SCIENTIFIC JOURNALS AND ARTICLES.

The Museums Journal of Great Britain for September contains the following articles: 'On a Collection to Illustrate the Origin and Structure of Rocks,' by H. C. Sorby; 'A Method of Exhibiting Coins,' by F. R. Rowley; 'Notes on Models of Protozoa,' by F. R. Rowley; 'On the Hanging and Care of Pictures,' by Richard Quick; and 'A Method of Preserving Tortoises,' by J. E. Duerden. The reports of a number of museums are noted, giving a good idea of the general activity in museum work in England, as well as showing how much is being done there to make museums at once interesting and instructive to the general public. It is stated that it is the intention to make the Tolcross branch of the Glasgow Museum a museum for children.

The following extract from the report of the Stockport Museum deserves a wide circulation: "Many people do not realize that the true foundation of a municipal museum is educational, seeming to regard it as a receptacle for their useless old lumber and rubbish."

The Report of the Manchester Museum, Owens College, for 1905–1906, shows a welcome improvement in its finances, due to an extra appropriation by the university, which already furnishes the major part of the $14,000 (in round numbers) devoted to its support. Dr. Hoyle's address, 'The Education of a Curator,' is reprinted as one of the museum publications and should be widely read. There are still people who inquire if a curator needs any special training and apply for a position as curator of anything.

The Report of the Curator of the museum of the University of Michigan shows progress in rearranging and caring for the collections, and gives an outline of the summer's work in the ecological survey of Isle Royale. Under the direction of Mr. C. C. Adams the museum work seems to have been carried on in the best possible manner for a university museum, but it is doubtful if Mr. Adams's remarks as to the benefits of explorations can be applied indiscriminately to *all* museums. Mr. Adams has just accepted a position in the Museum of the Cincinnati Society of Natural History where his energy and experience in museum work will be of great service.

SOCIETIES AND ACADEMIES.

THE AMERICAN PHILOSOPHICAL SOCIETY.

THE program for the stated meeting, October 19, 1906, was as follows:

T. J. J. SEE, PH.D.: 'The Cause of Earthquakes, Mountain Formation and Kindred Phenomena connected with the Physics of the Earth.'

ERIC DOOLITTLE: 'Problems of Double Star Astronomy.' (With lantern illustrations.)

GEO. M. ROMMEL, B.S.A., AND E. F. PHILLIPS, PH.D.: 'Inheritance in the Female Line of Size of Litter in Poland China Sows.'

THE SOCIETY FOR THE PROMOTION OF AGRICULTURAL SCIENCE.

THE program for the meeting which will be held at Baton Rouge, La., on November 13, is as follows:

9:30 A.M. Meeting of officers and executive committee.

10:30 A.M. Public meeting. (1) 'Importance of Nitrogen as Plant Food,' Professor T. F. Hunt, Cornell University; (2) 'Teaching Agriculture in Public Schools,' Professor S. M. Tracy, Biloxi, Miss.; (3) 'The Growing Importance of Plant Physiology in Agricultural Education,' Dr. Chas. E. Bessey, University of Nebraska; (4) 'The Growing of Alfalfa East of the Mississippi,' Dr. J. E. Beal, Michigan Agricultural College; (5) 'The Problem of Reforesting New England,' Professor F. Wm. Rane, State Forester, Mass.

2:00 P.M. Symposium: Experimental Work. (*a*) 'What is Research?' Professor Thos. F. Hunt, Cornell University; (*b*) 'Tendencies in Station Work as Influenced by the Conception of Scientific Investigation,' Dr. H. J. Wheeler, Rhode Island College; (*c*) 'Scientific Methods in Research,' Dean H. J. Waters, University of Missouri; (*d*) 'The Experiment Stations and the Adams Act,' Dr. C. D. Woods, University of Maine. (*e*) General discussion.

8:00 P.M. Evening meeting. President's Annual Address. Subject: 'The Promotion of Agricultural Science,' Dr. Henry Prentiss Armsby, State College, Pa.

DISCUSSION AND CORRESPONDENCE.

THE SMITHSONIAN INSTITUTION AND RESEARCH.

So much has been said about the advantages of herding scientific workers that a small voice

on the advantages of isolation may not be out of order.

I have read with amazement about the innocuous desuetude into which the Smithsonian Institution is said to have fallen, of its crime of accumulating collections, the lament that

> A national museum has been developed, a new four million-dollar building is now going up for the same; a zoological garden and an astrophysical observatory have been established; finally costly experiments on flying machines have been provided for by congress, all under the management of the secretary of the Smithsonian Institution, who is not an officer of the nation, but elected as executive officer of the Smithson Trust, and paid exclusively from the Smithson fund.

