my reasons for holding them, or any defence of my convictions in the premises.

As to the age at which children should first take up the study of biology, I contend that it largely depends upon the aptness of the individual child, and the capacity for teaching of the instructor. My oldest son is not yet quite ten, and he can pass a stiff examination upon Morse's 'First book in zoölogy,' name the bones of the vertebrate skeleton, comprehends the general principles of a natural classification, reads well, and has his other studies fully up to those in biology, and, finally, makes an unusually creditable drawing *direct* from any natural object. I would say, then, to those children to whom all the advantages of the schools are open, that they may safely begin with their first steps in zoölogy and biology at nine years of age.

As to the methods, I would say, then, for a child of nine years of age, that mere descriptive zoölogy be simply considered a part of his general reading; that such training as comes from the study of the naming of animals I would surely confine to a very limited list of the commonest forms of the several groups, but let these be thoroughly understood; and I would say right here, that, even at this age, it is truly wonderful how well a child can comprehend the general principles of nomenclature, if they be properly presented to him. Even clear through the university course, I am by no means an advocate of the student putting forth the effort to commit to memory the names of animate objects, even so far as they apply to the fauna of his own country. Coming next to classification, I would say that this, too, be borne upon but lightly at first, though *its principles* can be introduced at a very early stage in the programme of biological education. What I object to, is the early course of zoölogical studies being based upon any system of classification. I agree with Professor Conn when he says that "classifications have, by reason of recent discoveries, grown so intricate and complicated that they no longer can be taught to the general student with any degree of satisfaction." But the principles of classification, as I say, can be easily made clear to the child; and it soon learns to grasp these, and prattles quite learnedly as to why bats are grouped with the mammals, and whales are not fishes!

By this time I expect my views upon this part of the subject have been anticipated; and I hasten to say that my firm convictions are, that the principle upon which biology should be taught to children, is to begin with the study of TYPES. Not only that, but I contend that it is the question of a study of types that should be held to, all the way through the entire course of study, until the day of graduation at the university.

And, figuratively speaking, at all ages these studies must be pursued with text-book in one hand and the actual specimen in the other, with the lens and scalpel constantly at work.

If we start in with a child nine years of age, and commence to carefully point out to it, constantly using fresh specimens, all that can be learned from the body of any *one kind* of small animal, appropriately illustrating it as we proceed with a sufficient number of the proper kind for comparisons, and introducing at the same time the simpler laws of chemistry and physiology, it is absolutely marvellous the interest that can be aroused, and the progress that is the outcome of it all. Children soon learn, too, to make wonderfully good sketches of their work, and may be easily taught to compare them, and lay them aside for future use.

The text-book for this purpose, treating, as it ought to, of a few types, should be thoroughly and carefully illustrated; and none of the systems should be in any way neglected or hastily passed over. Take the muscular system, for example. For children nine years of age, it will only be necessary to illustrate the larger and more important muscles of the trunk and extremities, but good figures of them must be given in the text-book; and, say the instructor has before him as his type some such an animal as a squirrel, he can easily lay bare the biceps in the fore-limb, and, in an attractive way for children, speak of the composition of a muscle, show the physics involved in its leverage, and say how it is found in most all vertebrates with fore-limbs, how in mammals it is inserted into the radius, and in many birds into the ulna; its presence in ourselves can at once be demonstrated upon any child present; and so on. Lessons of this kind, I know from personal experience, are entered into with a growing interest, and are pursued with an ever-increasing profit.

So far as I know, to my mind, the text-book in zoölogy and biology, for the use of our children from nine to fifteen years of age, remains yet to be written. R. W. SHUFELDT.

Fort Wingate, N. Mex., March 5.

Thought-transferrence.

I read with much surprise Mr. Edmund Gurney's letter on the article of which I gave an account in *Science* of Feb. 4. I thought I had made it quite clear that I was simply saying, in part in my words but mostly in their own, what two ladies had written on an overlooked factor in thought-transferrence. As these ladies have so clearly proved their ability to speak for themselves, I will take the liberty of forwarding them a copy of Mr. Gurney's letter, and, if they think it advisable, they may answer it.

