

hæmatoscope being applied to the nail, which is exposed to the usual daylight (as strong as possible, but that from a house-window is enough), the energy of the exchange going on between oxygen and the tissues can be seen. This new idea is of great practical importance in the study of the phenomena of nutrition, both in physiological and in pathological states; so that such physicians as Professor Germain Sée are now taking the matter up and applying it to the study of many pathological states, such as anæmia, etc. Dr. Hénocque is one of Professor Brown-Séquard's best men. He has given the results of some three hundred and seventy cases in which experiments were made.

#### BOOK-REVIEWS.

*Proceedings of the American Society for Psychical Research.*  
Vol. i. No. 3, 1887.

THE appearance of Miss Fletcher's paper upon 'The Supernatural among the Omaha Tribe of Indians,' in the Proceedings of the Psychic Research Society, is of importance, because it shows that this society is in part ready to take the anthropological view of such notions, to find their interest in the recording of such popular beliefs as a contribution to the statistics of human thought with no more reference to their possible objective verification than is necessary to shed light upon their origin. Apart from this, Miss Fletcher's paper is extremely interesting as showing the naturalness with which the supernatural enters into the every-day life of unenlightened people. It is also noteworthy that the Omaha ghost lets himself be heard so much more than seen, while with us the reverse is the case. This fact is very suggestive, and several aids to an explanation present themselves. It is also worth mentioning how little the evolution of terror is associated with the 'ghost-noises' of the Omahas.

All those who have followed the eventful career of the 'Phantasms of the Living'—the depository of the work of the English Psychic Research Society—will read with interest the controversy between Mr. C. S. Peirce, the well-known mathematician and logician, and Mr. Edmund Gurney. The former makes a detailed enumeration of all such cases regarded by Mr. Gurney and his associates as a proof of spontaneous telepathy, and shows that a large proportion of these suffer from serious omissions and fallacies, mainly sinning against the principles of the logic of induction. This brings a lengthy reply from Mr. Gurney, and a still longer rejoinder from Mr. Peirce. The discussion turns upon details, and must be read in full. Two points may be briefly noticed. The first relates to the estimation of the probability of a certain thought occurring to our minds within a given period. This is always a delicate task; and, as so much of our mental activity goes on in the region of the unconscious, it seems safer to make a very liberal estimate in this regard; and, if we do this, a larger number of coincidences of such presentiments as the death of a friend (as prompted by an undefined feeling about his welfare) with the actual occurrence will be attributable to chance. It is through the neglect of this consideration that the evidential value of many of the best cases is decidedly weakened. Next, as Mr. Peirce well argues, if we admit that the cases as they stand defy explanation by ordinary reasoning, it is very easy to invent half a dozen hypotheses explaining the facts as well as does the telepathic theory, and in the minds of many people by no means as improbable as the latter.

The reports of the several committees are more than usually satisfactory. The report of the committee on thought-transference, apart from an injudicious closing paragraph, is a frank confession of negative results. The committee on experimental psychology, of which Dr. C. S. Minot is the chairman, give the results of their inquiries as to the prevalence of a feeling sufficiently strong to influence action with reference (1) to sitting down thirteen at table, (2) to beginning a voyage on Friday, (3) to seeing the new moon over your left shoulder. The results are, that both in men and in women the most prevalent superstition is (3); the least prevalent is (1); and that about one man in ten, and two women in ten, acknowledge a belief in these superstitions. Furthermore, the question, whether in choosing between two otherwise equally desirable houses you would be influenced by the reputation of the one as haunted, is answered in the affirmative by forty-four men and sixty-

six women in one hundred; but it should be added that a large number place this choice on accessory grounds, and not on the hauntedness of the house. Whether these statistics will be taken as marking the prevalence of frankness or of real superstition, must be left for each to decide.

The reports on haunted houses and on mediumistic phenomena presents few points of interest. The opposite is true of Mr. Cory's admirable observations on hypnotic phenomena. Only a single observation of the many ingenious tests devised by Mr. Cory can here be given. The fact that some hypnotic subjects can associate a suggested hallucination with a blank card, is explained by supposing that some trifling irregularity on the card serves to their hypersensitive senses as the direct excitant of the hallucination. This Mr. Cory supports, and really proves. A pencil with one end slightly nicked is placed on end on a mantel, and the subject is given the suggestion that nothing is upon the mantel. Then eleven other precisely similar pencils are placed on the mantel, when the subject is asked to count them, and counts eleven. A strip of board is so held as to cover the nick on the one pencil, and under this condition the subject counts twelve, showing that the sight of the nick sets the mind so as not to count that pencil.

