species is indeed related to the monkeys, possibly more closely than even to the anthropoid apes.

THE REPORT ON THE MUSEUMS of America and Canada, recently made by Mr. Ball of the Dublin museum to the Science and art department of England, is not a very satisfactory document. Apparently designed to furnish hints to similar museums in the United Kingdom, it is nevertheless chiefly occupied with descriptions of the scope of the different establishments and of the contents, and to some extent the general arrangements of the several museums. But the account of the last is generally unsatisfactory and imperfect, while very slight or no mention is made of such devices as are characteristically American, and in which museology has been notably advanced by us. The best applications of American ingenuity to questions of installation are unnoticed: such as, the methods by which cases are made air-tight, and are locked at several points by a single turn of the key; by which shelf-supports are made light, secure, and graceful, and variable at pleasure with slight labor; by which a case applied to one use can be converted in a few minutes to another very different one without interfering with its sightliness;—these and many other problems of museum economy are altogether overlooked. The unit system of the National museum and the systematic registry of the Smithsonian institution are praised but not explained; while the applications of museums to public educational uses by the special arrangement of their material is very inadequately treated. Although it is true that in this last point our museums have more to show in promise than in fulfilment, we have still not a little to teach Europe; while America, on its side, has much to learn from such collections, for example, as the Liverpool free museum.

LETTERS TO THE EDITOR.

Miocene deposits in Florida.

IN view of the discussion as to the extent of miocene deposits in Florida, it may be of interest to call attention to the discovery of the extremely characteristic Ecphora quadricostata by Dr. R. E. C. Stearns at Tampa. The matrix is a compacted fine greenish sand, crumbling under moderate pressure. The locality of the find is on the long rocky point. It is probable that there is a large area in Florida corresponding in age to what has been called miocene in Virginia and the Carolinas, and that it includes part of the phosphatic sandstones, as well as the mammalian and reptilian bone-deposits noted by Jeffries Wyman, Leidy, Neill, and others.

WM. H. DALL, *U.S. geol. survey.*

Washington, D.C., July 23.

Abert's squirrel.

I have read with interest the article in *Science* respecting the Sciurus Aberti, from Dr. Shufeldt.

Sciurus Aberti is not uncommon in northern Colorado. I have seen it as far north as the Câche à La Poudre River, about 40° 30′ north latitude, and up to eleven thousand six hundred feet altitude near Gray's Peak. In this part of Colorado (latitude 39° 45′ north), and along the South Platte River in the mountains south-west of Golden, I have seen this spring three different individuals,—two of them black; one gray and lighter beneath, with tips of its hair on its back and sides mottled with black. In fact, we see them here from gray to blackish gray, and entirely black, although but little differing in size, and all noticeable by long, tufted ears. It is more terrestrial than arboreal in its habits, and, from its extreme range, cannot be called or considered a southern species straggling northward. Having been in the San Francisco mountains, and in all northern Arizona, I have not seen any S. Aberti as deep black as those in northern Colorado.

I have mentioned its existence here up to eleven thousand six hundred feet altitude; but I should qualify this statement by saying, that a squirrel in every respect identical with the S. Aberti was seen by me several times at the Loneland Pass, west of Gray's Peak. But it was more than twice its size; indeed, larger than any other species of gray, black, or fox squirrel I have ever shot or seen. Its habitat was near timber-line, feeding on pine-cones, and generally returning to the enormous heaps of disintegrated rocks which seemed its usual abiding-place. I never succeeded in getting a specimen of this rare squirrel at that place.

E. L. BERTHOUD.

Golden, Col., July 2.

Color associations with the months.

A lady whom I had the pleasure of visiting to inform myself concerning some curious planchette-writing in which she had participated, has, she told me in answer to my inquiries, several interesting arbitrary associations of the class which was discovered by Mr. Francis Galton, and of which the number-form is the most familiar example. She had a curious number-form, — a form for the twenty-four hours, and another for the months. A sister had likewise various forms, but different from those of the first-mentioned lady. Both said that music always speaks. 'Why, yes! it speaks, *of course*,' they both remarked.

The one to whom I wish specially to refer associated colors with the months, and in a way which struck me as particularly curious, as it is a jumble of arbitrary and of obviously natural associations.

January, February, and March are bright yellow; upon a second question, 'shining white yellow.'

April is blue, 'the shade ladies call French blue.'

May, light yellow, 'not at all like January.'

June, bright green.

July is glaring yellow; and *August*, orange.

September is golden brown; *October*, dark brown.

November is 'indiscriminate gray. I cannot exactly describe it: it is like lead color.'

December is gray.

This case appears to me sufficiently different from any of those mentioned by Galton to deserve special notice.

It would be very desirable, I think, to make a systematic investigation of the influence of heredity on such associations of color and form. Could not the Psychical society undertake such work?

CHARLES S. MINOT.

Boston, July 22.

Maxwell's demons.

