Harger, Oxford, and Mr. J. N. Bishop, Plainville.

Several papers were heard with great interest, followed by much discussion on these and botanical matters in general. It was also decided to hold field meetings at intervals through each season, more thoroughly to study the flora of the state, and give additional stimulus to the prosecution of careful work in this direction.

Withal, the meeting was very enjoyable, and indicated a permanently active organization.

Thirty-one members were accepted as organizers of the society and the probability of greatly increased membership is already apparent.

E. H. EAMES,
Secretary.

BIOLOGICAL SOCIETY OF WASHINGTON.

THE 368th meeting was held Saturday, March 7.

F. A. Lucas exhibited some lantern slides made from photographs taken by R. H. Beck, showing groups of several hundred specimens of *Conolophus cristatus,* one of the two large lizards found on the Galapagos Islands. Mr. Lucas stated that Mr. Beck had taken a large number of photographs showing the more striking features of the fauna and flora of those islands.

Frederick W. True spoke on the 'Attitudes and Movements of Living Whales,' illustrating his remarks by lantern slides showing whales as depicted in books and as they actually appear in life. The species discussed were the large whales pursued for commercial purposes, and the speaker showed that there was considerable discrepancy in the accounts of observers as to their behavior. Under this was included the form and height of the spout, the movements of tail and flippers, duration of stay beneath the surface and method of descending, or 'sounding.' Various observations were plotted on a large diagram, and attention was called to the fact that the closest agreement as to facts was found in observations made on the bowhead and sperm whales, the two species that had been longest hunted and were best known. It was suggested that with better knowledge of other species there would be better correspondence of the observations concerning them.

O. F. Cook presented 'Some Biological Aspects of Liberia,' exhibiting a number of views of the more characteristic features of the flora and describing in detail some of the more interesting trees and plants. It was stated that the oil palm was the only African palm not represented by some species in South America, and attention was called to the fact that this palm was not found in a wild state. Where it seemed to occur wild, observation showed that the spot had formerly been inhabited and the species was preserved and disseminated by the agency of man.

F. A. LUCAS.

DISCUSSION AND CORRESPONDENCE.

THE PUBLICATION OF REJECTED NAMES.

IN the issue of SCIENCE for January 30, 1903, p. 189, Professor T. D. A. Cockerell, under the above caption, calls attention to what he regards as adequate publication of rejected manuscript names by Mr. Banks and myself. As Professor Cockerell very well says, there is evidently a misconception or divergence of opinion among naturalists on this point that it is well worth while to discuss. I have taken the trouble to submit my particular case to some forty workers in systematic biology, and the 'various and sundry' ways that have been suggested for handling the question are certainly surprising, showing that the practice in such cases is by no means uniform. A large number, mainly zoologists, hold that my printing of Lesquereux's manuscript name *Carya globulosa* before the one I intended to give the organism was merely of the nature of narrative or explanation, and did not have the effect of validating the manuscript name. The intent of the author, it is said, is to be respected, and as it is perfectly clear that I intended to name it *Cucumites Lesquereuxii* and not *globulosa,* they hold that *Cucumites Lesquereuxii* stands. Others take an exactly opposite view, namely, that because I printed the manuscript name first and followed it by a description of the

fossil before printing the name I proposed to give it, I thereby validated the manuscript name, and no matter how plain the author's intent may have been, the specific name *globulosa* must prevail. They would, therefore, write the binomial as *Cucumites globulosus*. Accepting this latter view, an immediate and pronounced divergence of opinions arose as to the authority for the specific name and its combination. Some aver that, although I did mention Lesquereux's manuscript name, I was the first to rescue it from the limbo of *nomina nuda* and habilitate it by means of a description and illustration, hence it became my name. Those who hold this view would write it *Cucumites globulosus* (Knowlton), or if using the double citation, as *C. globulosus* (Knowlton) Cockerell, on the ground that Cockerell first actually made the combination in his note in SCIENCE. Others, all of them botanists, claim that *globulosa(us)* was Lesquereux's specific name which I had obligingly published for him, and that the authority should read: *Cucumites globulosus* (Lesquereux) Cockerell. Still others argue that although I did not actually refer *globulosa* to the genus *Cucumites* I virtually did so, and they would write it *C. globulosus* (Lesquereux) Knowlton. This last contingent, while denying the right to interpret my obvious intention to give the plant a new name, insist on supplying me with an intention to do that which I did *not* intend!

Tabulated we have the following results:

Cucumites Lesquereuxii Knowlton. Advocated by twenty-one systematists, mainly zoologists.

Cucumites globulosus (Knowlton). Advocated by two zoologists.

Cucumites globulosus (Knowlton) Cockerell. Advocated by six zoologists.

Cucumites globulosus (Lesquereux) Cockerell. Advocated by eleven botanists.

Cucumites globulosus (Lesquereux) Knowlton. Advocated by two botanists.