These are, indeed, all serious crimes that are charged to the Smithsonian, and I am afraid that a close search would reveal others, *i. e.*, the organization of the U. S. Fisheries Bureau and the Bureau of Ethnology—perhaps still others. It was doubtless criminal for Langley to experiment with useless flying machines, and, if memory serves me, Henry was equally criminally working with the useless telegraph. Still some of these misdemeanors on the part of the Smithsonian officials seem to have their mitigating circumstances.

Every one ought to know, but, apparently, every one does not, that the truly national institutions of the National Museum, the Fisheries and Ethnological Bureaus, the Geological Survey, have provided the very opportunities for temporary or permanent research on the part of paid investigating curators and of visiting college professors that some one has asked should be provided. These provisions may not be for 'our' narrow little lines of research, but they are for research.

Divorce of the museum idea from the Smithsonian, if this implies the abolition of collections and discouragement of making collections, means the abandonment in biology at least of the fields of research that have in the past been most fruitful in detailed results and broad generalization. The plea seems to me to be not so much for a broad national institution as for the abolition of things that are in favor of the narrow lines in which 'we' are personally interested.

Mr. Hinrichs in his youthful days evidently looked upon Henry as a broad, great man, as I in my youthful days looked upon Baird as a broad, great man, and it comes as a great shock to me to find that my youthful vision was blurred and to read that Baird was only a 'specialist in fishes' and Langley a specialist in stars (and flying machines?). However, we are always inclined to think of the man clamoring for the shoes of some departing worthy as entirely too small in parts to fill them, and the trees we climbed when we were young were much larger than those that grow nowadays.

If we seriously ask ourselves whether the Smithsonian has increased and diffused knowledge among men, I think we must in sorrow admit that indeed it has, and we must, perhaps, even sneakingly honor the men who 'worked' the nation by starting various lines of investigation and then eked out the slender original means of the Smithsonian by gradually relegating these lines of research to independent bureaus of research supported by the nation. Perhaps, as an immigrant I shall not give too great offense by pleading guilty to some little patriotic pride in the collection of bureaus or departments which certainly form a great national institution of research.

The fact that the work in botany, in fishes, in geographical distribution, in geology and geography is done in departments independent of each other ought not to worry a college man, and the fact that the Smithsonian has hounded the departments to their work is not too great a divergence from the aims of Smithson.

The discussion concerning the Smithsonian is a mere trailer to the discussion of the Carnegie Institution to which we treated ourselves some time ago. In our disappointment that the means of the Carnegie Institution are not sufficient to enable us to do all that its establishment caused us to hope for, we turn on the Smithsonian and urge that it must be reformed and, with its much slenderer means, do what the Carnegie Institution found can not be done.

It is possible that the time has come to modify the methods used for 'the increase

and diffusion of knowledge among men.' I am not sure about that, but I have a conviction that it ought not to be turned into a superior club where scientific men may congregate to imbibe the spirit of research, take intellectual stimulants, eliminate their individuality and aberrant ideas and get into the beaten path.

For biology we have a splendid series of laboratories at Woods Hole, Cold Spring Harbor, Dry Tortugas, Beaufort, all of them national in their scope. But the Woods Hole laboratories do not receive the support from investigators that their equipment warrants, and the director of the Tortugas laboratory has recently advocated the abolition of the summer sessions of the universities that the professors who now prefer to teach would, in sheer desperation of *ennui* at nothing else to do, be compelled to conduct research at the splendidly equipped laboratories at the Tortugas.

It is my firm conviction that the middle-aged and older men for whom the advocated central, national, Washingtonian institution would exist, who have not laid out their own paths and are not diligently engaged in clearing and traveling them; who must be sustained by their environment and for whom, therefore, an environment must be created, are not worth any consideration whatsoever.

Segregation has been the most potent factor in organic evolution. Segregation has been and is the most potent factor in developing new ideas in biology. The mass has a regressing, leveling or swamping effect on incipient diverging ideas as well as on diverging variants. Self-sustaining ideas originate as rarely in a crowd without elbow room as self-sustaining mutations in small geographical areas.

Unique faunas are found on isolated islands, in caves or other segregated units of environment. The most pregnant ideas in biology were conceived by Wallace alone in the Malay Archipelago, by de Vries alone at Amsterdam, by Wagner alone in the tropics of America, by Gulich alone in the Sandwich Islands, by Mendel alone in his monastic seclusion and by Darwin flocking by himself at Down.

The astronomers at Arequipa would probably have a more congenial time at Washington, but they would not photograph the southern heavens.

