tendence of the National Educational Association meets annually in February, and many college teachers of education find it desirable to attend its meetings. Consequently the society plans to alternate between convocation week in January and the superintendents' meeting in February. It is very doubtful if the educationists could secure satisfactory attendance during convocation week unless the meetings were held in a very central location. Nevertheless, I feel that it is worth while occasionally, say every other year, to sacrifice something in order to come in touch with the other great societies. This object, however, would hardly be gained if other societies should act in the same manner, unless some agreement could be reached concerning the biennial sessions.

My suggestion is that once in two years all the societies meet in the same place, and that on alternate years the chance be given the affiliated groups to serve their various interests. The place of holding the biennial sessions should be on or near the trunk lines and have suitable hotel accommodations. In my judgment, too, much would be gained by returning biennially to the same place. It would tend to give the association a fixed home and, what seems to me of great importance, a permanent and reliable constituency.

James E. Russell.

Concerning the plan of holding our annual session in the winter and also of the conflicting interests of the association and the affiliated societies, it is perhaps too soon to give a decided opinion, but I have a strong impression that a definite plan separating the sessions of the association from those of the societies is necessary to the highest welfare of both. Unless something is done the affiliated societies will swamp the association.

My preference would be to have it generally understood that the affiliated societies make a special business of meeting during the convocation week, each one where it chooses, and that all come together in the summer, either the week before or the week after the National Educational Association, for a grand association meeting, which shall be scientific, technical and social, and where all papers will be delivered either before the general association or before the departments of the same. By such an arrangement every section would be a success, and there would be no serious conflict of interests, and the delightful social features of the association would be perhaps a prominent feature at the summer meeting.

In my judgment persons who claim membership in the American Association by virtue of membership in an affiliated society ought to pay something into the treasury of the association, or the affiliated society should pay for them. The present arrangement seems to me unfair and unjust.

At the present time the great body of people who would naturally be most interested in Section D have their special societies. Civil engineers, mechanical engineers, electrical engineers, architects and (including them all) the Society for the Promotion of Engineering Education; all these would naturally be more or less interested in the work of Section D. None of these societies are affiliated. They meet independently when they will, but they do not desire to conflict in any way with the American Association. In fact it may truthfully be said that the American Association looks at the matters which interest all engineers and teachers of engineering from a somewhat different point of view from that of the societies I have named, and consequently it has a distinct function and sphere of its own. Section D affords an opportunity for the members of all these societies to get together on a common platform.

C. M. Woodward.

PROFESSOR METCALF'S EVOLUTION CATECHISM.

To the Editor of Science: In Science of January 8, 1904, Professor Metcalf formulates (p. 75) a series of crucial evolutionary questions. It is undoubtedly true that 'much further observation' will be necessary to decide them, to the satisfaction of everybody, but it is not less evident that we have already a vastly larger body of evolutionary facts than we have adequately interpreted. In the belief that the problem is at present one of interpretation quite as much as of observation, I

venture brief replies to Professor Metcalf's questions, premising only that these suggestions are incidental to 'A Kinetic Theory of Evolution' outlined in SCIENCE of June 21, 1901, and in subsequent papers.

"Are there mutations which are distinct from fluctuating variations? Are fluctuating variations restricted to rather narrow limits, and are the larger variations which occur of a different sort, establishing a new mean about which a new series of fluctuating variations cluster?" Yes; mutations or spots which appear among inbred domesticated plants and animals differ from the 'fluctuating' individual diversity of unsegregated wild types in the amplitude or abruptness of the variation, and in a more or less pronounced reproductive debility. 'Fluctuations' and mutations are extremes of the same series of phenomena, but their evolutionary significance is very different. New types are built up through the interbreeding and accumulation of genetic variations, but mutations which depend for their existence on narrow segregation do not contribute to the evolutionary progress of species.

"Are mutations (or variations) definite or indefinite? Do they follow certain lines or do they occur in all directions?" Variations of both kinds occur in many directions. The idea that they exactly offset each other and thus maintain a stationary average has no warrant of observation and is opposed to the calculus of probabilities. Species tend to move in *some* directions, but not in all directions (Darwin), nor in one particular direction (Naegeli).

"If the direction of mutations (or variations) is wholly or in part predetermined, what are these predetermining factors? Are they internal (involved in the nature of the organism), or external (environmental), or both?" They are internal, but not predetermined. Organisms of the same descent under the same conditions give diverse mutations. Of their causes in detail nothing is known; mutations are, however, *induced* by persistent inbreeding. The direction or the extent of variation may also *depend* upon external conditions. A variation in the direction of larger size would not be able to develop without adequate food.

"Is there a tendency in mutants (or variants) to revert toward the condition of the parent stock?" Normal genetic variations are more vigorous and prepotent than their immediate relatives, but mutations tend to 'revert' when the abnormal inbreeding is remedied by crossing.

