matics, and Dr. J. H. Orton, chief naturalist at the Plymouth Marine Biological Laboratory, has been appointed Derby professor of zoology. Dr. D. B. Blacklock, professor of tropical diseases of Africa in the university, has been made Walter Myers professor of parasitology.

DISCUSSION

BIOLOGICAL CONTROL OF THE PRICKLY PEAR IN MADAGASCAR

THE recent accounts in SCIENCE of prickly pear control in Australia have dealt with the problem only from the standpoint that the complete eradication of this pest is an unmixed blessing. However true this view-point may be for most places in the world, in the south of Madagascar the destruction of the *raketa* by a cochineal insect (*Coccus cacti*) is being looked upon both by the natives and the Europeans as a calamity.

What are the conditions in the south of Madagascar that make it unlike the rest of the world, differing from the territory a few miles to the north where the disappearance of the prickly pear has been witnessed with acclaim? M. Decary, for many years a French official in southern Madagascar, and a botanist of note, has called attention[1] to the dependence of the Antandroy natives upon the *raketa,* quoting a local proverb to the effect that "The *raketa* and the Antandroy are inseparable," and going so far as to say that without the prickly pear there could have developed no Antandroy tribe!

Certainly this plant has its undesirable features, but to these people it is not only endurable, but essential; it furnishes food and drink for man and beast in this desert region, and these at the time of year when they are most necessary. Among other things it also provides impenetrable barricades around the cultivated fields, the livestock corrals and the villages.

Some years ago, before the appearance of the cochineal insect, a partially successful attempt was made to introduce *Opuntia inermis* into this region to replace the thorny types (*O. dilleni* and *O. ficus-indica*) which, during the last two centuries, have become the commonest plants in this region. Although *O. inermis* is immune to the attacks of the insect, one of the cultural requirements for its establishment has been a barrier of the thorny type sufficient to keep out the livestock; hence the destruction of the immune species is indirectly being brought about by the destruction of the species actually susceptible to the insect.

I can not offer any direct testimony as to the exact nomenclature of the causal insects; however, I can vouch for the thoroughness of the destruction now being wrought. Our party marched for days through country where, a few months before, the commonest plant had been the fifteen-foot-high prickly pear, now all completely destroyed; very rarely we would see a weak shoot, six inches high, which had come up from the root and which was also rapidly being destroyed. If such complete eradication could be effected in Australia and other places where the presence of Opuntia is not desired, in a few months the prickly pear would cease to be a problem.

CHARLES F. SWINGLE
BUREAU OF PLANT INDUSTRY,
U. S. DEPARTMENT OF AGRICULTURE

[1] Bull. Economic Madagascar, 1927 (1); Rev. Bot. App. No. 50, 1925, and No. 77, 1928.

HORSES, DOGS AND CATS

IN a recent number of SCIENCE (May 10, p. 494), Professor W. D. Matthew objects to my using the horses as an example of "linear evolution involving a time element." But he admits, apparently in connection with horses, that:

> We can and do have, in many cases, a succession of collateral ancestors so nearly related to the direct genetic line as to afford, when critically studied with due recognition of their status, a clear record of the physical evolution of the race, sometimes in more general, sometimes in more detailed terms, according to the nearness of their approximation to the direct ancestral line.

This "direct ancestral line" from *Eohippus* to *Equus* was precisely the line to which I referred. He therefore himself predicates its existence, and admits that it involves a time element. He says that in the case of the phylogeny of the horses the "analogy to the growth of a tree is a sound and a real one." No one doubts this. But does not the trunk of the tree run from *Eohippus* at the base to *Equus* at the summit?

He regards my statement that "the gap between cats and dogs is broad, and it remains broad throughout the fossil record" as misleading. However, he says

> No one, so far as I know, ever suggested that cats became dogs or dogs cats . . . but it *has* been believed that these two diverse families of Carnivora are descended from the primitive Carnivora (Miacidae) of the Eocene epoch.

