of confusion, which should be eliminated as soon as possible, either by the actual description of the species, or by the rejection of these manuscript names. The mischievous practice of attaching names to insects without describing them has long been abandoned by lepidopterists in every branch of the study except sericiculture.

W. F. KIRBY.

London, England, Sept. 25.

Destroying Mosquitoes by Kerosene.

THE reason for the existence of mosquitoes has often been asked. Some means for their destruction has, perhaps, been even more earnestly sought after. The idea that their numbers can be kept down by propagating dragon-flies does not seem to be any longer entertained; and any experiment bearing on some means for their destruction is of interest. In a late number of *Insect Life*, Mr. L. O. Howard publishes a note upon the use of kerosene against them, the substance of which is as follows: On the surface of a pool of water, containing about 60 square feet, he poured four ounces of kerosene. This formed a very thin oily film on the surface of the water. On the 5th of July the pool was teeming with animal life, but for the next ten days that the pool was under observation no living insects were observed. At the end of this time, a count of the insects on a small portion of the surface, from which was estimated the total number, showed 7,400, — 370 of which were mosquitoes. The observation is of interest as showing the remedy to be an effective one, and, further, that a single application of oil will remain operative for ten days or longer, although two rain storms occurred during the interval. The matter is worthy of further observation and experiment.

JOSEPH F. JAMES.

Washington, D.C., Oct. 10.

Phonetics in Science.

FOLLOWING almost in the "wake" of the geological word-makers, who have apparently a dictionary of their own construction, comes another scientific writer who has decided to use the phonetic system of orthography. My attention was called to an article in a chemical journal published in this country, and almost at a glance I should have decided, had I not known the system, that the author had just finished writing a translation from the Spanish, and had his alphabet somewhat confused; for here before me was *sulfate;* but reading further, I should have said, perhaps, that he had just finished a German translation.

All this would have occurred to me if I had been ignorant of the existence of the phonetic system. Now, why did not this author change *phenol-phtalein*, which appears in the article referred to? Perhaps this word does not occur in the phonetic dictionary.

Is it not high time for American scientists to stop "coining" words? To be sure, these words differ from the geological ones in that they come well recommended by some philologists, and then the author in this case has not been guilty of owning an "orthographic mint." Why not continue to use the good old spelling, when it answers every requirement? The only disadvantage (?) in so doing, to my mind, may be in the fact that the words are longer than those in the phonetic system, and, as the advocates of this system claim, are more difficult to spell; so they are to some people, but unless they are foreigners, one is not in the habit of meeting such scientists in every-day life. Scarcely has our American language secured a strong foot-hold than it must be changed for the benefit of a few who would receive the honors as the originators and champions of a new system of orthography. I know of one advocate (not the author, it is needless to say, of the paper in the chemical journal above referred to) who "prides himself not only upon his ability to use the phonetic system, but also upon his beautiful English." Yet this very same man habitually uses, for example, such phrases as "Ain't he funny?" Still this hardly belongs to my criticism of phonetics in science. Why not leave the phonetic system to the philologists; why incorporate it in our scientific work?

When the advocates of this system have succeeded in establishing a strong foot hold for their system, and permanency (for it) stares the old system in the face — and let us hope that time is far distant — then we can almost picture our laboring scientists, with the new system (?) dictionary before them, ever fearful of beginning one word with an F after the new, and the next with a Ph after the system they have so successfully used for generations.

E.

Grand-Gulf Formation.

DR. WM. H. DALL'S contribution to Miocene literature under this head calls for some notice, were it only to thank that eminent palæontologist for correcting my mistake with regard to the Gnathodon of Pascagoula and Mobile. With his unrivalled opportunities of comparison and long experience in these studies, his determination is naturally satisfactory and final. I knew that in mollusks the young and the adult forms often differ considerably; but I knew not the life history of this one.

It is complimentary to me also that he has accepted my outline of the evolution of the Florida Peninsula,[1] although he probably arrived at his conclusions from different and independent sources. And I wish to correct the impression he seems to have of my notions of the genesis of the Grand Gulf. I do not say that the Pascagoula is a deep-sea formation, but speak of it as a "marine aspect" of the more intensely fresh-water Grand Gulf on the Mississippi; and I do not suppose that in an estuary marine influences prevail over the fluviatile, in order to foster the life of any of the creatures that have left their remains in these calcareous clays and sands; so that it may be said to be "partially of marine genesis." The same views here expressed by Dr. Dall were indicated by myself in another paper published by the Geological Survey of Alabama on the "Nita Crevasse" in 1889, in which I speak of the progress of later formations on and in the Mississippi Sound and its older extension as presenting a "marine-aspect" of the "Port-Hudson group" of Dr. Hilgard, and sufficiently different to be called the Biloxi Formation — a nomenclature I understood to have been approved by him among others. The method of genesis sketched in that paper for the Port Hudson was considered applicable to the older Post-Eocene formations of the same embayment.

I do not perceive, therefore, that Dr. Dall's "correction of my definition of these clays" was "required;" nor have I any to make of his, for similar views have been elaborated for the forthcoming Alabama Geological Report, which will be in effect a new edition of Bulletin 37 of the United States Geological Survey.

The only criticism here to which Dr. Dall might seem amenable is a tacit endorsement of his own brochure of January last upon these same Miocene formations, in which it may be said he has permitted conjecture upon general principles somewhat to outrun and forestall positive discovery. Hasty generalization is the bane of science. The Pascagoula Clays may be equivalent to his Chesapeake, but the testimony as yet can scarcely be said to be satisfactory. Whilst he has shown the younger Miocene of northern Florida, originally named by me the Waldo Formation, phases of which are seen at White Springs, in Hamilton County, and in the overlying clays at Aspalåga on the Apalachicola River, to be Chesapeake; this surely cannot be identical with the upper layers at Alum Bluff, much less with the lower.[2] As he himself has shown, the latter is an older Miocene, identical with that occurring on Chipola at Bailey's Bridge, and called by myself Chipola at a time when, from high water, I had not seen the *Ortholax beds* at Alum Bluff, and when I had not seen the perfect instance of contact and overlap presented at that place. At that time, I had previously discovered a Miocene in the vicinity of Defuniak Springs, on Shoal River, and on Alaqua River (and named it from the last), tracing it across Choctawhatchie, near Knox Hill, and across Washington County a little south of Vernon, and across Chipola at Abe Springs, eight miles south of Ten-Mile Bayou, the principal site of the older Miocene. With the help of Mr. Jüssen (both of us then working with Mr. Geo. H. Eldridge on the geological

[1] See Dr. J. W. Spencer's First Report of the Geological Survey of Georgia, p. 60; and short papers of my own, read severally at the meetings of the Geological Society of America, August, 1891, and August, 1892.

[2] There is no fossiliferous formation at Hawthorne, nor any at Ocheesee, as Dr. Dall seems to suppose.