What we need most is not more opportunities for the herding of scientific men, but opportunities for them to work to the best advantage where they are or where their material is to be found. If that happens to be in Washington, by all means let provision be made for them at Washington if it does not already exist, but let us not repeat the mistake made years ago, when it became fashionable to think that all that was worth doing must be done on and by the sea. I have, among others, two good friends in Washington who have confided to me, the one that as soon as the weather permits he leaves Washington, because he can do nothing in the swirl that there exists, and the other that when he has a big piece of work on hand he takes his material to one of the universities, because he can do much more there than at Washington.

If the time has come for the Smithsonian to adopt a different method for the 'increase and diffusion of knowledge among men,' I would urge, as I urged when the nascent Carnegie Institution was under discussion, what my own bitter experience leads me to believe is the most urgent need of American science.

My experience no doubt is not at all unique. I too have received liberally from the Smithsonian and its affiliated bureaus whatever publications were issued. This policy of the Smithsonian for the 'diffusion of knowledge' followed by government bureaus has become so liberal that we have protests and suggested reforms of its extravagance. I have been assisted by the loan of books, of valuable museum material and copies of inaccessible fragments of literature. I have reciprocated by sending specimens and preparing reports. I could ask for nothing better along these lines.

Several years ago I set myself to work on the development of the remarkable viviparous fishes of the Pacific coast. I gave all of my time to the collection and working up of the

material in a region at that time thousands of miles from any other embryologist or morphological zoologist. The work could be done nowhere else, but try as I might, not a cent could I secure from anywhere to support me in my work.[1] The result of that work was, aside from smaller papers describing new species, etc.:

1. 'On the Precocious Segregation of the Sex Cells in *Micrometrus aggregatus* Gibbons,' *Journ. Morph.*, V., pp. 480–492, 1 plate.

2. 'The Fishes of San Diego,' *Proc. U. S. Nat. Mus.*, XV., pp. 123–178, 9 plates. Giving spawning seasons and embryology, as well as a list of San Diego fishes.

3. 'On the Viviparous Fishes of the Pacific Coast of North America,' Bull. U. S. Fish Comm. for 1892, pp. 381–478, 27 plates.

4. 'Sex-differentiation in the Viviparous Teleost *Cymatogaster*,' *Arch. f. Entwickelungsm.*, IV., pp. 125–179, 6 plates.

A request for fifty cents a day while working in California was declined by one of the government bureaus. A request for assistance from two other institutions was declined.

The article 'On the Viviparous Fishes' was sold for $100, not quite twice as much as had been paid to draw one of the figures submitted to illustrate it. It is thus that scientific work has been encouraged in the past. The article was a fragment, and the viviparous fishes still await a worker *who must be in the 'field'* among those fishes. Wallace long ago pointed out that individual workers in the field do proportionately vastly more than big, expensive government expeditions. Just as surely vastly more will be accomplished if individual workers are subsidized to do their work where they can do it best than if they are herded at Washington.

The most urgent need is temporary or permanent research professorships: appointments made for specific work of men who will receive their pay from the appointing institution, who are responsible for all of their time and results to the appointing institution, but who carry on their work in their home institution or wherever else their work can be done to best advantage.

CARL H. EIGENMANN.

[1] Thirty odd dollars I spent in incidentally picking up rare or new species of fishes were refunded later.

THE MUTATION THEORY IN ANIMAL EVOLUTION.

THE question of the origin of species is that of the origin of specific characteristics or differential marks. According to one theory they arise gradually by accumulations of the order of fluctuations. According to the other they arise suddenly and completely as mutations. The former theory explains cases in which species are connected by intergrades. The latter best explains discontinuity in species; without it a subsidiary hypothesis to account for observed discontinuity is necessary.

The first reading of de Vries's great work 'Die Mutationstheorie' carried a conviction to the minds of many zoologists as well as botanists that the truth of the discontinuity theory—which has long been urged under other names—had been insufficiently recognized. Of late opposition is appearing and the mutationist is led to reexamine the grounds of his faith. One of the most vigorous of the reactionists is Merriam[1] (1906), who concludes his address before the zoological section of the American Association with the words: "The theory of the origin of species by mutation, therefore, far from being a great principle in biology, as some seem to believe, appears to be one of a hundred minor factors to be considered in rare cases as a possible explanation of the origin of particular species of plants, but, so far as known, not applicable in the case of animals."

The evidence for so sweeping a generalization is to be looked for in the body of the address and I have carefully reread and analyzed his paper. He offers first certain general objections to the mutation theory and then cites cases supporting the alternative theory of gradual modification. His general objections do not seem to me to be important. His query (p. 242) 'if sport variations are less likely to disappear by reversion than are

[1] 'Is Mutation a Factor in the Evolution of the Higher Vertebrates?' SCIENCE, XXIII., No. 581, February 16, 1906.