolina) which had in the muscles on either side of the neck about thirteen large bot-fly larvae. The turtle was alive, but evidently suffered inconvenience from the intruders which had taken up their abode at a point from which they could not be dislodged by claw or beak. They were removed with forceps, and sent to Mr. Howard of the agricultural bureau, who informed me that they belonged to the family Oestridae, and to a genus probably undescribed. He also brought to my attention an exactly parallel case reported in the *American naturalist* (xvi. 598) about two years ago by Prof. A. S. Packard.

FREDERICK W. TRUE.

U. S. national museum, Washington, Nov. 24.

On the function of the serrated appendages of the throat of Amia.

Through the kindness of Prof. B. G. Wilder I have at present two living specimens of Amia which I propose to employ shortly in a comparative study of the brains of American ganoids.

My attention was first attracted to the serrated appendages of the throat by Professor Wilder's own note upon the subject, published in the *Proc. Amer. assoc.*, 1876, and more recently by a reference to the same structures in one of Sagemehl's admirable contributions to the anatomy of fishes (*Morph. jahrb.*, x. 63). Sagemehl concludes, from the examination of alcoholic specimens, that these 'flagella' are, during life, in constant motion, and thus help to renew the water in the gill-cavity. Such is by no means the case. The 'flagella' are attached by their bases to the lateral aspects of the sterno-hyoid muscles (hyopectorales of McMurrich), the chief function of which is to enlarge the cavity of the mouth. When these muscles are at rest, the flagella lie flat along their surfaces: when they contract, the cavity of the mouth is enlarged, the flagella erected, and the gill-covers pushed outwards. At the suggestion of my assistant, Mr. A. B. Macallum, we stimulated the proximal part of the muscle with the result of a perfect demonstration of the above facts. The flagella thus help to replace functionally the absent dilatator muscles of the gill-covers. A strip of condensed tissue occupies a precisely similar position on the hyopectoral muscle of Amiurus, perhaps a rudiment of similar organs possessed by the ancestors of the siluroids before the differentiation of the dilatator muscles of the operculum.

My specimens of Amia, after being in captivity for some time, became very sluggish, and hardly any movements of respiration could be detected. After the fish had been removed for a little out of the water, however, and then returned to it, the movements were sufficiently active to disclose the following facts: —

During the enlargement and filling of the cavity of the mouth, the posterior flexible (and muscular) border of the gill-cover is tightly applied to the soft parts behind the gill-opening. When the mouth-cavity is quite full, the mouth closes, the muscular border of the gill-cover releases its sucker-like hold of the soft parts, and the water is driven out by the contraction of the walls of the mouth-cavity.

Professor Wilder's account of the structure of the serrated appendages is so complete as to render any further reference to this subject unnecessary.

R. RAMSAY WRIGHT.

University college, Toronto, Nov. 27.