This valuable number of the Proceedings is concluded with two notes from the pen of Prof. William James. In the first, Professor James gives the results of experiments upon the 're-action time' in the hypnotic state; showing that it is at times longer, and at times shorter, than in the normal state, and that a more detailed analysis of the kind of hypnosis is necessary to explain these results. The other brings together a number of important facts concerning the 'consciousness of lost limbs.'

#### 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.

#### Diamonds in Meteorites.

ON Sept. 4, 1886, a meteoric stone weighing about four pounds fell at Novy Urej, Krasnoslobodsk, in the Government of Penza, Siberia. In this MM. Latchinoff and Jorefeif found what they supposed to be diamonds of microscopic size. In an insoluble residue small corpuscles, showing traces of polarization, were harder than corundum, and having the density and other characteristics of the diamond, and were present to the amount of one per cent of the whole mass (see *Nature*, Dec. 1, 1887). Through the courtesy of his Excellency Julien V. Siemaschko of St. Petersburg, I have been able to procure a small piece of the meteorite. Mr. H. Hensoldt, section-cutter at the School of Mines, very kindly prepared sections of the same, which I found to contain metallic iron in small thin plates, magnetite in small opaque grains, a plagioclase felspar, and olivine in oval grains, but was unable to detect any of these bodies in the sections. Prof. H. Carvill Lewis, to whom I sent the material, informed me that he had extracted two small oval bodies, almost isotropic, and showing no more traces of polarization than occur in many diamonds. With some other fragments of the meteorite, and not with these, he made two good scratches on a polished sapphire. He did not mount the crystals, because they were again lost: so I could not examine them. He was, however, inclined to support the views of the describers.

I found, that, by grinding with a sapphire four particles of the meteorite, I distinctly made a number of minute but deep scratches on each polished face of four different sapphires with each piece of meteorite. These scratches are characteristic of but one mineral that we know, and that is the diamond; but they are evidently so minute, that they form a coating or an aggregate over the other minerals, and were too small to distinguish, but yet exist in quantity, and may also possibly be the amorphous form of the diamond known as carbon or carbonado(?). Small pieces of the meteorite were then boiled for some time in hydrochloric, sulphuric, and nitro-muriatic acids. This readily removed all of the iron and magnetite, leaving only the skeletons of olivine, on which were small black particles, one of which was elongated but rounded, suggesting two joined cubes(?). On crushing one of these olivine pieces

with black crystals attached, and grinding it with a polished sapphire, it readily scratched the same. If a larger quantity of material comes to hand, the writer will have polished a diamond with the powder of the meteorite, using a new wheel for the purpose. The writer has not seen the paper of MM. Latchinoff and Jorefeif, but there seems to be every reason to substantiate their conclusions.

These facts are of especial interest, since on Jan. 15, 1887, Prof. L. Fletcher, curator of the Mineralogical Department of the British Museum, read before the Mineralogical Society of England a paper on a meteorite which was found in the sub-district of Youndegin, Australia, in 1884, and in which he stated he had found a new form of graphite of cubic form, with the hardness of 2.5 and a specific gravity of 2.12. To this he gave the name of 'cliftonite,' calling attention, also, to the fact that Haidinger, in 1846, had found what he described as graphite pseudomorph after iron pyrites (*Poggendorf Annalen*, 1846, lxvii. p. 437), obtained by him from a nodule of graphite which had dropped out of the Arva meteorite. Gustav Rose (*Beschreibung und Enttheilung der Meteoriten*, 1864, p. 40; *Poggendorf Annalen*, 1873) expressed an opinion that this mode of replacement of the cube edges on these crystals was suggestive of holo-symmetry rather than hemi-symmetry, and that this interpretation would exclude iron pyrites as a possible antecedent mineral.

The cliftonite was readily examined with a  $\frac{1}{4}$ -inch objective; and from its structure Professor Fletcher concluded, that, while it is different from native graphite, the sharpness, separateness, and completeness of the crystal, the brightness of the faces, the delicacy of the acicular projections, and especially of the obtuse, almost flat, square pyramids, or some of the faces, are quite sufficient to prove that the form has never had any other than its present tenants; in other words, that it is not a pseudomorph. When in cubes, the diamond has faces not very unlike those of the Youndegin crystals, and shows a similar bevelling of its edges by the rounded tetrahedra. Again: Professor Fletcher says it might be argued, that, during a hurried crystallization of the carbon, circumstances initially favorable to the formation of the diamond had finally permitted the existence of carbon in a graphitic form only. He had also found distinct graphitic crystals, cube octahedrous in form, in the Cocke and Sevier County (Tenn.) meteorites.

When we consider that only a few meteorites have been examined for this mineral, we have reason to expect some interesting results in the future.

GEORGE F. KUNZ.

New York, March 6.

