of the audience can follow him. Let him always remember that there are some statements which the mind cannot readily receive through the portal of the ear; and there are but few which cannot be simultaneously presented, both to the eye and the ear. The diagram, the printed formula, the abstract, may cost the speaker a little expenditure; but it will save the hearer a vexatious outlay of time and attention. *Third*, Let there be a liberal margin allowed for social intercourse outside of the meetings, not merely for public receptions and excursions, but for those informal introductions and interviews which to many persons are the best part of scientific gatherings. We should not then hear it said so often, "This would have been a very pleasant meeting were it not for the papers which were read."

A REMARK made in one of the papers read before the recent Woman's congress in Baltimore suggests an interesting argument in favor of the kindergarten. It is well known, that, in its development, each new-born being passes through very much the same stages that his ancestors have been through before him. Even after birth, the growth of the child's intelligence simulates the progress of the human race from the savage condition to that of civilization. It has been shown by Preyer, and others who have studied infant-development, that a faculty which has been acquired by the race at a late stage is late in making its appearance in the child. Now, reading and writing are arts of comparatively recent achievement. Savage man could reap and sow and weave, and build houses, long before he could communicate his thoughts to a person at a distance by means of written speech. There is, then, reason to believe that a child's general intelligence would be best trained by making him skilful in many kinds of manual labor before beginning to torture him with letters; and the moral to be derived is, that primary instruction should be instruction in manual dexterity, and that reading and writing could be learned with pleasure and with ease by a child who had been fitted for taking them up by the right kind of preparation. The argument is a novel one, and it certainly seems plausible.

LETTERS TO THE EDITOR.

Change in the color of the eye.

IN *Science*, p. 367, you say the color of the iris, 'after early childhood' 'does not vary with age:' I think I can give you positive evidence that it does. My own eyes were called black (in reality dark brown) until after I was forty years old. About that time they commenced to change, and are now blue-gray, with streaks of light hazel, which last are fast fading out. The same thing happened with my father's eyes. I remember him at forty years and under, with thoroughly black eyes, and there are portraits of him which show him thus; but between forty and fifty, his eyes changed, and eventually became a blue, with a very slight tint of hazel, not noticeable without close observation. THEODORE F. MCCURDY.

Norwich Town, Conn.

The eggs of Ornithorhynchus.

The editorial comments in a recent number of *Science* (p. 412), on the revival of forgotten statements, lead me to believe that some more old matter may be revived with profit. The telegram sent to the meeting of the British association from Professor Liversedge, announcing the fact ascertained by Mr. W. H. Caldwell (*Science*, iv. 261), that Ornithorhynchus lays eggs, has been universally hailed as an entirely new discovery; and a number of the prominent British zoölogists, whom we had the pleasure of welcoming to Washington recently, were unaware that the oviparity of the monotreme had long before been definitely announced, and an egg figured. Nevertheless, such is the fact; and an extensive series of old comments and applications of the fact appears in the literature of zoölogy. I need only refer to some of the most prominent, and others can follow up the subject in the publications of the day.

In 1829 Geoffroy Saint-Hilaire published a memoir in the *Annales des sciences naturelles* (xviii. 157–164), in which he reproduced a *figure of an egg* of the natural size (pl. 3, fig. 4). This was communicated to him by Prof. Robert E. Grant of London, who drew one of a nest of four obtained by a Mr. Holmes. Two of these eggs were reported to have been obtained by the 'Muséum de Manchester;' and it would be well for our Manchester friends to hunt them up, and see whether they are still to be found. As a result of a general belief in the oviparity of the animal, several of the naturalists of the day revised the classification of the vertebrates.

In 1830 Dr. Joh. Wagler, in his 'Naturliches system der amphibien,' proposed a peculiar class (Gryphi—Greife), in which, however, by illegitimate assumptions, he included the ichthyosaurians, plesiosaurians, and pterodactyles.

In 1831 Charles L. Bonaparte, prince of Musignano, in his 'Saggio di una distribuzione metodica degli animali vertebrati,' also isolated the monotremes as a peculiar class (Monotrema), defining it in the following terms: "I Monotremi sono animali vertebrati, a sangue calido, *ovipari*, quadrupedi; respirano per mezzo di polmoni; hanno un cuore biloculare biaurito."

And even long before the egg was thus figured,

and, it may be said, the oviparity of the monotremes firmly established, the fact had been authoritatively proclaimed. Sir John Jamison, for instance, especially declared that 'the female is oviparous, and lives in burrows in the ground' (*Trans. Linn. soc. London*, xii. p. 585). The Rev. Dr. Fleming, in his 'Philosophy of zoölogy' (ii. 215), published in 1822, remarked, that, "if these animals are oviparous (and we can scarcely entertain a doubt on the subject, as *the eggs have been transmitted to London*), it would be interesting to know the manner of incubation." Further, Fleming refused to admit the monotremes among the mammals, dividing the Vertebrata 'with warm blood' into 'quadrupeds' and 'birds,' and the former into 'I. Mammalia' ('1. Placentaria' pedota and apoda, and '2. Marsupialia'), and 'II. Monotremata.'