"Are mutants (or variants) of one sort more (or less) fertile or more (or less) vigorous when bred together than when bred with the parent stock or with mutants (or variants) of another sort?" Sustained vigor and fertility, and evolutionary progress, as well, depend on normal interbreeding (symbasis). Mutative varieties are, in general, rendered more vigorous by crossing with less inbred stock, but often at the loss of their peculiar characters.

"Does mutation (or variation) cause partial (or complete) segregation?" Mutations are sometimes completely segregated by sterility, perhaps also by cytological or other malformations which prevent the resumption of interbreeding, but such abnormalities have no general evolutionary significance.

"Are hybrids between mutants (or variants) of different sorts or between mutants (or variants) and the parent stock intermediate in character between the two parents, or do they follow wholly or chiefly one parent? If the latter, which parent is followed in the several kinds of crosses?" Crosses between different mutants or even between similar mutants of different descent tend to 'revert' to the parental type. In crosses between mutants and their immediate and equally inbred relatives the mutant is prepotent, but individuals of the parental type may be prepotent if of a sufficiently remote line of descent. When the divergence of descent is too great or too long-standing to permit a return to the ancestral form, or when the prepotency of the mutation is balanced, as it were, by the prepotency attaching to smaller degree of inbreeding of the form with which it is crossed, there result disjunctive or 'Mendelian' hybrids.*

* Further confirmation came to hand after this letter was sent in. Professor Davenport finds

The discussion of evolution has long since passed the stage when particular facts could be used to prove general conclusions. The difficulty with the current hypotheses of evolution through selection and mutation is that while apparently supported by some facts, they are as definitely contradicted by others; a theory which can accommodate both series of phenomena has a larger basis of probability than either. From the standpoint of the kinetic theory the rejection of selection as the actuating cause or principle of evolution does not require the denial of selective adaptation. The recognition, on the other hand, that mutations are not caused by environment, does not mean that they are definitely predetermined. The abrupt and striking but more or less sterile aberrations of heredity which occur under inbreeding do not show that evolution depends upon segregation. Neither do they afford evidence against the view that evolutionary progress goes forward through the gradual accumulation of lesser and more normal variations, independent of environment, but not beyond selective influence. The kinetic theory affords the explanation, hitherto lacking, of how selection produces adaptation. It does not set stationary organisms in motion, but it may, at times, determine which variation shall most affect the direction of the motion of the species.

O. F. Cook.

Washington, D. C.,
January 14, 1904.

(Science, N. S., 19: 112, January 15, 1904) that albino mice of mixed ancestry are more prepotent or less recessive than those of pure breed, a result contrary to that which should follow under the pure-germ-cell, character-unit theories of Bateson, Wilson and Castle. The improbability of these mechanical hypotheses was already evident, however, from the fact, known since the time of Darwin, that the crossing of two 'recessive' inbred 'mutations' may bring a return to the ancestral type. The tendency to disregard older data seems to indicate that the recent DeVriesian and Mendelian mutations of terminology are prepotent in closely segregated evolutionary investigations, but the ancestral facts are still vigorous and likely to reassert themselves whenever a wider intercourse of ideas is resumed.

THE ANIMAL PARASITE SUPPOSED TO BE THE CAUSE OF YELLOW FEVER.

In Science of January 1 there appeared a letter signed H. W. Robinson, which purported to be a defense of one of the members of the working party which I arraigned in my article under the above caption in Science of October 23, 1903.

In reference to this letter I beg to state that I am not expected to give any attention to what one has to say whose knowledge of the matter is second-hand, but that I am fully prepared to defend whatever I have written in my article, whenever any of the working party answers to my arraignment of its members.

J. C. Smith.

New Orleans, La.,
January 25, 1904.

SPECIAL ARTICLES.

A FISH NEW TO FLORIDA WATERS.

While dredging off the coast of Florida in 1902, the steamer *Fish Hawk* collected four specimens of a fish whose occurrence in that region was most unexpected and whose known distribution is thus extended in a most interesting direction. The fish in question is the snipe-fish or bellows-fish, *Macrorhamphosus scolopax* (Linnæus), which is common in the Mediterranean and has occasionally been found as far north as the southern coast of England, inhabiting depths up to 170 fathoms. The *Fish Hawk* specimens were taken at two stations in the Gulf Stream off Key West at depths of 98 and 109 fathoms, respectively.

There is one other known occurrence of this fish in American waters, recorded by Storer in the *Proceedings of the Boston Society of Natural History* for 1857 (Vol. VI.), a specimen having been found at Provincetown, Massachusetts.

H. M. Smith.

NOTE ON A RUBBER-PRODUCING PLANT.

Recent experiments have shown some interesting facts in regard to the products of *Picradenia odorata utilis,* Ckll., *Bulletin Colo. College Museum,* December, 1903, a plant belonging to the Compositæ and growing abundantly in the neighborhood of Buena