I said that both cats and dogs are carnivorous mammals, expecting that zoologists would understand

that this presupposed descent from an ancestor common to these two groups in the Carnivora. He says that my statement "is evidently intended to give the impression that the 'gap' between them is not reduced as we trace them backward. That is very far from being correct." I never said or implied anything about the gap not being reduced; I said that it remains broad, and that "cats never became dogs nor dogs cats"—and they never did. This he admits.

He mentions my "new" theory of evolution with no explanation of the quotation marks. I never said it was new. What I said was that "It is rather a harmonizing of previous theories than a new idea; but parts of it [concerning the interrelationships of the phyla] are wholly new." This statement is absolutely correct. He adds that "the idea of separate origins of the major phyla of animal life [just what are major and minor phyla?] was a commonplace when I went to college in the late eighties, and still remains an open question, so far as I know." This gives a totally erroneous impression of my viewpoint. I traced all the phyla to a *common,* not to a *separate,* origin. Had he taken the trouble to read what I wrote in the paper to which I referred in a foot-note he would have discovered that I had offered a solution to the problem that so worried Professor Meckel in 1811 and interested him in the late eighties.

Lastly he says that my statements seem to him "gravely misleading as to the actual facts of phyletic evolution, although worded in so vague a way as usually to escape being absolute misstatements."

I would like to see Professor Matthew, or any one else, formulate general statements concerning phyletic evolution which will be equally applicable to crustaceans, insects, mollusks, vertebrates and other types of animal life and at the same time will not be "vague" when considered as a detailed exposition of the case in any single phylum or part of a phylum.

If Professor Matthew will point out a group of well-known creatures which demonstrate the principles of an evolutionary line or tree better than the horses, and another group of well-known animals that show systematic isolation better than the cats, we shall all be very grateful for the information. And if he will go further and arrange all the phyla in such a way as to combine a common origin and simultaneous development more logically than my scheme does he will have performed an outstanding service.

Until that time I can not see that he has any valid reason for dissenting from what I said regarding evolution.

AUSTIN H. CLARK

U. S. NATIONAL MUSEUM

ATMOSPHERIC ELECTRICITY

I WAS much interested in the account of the generation of static electricity in sand-storms, given by Mr. R. H. Canfield in SCIENCE for May 3, 1929.

On the night of April 30 I camped on the desert near Yuma, Arizona, with a companion; I slept on a cot while he occupied a mattress on the ground. During the night a sand-storm came up with a rather high wind. Being unable to keep control of the bedclothes on the cot, I moved into the car, which had an all-steel body, for the night.

Upon arising in the morning and touching the car, my companion received a rather painful spark from it, and this was repeated several times in the next few minutes. The storm meantime had blown out. There was no effect whatever so far as I was concerned.

A partial theory to account for this would be that the car body became charged with static of opposite sign to that of the ground; that my companion had a skin charge of the same sign as that of the earth and myself the same as of the car body, and that the potential naturally equalized when he touched the car. If this is true, however, I should have noted a spark between myself and the ground on getting out, but did not.

The unrest noted by Mr. Canfield is a very marked symptom in all the sand-storms I have experienced, and in some of them I have been inside sand-tight quarters and in a comfortable bed. On the other hand, I have slept in high winds unprotected, or in sandy blankets, without feeling particular discomfort —certainly not the same kind of discomfort.

This appears to be a subject which needs elucidation and experimentation, the results of which might be of more importance than may appear at first sight.

VICTOR A. ENDERSBY

R. H. CANFIELD'S article in the May 3 issue of SCIENCE on "Atmospheric Electricity During Sand Storms" brings to mind some accidental observations made by the writer in the late winter of 1919, which may be of some interest.

The writer was installing a wireless receiving set in his house in Boston, Massachusetts. Upon accidentally bringing the lead-in wire into close proximity with the ground connection, a strong spark was observed to jump between the two conductors. Two wires were arranged and varied until the maximum distance the spark would jump was found. This turned out to be about one centimeter. Thereafter sparks would jump at intervals of about two seconds with extreme regularity. The discharge began as a