#### A Pseudo-Meteorite.

THROUGH the kindness of Dr. DeWitt Webb of St. Augustine, Fla., I have been able to examine a portion of the so-called 'meteoric stone,' weighing over two hundred pounds, which was said to have been seen to fall in an old cultivated field near Middleburgh, Clay County, Fla., and which was exhibited at the Subtropical Exposition at Jacksonville, Fla. It is a concretionary limonite, and not of meteoric origin.

GEORGE F. KUNZ.

New York, March 6.

#### Monocular vs. Binocular Vision.

As a constant student of binocular phenomena, I have been much interested in Mr. Hyslop's letter in *Science* of Feb. 10. I have repeated the experiment illustrated by his Fig. 1, and confirmed his results. But I do not think they are to be explained by any supposed struggle between monocular and binocular vision, but in a far more obvious way, which, in fact, he himself suggests.

In binocular combination of such simple figures as circles, where the means of estimating distance is reduced to ocular convergence alone, the estimate is very imperfect and uncertain. Our knowledge so interferes with our visual judgment that we are apt to over-estimate the distance. In fact, many persons even find a difficulty in seeing the combined binocular image any nearer than the two monocular images. As long as attention is fixed on the combined circle, the homogeneous image of the needle will seem beyond, as it ought. This will be much more distinct if we range the point of sight back and forth, combining successively the needle-points and the circles. But when we transfer attention wholly to the double images of the needle, these latter will sometimes appear nearer

than the circle; not, however, because the needle seems nearer than before, but because the circle drops to the plane of the paper, where it tends to go, anyhow.

The experiment illustrated by his second figure I cannot confirm. It is true that experiment with his figures as drawn in *Science* confirms his results, but this is only because the figures are badly drawn. The positions of the two small circles *b* and *c* are not symmetrical. When accurately drawn, I find, on combining, that the small circle and the large circle appear exactly on the same plane. My son, aged eighteen, and well practised in binocular experiments, confirms my results perfectly. Whether Mr. Hyslop's original figures were imperfect, or have been only badly copied, I know not; but the wonderful distinctness with which binocular combination will bring out and exaggerate the smallest differences in apparently similar figures, is well known.

JOSEPH LECONTE.

Berkeley, Cal., Feb. 22.

#### The Scientific Swindler Again.

THE following from the *Indianapolis Journal* of Feb. 24 may be of interest to those who have been the victims of the swindler so extensively advertised by your own and other journals: "The book-thief who has, under the names of W. R. Taggart, Professor Cameron, Professor Douglass, and various aliases, travelled over the country, representing himself as a scientific student, and borrowing valuable books, has been arrested in Cincinnati, where he gave the name of Otto Syrski. He was recognized yesterday by Professor Collett of this city, who was one of his victims. Professor Collett learned where his books had been sold, and will probably recover them." It is to be hoped that this will stop his operations, at least for a time.

A. W. BUTLER.

Brookville, Ind., March 1.

#### A Critique of Psycho-Physic Methods.

DR. JOSEPH JASTROW, in the second number of the *Journal of Psychology*, discusses the principal psycho-physic methods now in use, and advocates a thorough reform of the science of psychophysics. One of the principal conclusions at which he arrives is that no such thing as a differential threshold exists; that is to say, that there is no definite point at which the difference of two sensations ceases to be perceptible. Dr. Jastrow's arguments fail to convince us. He says, "The threshold is described as a point not exactly constant, but nearly so: above it all differences can be felt, below it all differences vanish into unconsciousness. No matter whether little or much below this point, they are utterly lost. It is idle to say, as Fechner at times does, that they differ in the amount of additional stimulation necessary to bring them up into consciousness, unless you mean that the series below the so-called threshold is an exact continuation of the series above it; and, if you do mean this, then the threshold loses all its distinguishing peculiarities, and ceases to exist." Further on, in discussing the theory of the right and wrong cases, he says, "It has been proved that the ratio of wrong answers increases as the difference between the stimuli decreases; but the 'threshold theory' claims that this last fails to hold after this difference has been diminished below a certain ratio."

In considering these objections, I may be allowed to treat two classes of sensations separately: first, the judgment that a difference exists is based on a sudden change in the character of the sensation either in space or time; second, the judgment refers to sensations separate in space or time or in both. As an example of the former, we may assume two adjoining fields of various colors or various intensities of light, or a sound suddenly increasing in intensity or height. The threshold theory says there is a certain difference between these adjoining sensations below which no difference will be perceived. Practically this is admitted by Jastrow. In trying to meet such an argument, he first says that there exists only an average threshold; i.e., the average smallest perceptible proportion of intensity or wave-length of the two sensations on which the observer is able to form a judgment. He continues, "Here you either (1) tacitly assume that not many observations are to be taken, or that (2) no matter how many observations were made, no mistake would ever occur."

The arguments of the advocates of the threshold theory are