The reason why I consider the article important is because it tells us something new and interesting about the 'number-habit,' not on account of its bearings on thought-transferrence. The latter point of view, however, was that which interested the authors of the article, and I thought it better to adopt their form of statement. The bearing of this fact on psychic research is to me of rather trivial interest compared to the psychological value of the fact itself. I fear there is great danger of magnifying the importance of psychic research in general, and of forgetting that it forms only a small and that rather an unimportant part of psychology.

It seems to me perfectly fair for the writers of the article in question to omit any detailed reference to the work of the English society; and I, for one, did not draw from it the inference which Mr. Gurney draws, — that they suppose the argument to apply to *all* the work of the English society. I do believe, however, that the principle has a very much wider application than Mr. Gurney supposes. The writers of the article in question took for granted some acquaintance with the work of the English society; and the charge of misrepresentation seems to me unfair against them, as I hope it is also unfair against my account of their article.

It can hardly be of interest to any one but myself to know that Mr. Gurney's own attempt at 'thought-transferrence' has been a failure. Not only have I read every page accessible to me of the writings of Mr. Gurney and his associates, and have begun reading the 'Phantasms of the living,' but, on the whole, I have spent more time in this department of literature than I care publicly to confess. The only justification with which I console myself for all this reading is the glimpse here and there of an interesting illustration of the psychology of 'psychic research' itself. If I have overestimated the importance of the article I reported, it may have been due to the bright contrast it afforded to so much of the literature on that topic with which I have come in contact.

J. J.

Baltimore, Md., March 12.

To some of the facts brought out by the English branch for psychical research, and which seem to me well established, — quite as well, indeed, as many facts in physical science which scientists accept because they cannot explain, — the American branch of the same society enters its demurrer. "The tests of so-called investigators have been rendered quite unreliable by the fact that they were themselves the dupes of their own ideas." Now, the investigator *may* be the dupe of his own fancies, — that is most true, — but his fancy may be a susceptibility favorable to the fact, or a non-receptive susceptibility, that would require more than a logical train of possibilities to dispel. He may be such a slave of sceptic habit, that the normal freedom of his judgment is weakened by preconceived ideas so tyrannical as to make of him a bigot. In scientific investigation the one man is as worthless as the other, — on the one hand, the scientific man who *will not* be convinced; and, on the other, the one who will be too easily convinced.

Humanity is made up of compounds pretty well known; and it seems hardly probable, that given the same opportunities, and with mental calibre of equal power, the English men of science should be the victims of their own fancies to a larger degree than those in the United States. So I take it that dupe No. 1 prevails in Europe, and dupe No. 2 in America. It will always be found difficult to explain psychological phenomena upon physical bases, — more than difficult: it is impossible. The theories followed out by the American branch do not seem to me to be applicable. In the first place, it is not a fact in mental science, that because the power of thought-transferrence occurs in one person, it must occur to a certain extent in all persons, or in at least a great many persons; and I very much question the existence of any mental system constructed upon the relation of the digits or the determination of numbers. Starting out with these preconceived, firmly rooted, and untenable hypotheses, the investigator has already made himself the dupe of an idea. He is the victim of the society's explanation. He comes to the work totally unqualified as an unprejudiced observer, because he is already prejudiced by preconceived trains of thought, originated by the society to which he belongs, and exaggerated by his own in-dwelling upon the subject. He has withdrawn from mental freedom something absolutely necessary to its unfettered action, and cannot give to the investigation that just and honest study which alone can be of service. The number of men in the world's life capable of passing such judgment is exceedingly small: they could be counted upon one's fingers. A man may be reverenced in the realm of letters, of astronomy, of medicine, of natural history, etc., and yet it is more than probable that he cannot bring to a crucial test of psychic phenomena the freedom of judgment that is necessary. In the very nature of things, I should doubt most strongly if a physicist is ever the proper person to pronounce upon metaphysical processes, because his whole habit of thought has been in a different direction.