Sir William Thomson has shown that since work is readily converted into heat, while heat is never wholly transformed into work, or in fact into any other form of energy, there must continually take place what Tait calls a *degradation* of energy; while its *dissipation* is pronounced to be the inevitable consequence of certain laws, connecting heat and work, established by thermodynamics.

Maxwell has pointed out that one of these laws is by no means a necessary truth ['Theory of heat,' chapter xxii., Limitation of the second law of thermodynamics]. Theory shows, that, in what is called a state of uniform temperature, some of the molecules of a body have by chance much greater velocities than others. If, therefore, as Maxwell says, we could suppose the existence of small beings, capable of following the motion of each molecule, and opening or shutting holes in a partition so as to allow the fastest molecules to pass through one way and the slowest the other, it might be possible theoretically, without expending any work, to separate a gas into two portions, — one hot and the other cold, — in contradiction to the second law of thermodynamics.

It seemed to me of interest to point out that what, as Maxwell has shown, could be done by the agency of these imaginary beings, can be and often is actually accomplished by the aid of a sort of natural selection.

When the motion of a molecule in the surface of a body happens to exceed a certain limit, it may be thrown off completely from that surface, as in ordinary evaporation. Hence in the case of astronomical bodies, particularly masses of gas, the molecules of greatest velocity may gradually be separated from the remainder as effectually as by the operation of Maxwell's small beings.

It is true, that, in overcoming the attraction of the central mass, the escaping molecules may be deprived of the whole or a portion of their velocity; but the transformation of heat into work marks the process still more distinctly as an exception to the second law of thermodynamics, which "asserts," according to Maxwell, "that it is impossible to transform any part of the heat of a body into mechanical work, except by allowing heat to pass from that body into another at a lower temperature" ['Theory of heat,' chapter viii.].

One might now dismiss the subject as a mere curiosity; but is it not possible that what may be called the *renovation* of energy plays an important part in the history of the universe? While philosophers, anxious to preserve their store of available energy, may speculate on the possible equivalence of renovation and dissipation, will not the scientist hesitate, without further examination, to extend the principle of universal dissipation from physical to astronomical phenomena?

HAROLD WHITING.

The classification and paleontology of the U. S. tertiary deposits.

In penning my protest (*Science*, June 12) against some recent geological and paleontological speculations of Dr. Otto Meyer, I had intended that it should represent my final words in the matter, inasmuch as the article under discussion appeared to me unworthy of exhaustive criticism. The appearance of instalment No. 2 of the same series (which, if any thing, is only more remarkable than No. 1), and a rejoinder to the first from Prof. E. W. Hilgard, constrain me to add a few additional paragraphs, more, perhaps, of a general than of a special character.

Professor Hilgard says, "I emphatically agree with Heilprin as to the impossibility of subverting the cumulative stratigraphical evidence to the effect that the relative superposition of the several principal stages — the Burstone, Claiborne, Jackson, and Vicksburg groups — cannot be otherwise than as heretofore ascertained;" and, further, "I recall to my mind that years ago I had occasion to repel a similar attempt, on the part of Mr. Conrad, to subvert the relative position of the Jackson and Vicksburg groups upon supposed paleontological evidence." It might appear, from the conjunction of these expressions, that the only evidence supporting the accepted superposition of the different members of the southern old tertiaries was of a stratigraphical character, and that the paleontological evidence was in conflict with that derived from stratigraphy. As a matter of fact, however, the paleontological evidence, whatever it may have been when Conrad first devised his scheme of classification, is, as we now know it, absolutely comfirmatory of the pregnant facts which the stratigraphy of the region presents; and, indeed, it would be difficult to find a region of similar deposits where it is more so. The absence or scarcity of forms of a distinctively old-type facies in the Vicksburg beds, and the introduction there of new forms whose equivalents or immediate representatives are known only from the newer horizon, are sufficient in themselves to establish the position. While it may be true, although this is far from being proven, that not a single one of the Vicksburg fossils is identical with species belonging to the typical oligocene basin of Germany, it is equally true that several of the species find their analogues or equivalents in the deposits of San Domingo, which are indisputably of post-eocene age; and whatever Dr. Meyer's own individual opinion may be as to the bugbear Orbitoides, and to its value as a 'leitfossil,' the keen appreciation of Hautken, Rupert Jones, Karrer, Fuchs, Suess, and Duncan has long since settled the question. It is amusing to have the forty-year old opinions of D'Orbigny and Edward Forbes referred to as authority on the value or no-value of certain fossil forms whose organization was barely known at the time that the opinions were rendered, and whose differences from other (distantly) allied forms were not even dreamed of. With singular perversity of purpose, Dr. Meyer fails to inform his readers that the American foraminifer whose merits are discussed by Professor Forbes, is confounded by that naturalist with a form which belongs not only to a distinct genus and family from Orbitoides, but to a distinct sub-order.

Aside from the testimony of the Vicksburg fossils