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.

survey of Florida) the differences between these two formations was established, and for the younger the name of Aliqua revives. Whether this is identical with the Chesapeake and Carolinian or not is for another discussion At the same time the same parties identified the Chattahoochee beds of Langdon, which underlie the Miocenes of Georgia and northern Florida, with the Chipola beds, and traced their continuity westward across the Choctawhatchie, until, meeting with the syncline of the great roll from Alabama, they sink out of sight under the great sand-beds which fill the depression now drained by Shool River.

The connection of these two Florida Miocenes with the eastward extension of the Grand Gulf into south Alabama is matter for field research, and cannot be decided in the closet upon general principles. Enough is certain, however, to render it clear that if it is proper to draw the line between an older and a younger Miocene in Florida, such a distinction continues westward into Alabama and Mississippi; and where can we draw it better than upon lithological grounds between the water-holding stratified sands and sandstones of the lower Grand Gulf and those overcapping clays which, pierced at Brewton and Pallard 70 feet, at Mobile 735, at Biloxi 770, at Pearl River 800, and at New Orleans 1,200 feet, yield similar flows of water with similar clays and fossils? Of the latter I have other collections, which shall be submitted to Dr. Dall, now that I know his attention has been turned to the matter.

Upon the use of the term *formation*, I finally have to say that it is at least provisional, for every discoverer to name every structure he finds having peculiarities from some locality where it is prominently developed, although in the course of palæontological research many of these provisional names may disappear; and I submit that the prevailing American practice is not an abuse. For these reasons I shall still insist upon the propriety of calling the Pascagoula Clays the Pascagoula Formation.

LAWRENCE C. JOHNSON.

Meridian, Miss., Oct. 2.

Jealousy in Infants.

OF my two children one is a boy of four years, the other a girl of ten months. The boy has just returned home after an absence of some months. His sister displays great affection for him. She is also much attached to her nurse, more so at times apparently than to any other member of the household.

Now if, while the girl is sitting on a mat alone or on the lap of either of her parents, the nurse should take the boy upon her knee and fondle him, the girl will immediately cry out in a distressful way, in a tone not precisely indicative of anger or vexation, but more nearly similar to the tone of grief or disappointed desire. In the case described the infant will not be appeased unless the nurse puts down the boy and takes her up It will not avail for the nurse to take her up on one knee, leaving the boy on the other.

If, however, while the nurse has the infant in her arms, either of the parents takes up the boy and caresses him, the girl displays only a strong interest, but no annoyance whatever.

It is evident then that the outburst of feeling in the former case was a display of jealousy. And, as the child is not precocious, it is allowable to look upon this case as an instance of ordinary mental development in children.

It is wonderful enough that infants of a few weeks or months should make unmistakable manifestations of the simpler emotions of fear, affection, and anger. But that an emotion so complex as jealousy should appear so early as at the age of ten months is especially remarkable, and indicates a degree of development at this age which, in the absence of observation, might justly be deemed incredible.

I have not by me the works of Taine, Preyer, or Perrez, and so am not able to say what observations, if any, they made in respect to this particular matter. Darwin observed jealousy in an infant of fifteen and a half months, but adds, "it would probably be exhibited by infants at an earlier age if they were tried in a fitting manner."

A. STEVENSON.

Arthur, Ontario, Canada.

Is There a Sense of Direction?

THE recent articles in *Science* by Dr. Hall and Dr. Work on this subject tempt me to say that in early life I was a believer in this sense, my belief being derived from Cooper's Leather Stocking Tales and similar sources. The winter of 1855–56 was spent in what was then called "the bad-axe country" of western Wisconsin, in company with an old French-Canadian trapper, who seemed to possess this gift in a (to me) marvellous degree; and, as he boasted of it and never to my knowledge made a mistake, my belief in this sense was confirmed.

The next winter, with a very limited knowledge of the Ojibwa tongue, picked up on the Bad Axe, I went with a government survey into northern Minnesota in the capacity of interpreter. Here the subject was discussed in camp, and the sceptics proposed a test. Five Indians were blind-folded, turned around several times, and led half a mile from camp in different directions. Not one could point to the camp until the bandage was removed from his eyes, nor could they point to the north. As soon as they could see they easily found the camp, although it was in the flat, low-rolling country north-east of Crow Wing, where there are no prominent land marks to be seen from the heavy-timbered lands. On several other occasions it was found that the Ojibwa was guided by the lie of the land, as indicated by water-courses, the twist of trees as seen on stubs denuded of bark, the sun, and the many minor indications of the cardinal points that are known to expert woodsmen, both white and red. Therefore I agree with Dr. Hall that man does not possess an instinct which teaches him to find his way to a given point regardless of darkness or of previous knowledge of locality.

I cannot agree that any animal possesses this sense. If so, it would be the wild animals, whose necessities would keep the sense in training, and not those whose needs have been supplied by man. Dr. Hall cites the cat, which has been taken in a box for fifty miles and yet reached home. This may be so; but such instances, if true, are recorded as wonderful, as they truly are; while the thousands of other cats which were taken less than five miles from home and never returned are never recorded. Dr. Work mentions the many carrier pigeons which never return, and it is generally conceded that these birds depend on sight alone, their trainers taking them short distances at first, and then increasing them until they know the way to the loft.

Let us take the case of the greatest of all migrating animals, the wild goose. All of us who have seen anything of these birds have seen them lost in a fog. Dr. Work thinks their flying at different altitudes may be determined by "the character of the upper currents," and if these currents determine the density of fogs, he is right; for on a clear day, when the geese can see many miles ahead and get a bird's-eye view of landmarks fifty miles distant, they fly very high, but let rain or mist prevail, and they drop within reach of gun-powder, because they must come near the earth to get their bearings and preserve the direction of their flight, by vision alone.

I have, among my flock of wild fowl, a pair of brant, *B. bernicla* (the only goose that Atlantic coast gunners call "brant," although in the West every goose is a "brant," except the Canada goose). One of these birds strayed from a flock going north in the spring of 1890, during one of the darkest of nights, when the rain came as hard as rain can come, and was captured while flying around a street-lamp in the village, thoroughly bewildered. The other was taken the same night two miles south of the village by a boy who found it on the ground. Such instances are common in every rural locality, not only with the "black brant," but with its larger relative the Canada goose as well; and if there are better navigators in the animal world who should have the "sense of direction," if there is such a sense, I do not know what animals they are.

Dr. Work covers the case in his last paragraph, when he says: "Whatever instincts animals may have in this direction, man has the same, with the additional faculty of reason." That is, he covers the question of a "sense of direction" in animals, and allows man as much; but I cannot subscribe to his implied assumption of reason by man alone. That, however, is another question.

Cold Spring Harbor, N.Y. FRED MATHER.