To accept nothing as positive that has not been proved dwindles our world down to the geometrical conception of a 'point,' which has position without dimension: it makes of human life a mere idea, that as yet lacks logical method, and is without definite fashioning; and robs every one that takes the life-giving oxygen into his lungs without knowing why he does it or what ultimate purpose it subserves, of the very sweetest hope that a student can have, — that some day the mysteries that now torment us shall be made as clear as the noonday sun. This is not the test of psychological phenomena, and never can be.

I can understand, from a very considerable experience in hospital work on the continent, that many conditions of self-deception are self-created. A man may be the victim of excessive introspection, and may conjure up mental states of being and mental imageries which to him are absolute. Another may receive into a ductile mind as truth certain disputed ideas, because he has already tilled the ground for the reception of the seed. Another will fail to receive any thing, because he has determined either that he will not, or that, if he does, it will conflict with his preformed scientific conception of the matter. Both of these latter are certainly dupes. I have seen a few examples of thought-transferrence; but even the few were so unmistakably the evidences of a new force or power, and so free from any suspicion of fraud, that I cannot deny the possibility because I am unable to explain the fact. I certainly do not incline to relegate such power to the mere rudimentary conditions of elementary human life; neither has it been my experience to find that the agent or percipient were persons in whom the intellects were at all weakened. We know so little of consciousness, of brain-power, and of the power of the senses, that we should blushingly announce ourselves as ignorant and blind, before opening the door that leads to regions of which the wisest know absolutely nothing.

I am writing merely as my thoughts suggest, and not at all as one versed in this the most abstruse of all sciences; and these thoughts have been called out by a study of the plans and purposes of the society for investigating these phenomena. It seems to me that the ends and purposes aimed at are handicapped at the outset by certain definitions and premental conceptions that must be more or less dominant, and thus tyrannize over the understanding; so that the very man who thinks himself free becomes the dupe of preconceived ideas. The instinct of the animal that leads him to interpret certain moods of his master, and which is of a part with the whole transmission of heredity, — the automatic action, so to speak, of the higher nervous ganglia, or the impress that these ganglia have acquired by similar experiences through hundreds of preceding generations, — is quite another thing from the complex phenomena of thought-transferrence, which are the exponents of a much higher degree of civilization, calling for a much more elaborate and intricate association of psychic functions.

If even the least significant of all of the facts reported from England be accepted, we are left to deal with an unknown something quite apart from instinct, — something, for so it seems to me, which cannot be compared with it in any way, but which is the evidence of a higher order of brain-manifestation than we have yet met with.

HORATIO R. BIGELOW, M.D.

Leipzig, Feb. 28.

The tail of Chlamydoselachus.

A recent opportunity of examining a second specimen of Chlamydoselachus furnished the means of adding an item or two to our knowledge of that peculiar genus. In several points the example differed from that originally described. This was notably the case with the tail. On the later capture, this organ was a little more than one-fourth of the total length, and, with the vertebral column, tapered to a sharp extremity; whereas in the first one it stopped abruptly, with vertebrae of considerable size, as if truncate. On the new one, the lateral line, with a few short breaks posteriorly, continued to within an inch of the end of the tail. All this indicates that the tail of that which served as the type was deformed and incomplete: the deformity, in all likelihood, being of embryonic origin. Proportioned as the new one, the tail of the type would have been seventeen inches long, instead of which it was but little more than ten. Completed, the type would have had a total length of sixty-six inches, to a circumference of eleven and a half. The more recent specimen had a length of forty-eight, to a circumference of ten and a half inches, which made it rather less slender and snake-like than its predecessor.

Another difference occurred in the dentition, which, in the last examined, showed variations in the number of denticles between each lateral cusp and the median: sometimes there were two, sometimes but one.