It may be worth while to attempt an analysis of the above diverse results to see if it is possible to ascertain the underlying principles which governed the several decisions. Those who advocate the first combination in the above list would seem to be going on the common-sense principle, namely, that the obvious intention of the author should be respected. This, as I understand it, the so-called Kew Rule permits. But it is very much with this as it is when a game is played with cards. It might be most logical for each card to have a fixed value, but when different games are played they are played according to the rules of the particular game, and the cards have the value fixed by the rules of that game. The ornithologists are supposed to be playing, to continue the simile, according to the rules of the American Ornithologists' Union, which, on the point at issue, is as follows:

"§ 5. Of names published simultaneously. Canon XVII., 3. Of names of undoubtedly equal pertinency, * * * that is to be preferred which stands first in the book."

As it is beyond question that the name *Carya globulosa* appears first in my paper, and is followed by a full description of the organism, the above rule would seem to fix *globulosa* as the proper specific name. In the matter of deciding the authority, Canon XXXII. of the A. O. U. code is very plain. This reads: 'A *nomen nudum*, generic or specific, may be adopted by a subsequent author, but the name takes both its date and authority from the time when, and from the author by whom, the name becomes clothed with significance by being properly defined and published.' In conjunction these rules fix the name as *Cucumites globulosus* (Knowlton).

The botanists are supposed to be working under the so-called Rochester rules, and this point is covered in part by Article VI., Publication of Species. 'Publication of a species consists only (1) in the distribution of a printed description of the species named.' As these conditions are fulfilled in my paper, this rule also fixes the specific name as *globulosa*. There appears to be no provision in the Rochester rules for fixing the authority in cases like this one under discussion.

In conclusion I may say that I am forced to agree with Professor Cockerell that *under the rules* the name of the Vermont fossil must

be *Cucumites globulosus,* although I am free to confess that is not the name I had intended it to bear! I would write the name and its authority as *C. globulosus* (Knowlton) Cockerell, and I may add, that, in my judgment, Professor Cockerell has himself further complicated the issue by intentionally publishing a combination in a field in which he has at most only a passing interest.

F. H. Knowlton.

Washington, D. C.

THOSE MANUSCRIPT NAMES.

To the Editor of Science: I am much averse to using the pages of scientific papers for nomenclatorial discussion, but since Professor Cockerell's and Dr. Bather's articles indicate that I introduced MS. names merely to upset them, a few words may not be amiss. Dr. Bather says 'It (*Filistata oceanea*) appears first on page 50 of Mr. Banks's paper.' Such is not the case, and in this very paper (p. 60, bottom) I refer to an unpublished name of Marx but am careful not to introduce it. Dr. Marx (as I state) published a list of spiders from the Galapagos Islands in 1889 which includes six MS. names. In order to make my paper on the spiders of these islands complete it was necessary to note previous publications. In order to show how many spiders were known from these islands I collated the previous lists (Butler's and Marx's) with my material, in so showing that three of Marx's published names were synonyms of previously described species, and two others were the same as those I would describe below. In sinking five of the six previously published names (every one of which is still a *nomen nudum*) under described species I believe I was doing a service. My case is not unique; I can mention dozens; commonly, however, the MS. name is referred to after the description. And the paper and ink wasted in so doing are as nothing to the time and type wasted in the two articles which are the mismated parents of this one. Nathan Banks.

EXPLORATION OF OKEFINOKEE SWAMP.

To the Editor of Science: Some of your readers may be interested to know that the vast wilderness, several hundred square miles in extent, known as Okefinokee Swamp, in southeastern Georgia, so long avoided by botanists and other scientists—though mentioned as long ago as 1791 in the writings of William Bartram—has at last been penetrated. In company with Mr. P. L. Ricker, of the U. S. Dept. of Agriculture, and a guide, I entered the swamp near the center of its eastern margin on August 6, and came out at the same place on the 8th, having in the meanwhile been about a dozen miles into the interior and secured a considerable number of interesting plants and photographs.

One of the first features of the swamp to attract my attention was the fact that all the thousands of cypress trees seen were undoubtedly *Taxodium imbricarium,* a species whose distinctness from the old *T. distichum* I have recently attempted to show (*Bull. Torr. Bot. Club,* 29: 383–399, June 20, 1902). According to the theory there proposed (see pp. 389, 395) this would seem to indicate that the Lafayette formation underlies the swamp, or at least that part of it visited by us; but direct evidence on this point is still wanting. This formation was actually observed however a few miles east of the swamp, and it is reasonable to suppose that it underlies the whole area.

Lumbering operations in the swamp seem to have been suspended for the last few years (owing mostly, it is said, to the death of the principal promoters of the scheme for deforesting and draining it), and fortunately the natural conditions have been very little altered thereby. The fauna seems to have suffered considerably from the ravages of sportsmen, but the flora is practically intact, and the swamp offers a number of most interesting problems in many branches of natural science.

Roland M. Harper.

Folkston, Charlton County, Georgia,
August 11, 1902.

SOUTHERLY DEVIATION OF FALLING BODIES.

Readers desiring a somewhat fuller historical account of experiments and theories relating to the southerly deviation of falling