But, notwithstanding all these facts, scepticism as to the truth of the representations and authenticity of the eggs, developed into positive disbelief; and Bonaparte himself recanted, and took that decidedly retrograde course, which others had entered upon, of associating the monotremes with the marsupials in the unnatural and artificial negative group of Ovovivipara, or Implacentalia. I, too, was so far influenced by the prevalent scepticism or disbelief, and by the similiarity of the monotreme egg to that of a reptile, that I retained viviparity as a special attribute of the mammals in 1872, although I declined, on other evidence, to include a small size for the eggs in my diagnosis of the class. I then, also, adopting the subclasses Monodelphia, Didelphia, and Ornithodelphia, segregated them into the major groups, combining the first two under the name Eutheria, and contrasting the last as the Prototheria. These names have since been accepted by Professors Huxley, Flower, and others; and, inasmuch as Professor Huxley did not accredit their origin, they have been ascribed to him. I must add, however, that Professor Huxley has restricted the name Eutheria, although apparently with a hypothetical qualification, to the monodelphs, while he has coined a new name (Metatheria) for the marsupials. I fail to appreciate the need for such modifications, which virtually become exact synonymes of Monodelphia or Placentalia, and Didelphia.

Finally, the old data as to the oviparity of monotremes became almost lost to memory, so that no one has recalled them since the rediscovery. In view of such forgetfulness and scepticism, therefore, further information was necessary to insure the admission of the old evidence as valid. But Mr. Caldwell has further added the intelligence, quite new, that the eggs of Ornithorhynchus are meroblastic. This discovery will have an important bearing on the question of the origin of the mammals, and is antagonistic to the suggestion of Professor Huxley that the type was a direct derivative from the amphibians, while it increases the possibility that Professor Cope may be nearer the truth in affiliating the ancestors of the mammals to the theriomorphous reptiles of the Permian.

THEO. GILL.

Sun-spots.

The long-delayed maximum of solar spots, now undoubtedly passed, has attracted unusual attention to the spot-periodicity. To-day and yesterday the visible hemisphere of the sun was, for the first time in nearly fourteen months, observed to be entirely free from spots; the occasion next preceding this being 1883, Sept. 25. During the past two years, the only additional days on which the sun was observed to be without spots were, in 1882, Oct. 9 and Dec. 3, and, in 1883, Feb. 23, and May 25, 26, 27, and 28.

DAVID P. TODD.

Lawrence observatory, Amherst, Mass., Nov. 8.

The numerical measure of the success of predictions.

Suppose we have a method by which questions of a certain kind, presenting two alternatives, can in every case be answered, though not always rightly. Suppose, further, that a large number of such answers have been tabulated in comparison with the events, so that we have given the following four numbers: —

(aa), the number of questions for which the answers were the first way and the events the first way;

(ab), the number of questions for which the answers were the first way and the events the second way;

(ba), the number of questions for which the answers were the second way and the events the first way;

(bb), the number of questions for which the answers were the second way and the events the second way.

Then the problem is, from these data to assign a numerical measure to the success or science of the method by which the answers have been produced. Mr. G. K. Gilbert (*Amer. meteorological journal*, September, 1884) has recently proposed a formula for this purpose; and I desire to offer another.

I make use of two principles. The first is, that any two methods are to be regarded as equal approximations to complete knowledge, which, in the long-run, would give the same values for (aa), (ab), (ba), and (bb). The second principle is, that if the answers had been obtained by selecting a determinate proportion of the questions by chance, to be answered by an infallible witness, while the rest were answered by an utterly ignorant person at random (using *yes* and *no* with determinate relative frequencies), then the approximation to knowledge in the answers so obtained would be measured by the fraction expressing the proportion of questions put to the infallible witness. The second witness may know *how often* he ought to answer 'yes;' but I give him no credit for that, because he is ignorant *when* he ought to answer 'yes.'

Let i be the proportion of questions put to the infallible witness, and let j be the proportion of questions which the ignorant witness answers in the first way. Then we have the following simple equations: —

$$(aa) = i\{(aa) + (ba)\} + (1-i)j\{(aa) + (ba)\},$$

$$(ab) = (1-i)j\{(ab) + (bb)\},$$

$$(ba) = (1-i)(1-j)\{(aa) + (ba)\},$$

$$(bb) = i\{(ab) + (bb)\} + (1-i)(1-j)\{(ab) + (bb)\}.$$

Now, whatever the method of predicting, these equations can always be satisfied by possible values of i and j, unless the answers are worse than if they had been taken at random. Consequently, in virtue of the two principles just enunciated, the value of i obtained by solving these equations is the measure of the science of the method. This value is,

$$i = \frac{(aa)}{(aa)+(ba)} - \frac{(ab)}{(ab)+(bb)},$$

$$= \frac{(aa)}{(aa)+(ba)} + \frac{(bb)}{(ab)+(bb)} - 1,$$

$$= \frac{(aa)(bb) - (ab)(ba)}{\{(aa)+(ba)\}\{(ab)+(bb)\}}.$$

Mr. Gilbert's formula has the same numerator, but