The tropeic folds, abdominal keel, were present, as on the specimen from which the original description was taken.

S. GARMAN.

Cambridge, Mass., March 11.

The Quebec group.

Thinking it may be interesting to geologists to learn the latest conclusions in reference to the stratigraphical succession and distribution of the rocks in the province of Quebec, hitherto known as the 'Quebec group,' I send you the following brief observations on this subject: —

As is well known, the divisions made by my predecessor, the late Sir W. E. Logan, of this interesting and exceedingly complicated group of formations, were in ascending order, — Levis, Lauzon, and Sillery, — and these together were supposed to represent a peculiar phase of the calciferous and chazy formations of the New York lower paleozoic series. I have elsewhere made known as the result of personal investigation that portions of several systems and formations had evidently been included in the Quebec group as described in the 'Geology of Canada, 1863,' and depicted on the geological map of Canada, published in 1866. During a personal examination of a large portion of the area during the seasons of 1876, 1877, and 1878, I recognized strata which I considered clearly belonged to systems and formations ranging from pre-Cambrian to Silurian; and also that much of the so-called 'Sillery' was in reality not the youngest, but the oldest member of the group, and of pre-Cambrian age.

All subsequent investigation has confirmed the correctness of these conclusions, first advanced in a paper read before the Natural history society of Montreal in February, 1879, and more fully treated in reports and papers since published in 1880, 1883, and 1884. Since the date of the last of these publications, considerable additional information relating to the distribution of the several formations has been acquired; and I now find that no less than four distinct horizons can be recognized, each of which is marked by important bands of conglomerate. Three of these (Nos. 2, 3, and 4) are fossiliferous limestone conglomerates, while one (No. 1) is chiefly felspathic and dioritic, is non-fossiliferous, and generally presents the appearance of a volcanic agglomerate or breccia, which in places becomes a brecciated serpentine, or is otherwise variously altered, and is often schistose and micaceous, — pre-Cambrian.

No. 2 is of Cambrian age, and is best seen along the south shore and at the north end of the Island of Orleans, at Bic, at Metis, and at several points lower down, on the south side of the St. Lawrence Gulf.

No. 3 is the celebrated Levis conglomerate, well exposed at Point Levis and at the south-west end of the Island of Orleans. It is interbedded with gray and dark blue highly graptolitic slates, recognized by Professor Lapworth as marking the phyllograptus zone of Europe. It also recurs with its associated phyllograptus slates at several points between Metis and the Marsouin River on the south shore of the St. Lawrence, always in discordant contact with the strata of the preceding group.

No. 4 is the limestone conglomerate of the Quebec Citadel Hill. It occurs there in three or four more or less lenticular beds, none of which exceed six feet in thickness: they are associated and interbedded with black highly carbonaceous and graptolitic strata, yielding a valuable cement-stone. Both to the north-east, before reaching the Island of Orleans, and to the south-west, these beds are cut off by the curving line of the great St. Lawrence and Champlain or Appalachian fault, and are brought into abrupt contact with the red and greenish gray slates of No. 2. They appear again, however, on the south side of the St. Lawrence near St. Antoine, and thence pass beneath the drift-covered level country to the south-west. I believe these beds to be a part of the Utica, Hudson River, or Lorraine group. Professor Lapworth, who has recently examined the graptolitic fauna from these rocks, considers it to denote a stage older than Trenton limestone, but decidedly newer than the Levis phyllograptus zone. The latter view is entirely in accord with the stratigraphical evidence as first published by me in 1879; but, so far as the stratigraphy is at present known, it is as decidedly opposed to the former conclusion. Lists by Professor Lapworth, of the graptolites from the different horizons above named, will appear in the volume of the Transactions of the Royal society of Canada, shortly to be published.

The fauna of No. 2 conglomerate, as well as that of the associated slaty and shaly beds, is exclusively of Cambrian type, — Dictyonema sociale, Eophyton Linneanum, Cruziana (?) Paradoxides-Archaeocyathus, etc.