our mountains have been largely determined by their geological structure, and by faults, contortions, and subsidences in the strata of which they are composed.

I cannot argue this question here. Suffice it to say that the "Great Gutter Theory," as I venture to call it, does not, in my opinion, explain our hills or our glens. There has been, no doubt, enormous denudation. But "in the main" the forms express structure, and the effects of subterranean force.

Mr. Green refers to the "graphic illustrations" of Mr. Geikie's book. But unfortunately those illustrations are sometimes very incorrect. For example, the general view given of the south-western termination of the Highland ranges, as seen from above Gourock on the Clyde, is a view as defective and incorrect as it is possible for a geological landscape to be. I know that range of hills well, and have seen it since my childhood in every variety of light and shadow. I have also drawn it frequently, and know almost every line of it by heart. It presents a section across a great anticlinal, as was first pointed out to me by Murchison; and it is full of surface markings which reveal its structure. Not one line of these is given in Mr. Geikie's drawing. If he had been sketching a set of mole-hills he could not have made them more featureless—more utterly devoid of their distinctive forms.

Let us have facts before theories. Let us have our hills so drawn as to express the lines of structure as they are seen in Nature, and in their relation to outline. But very often the eye sees nothing except what the brain behind it has preconceived; and a geologist who draws a mountain with a theory of guttering in his head, is pretty sure to make a mess of it.

There is really nothing in the argument about an average level along the tops, as any sure indication of an original "table-land," with all its hollows due to guttering. All sedimentary materials having an average composition, when subjected to strains, pressures, or fractures, would, and must, exhibit average resulting forms. This general fact is equally consistent with more than one explanation.

I believe Mr. Geikie has modified his former views as to the action of ice. A closer inspection of the Highlands will, I am convinced, modify greatly in other ways his teaching as to the small share which structure, and subterranean force, have had in determining the physical geography of the country.

October 15. ARGYLL.

IN your last issue Prof. A. H. Green, reviewing Dr. A. Geikie's "The Scenery of Scotland viewed in Connexion with its Physical Geology," described the alleged resemblance between the Durness fossils and certain North American types as "an announcement of the greatest interest." The fact is certainly of the "greatest interest," but the "announcement" was made nearly thirty years ago by the late J. W. Salter in the *Quarterly Journal* of the Geological Society, 1858, p. 381. Mr. Salter refers to the fauna as "this truly North American assemblage," and compares the species one by one with Prof. Hall's types.

CH. CALLAWAY.

Wellington, Shropshire, October 16.

[WE have referred these letters to Mr. Green, who has sent us the following reply.—ED.]

IT is well known that the Duke of Argyll has long been a strenuous and consistent opponent of the views as to the origin of the surface features of the earth which are accepted by the majority of geologists. Indeed, if I had been disposed to be personal, I do not think that I could have quoted a more pertinent illustration than his Grace of a fact in the history of opinion to which I drew attention in the opening part of my review of the "Scenery of Scotland." He hears not Moses and the prophets, and I fear he will not be persuaded by the pleadings of one of their humbler followers; but if he will let me have my small say, I will first point out that his objection to the expression "surface features" seems to me to savour a little of quibbling. It is a general rule of criticism to interpret any ambiguous words by the context. The whole tenor of my article shows that I did not use the words in the first of the two meanings which the Duke says they may bear. Again, I am quite prepared to admit that geological structure has had a large share in determining the form of the ground; and I cannot find that either Dr. Geikie, or any other upholder of the Gutter Theory (I thank thee, Duke, for teaching me that word: no happier designation could be found), denies that subterranean force has played an important part in determining the physical geology of a country. Rather the contrary, for hear Dr. Geikie himself. He avows himself wishful that his reader should "recognize that a belief in the paramount efficacy of superficial denudation in the origin of the features of the land is compatible with the fullest admission of the existence and potency of subterranean disturbance. Inability to make this recognition," he says, "has led to absurd misconceptions and misrepresentations of the views of those who hold that the topography of the land is essentially the result of a process of sculpture" ("Scenery of Scotland," pp. 95, 96).

I will leave Dr. Geikie to take care of himself and defend the drawing the accuracy of which is impugned by his critic. I do not know the special landscape of Fig. 19, but I have enjoyed a few panoramic views of Highland scenery, and I can honestly say thus much: I have everywhere recognized those surface *markings* (may I again congratulate his Grace on the happiness of this phrase?) which indicate the geological structure of the ground beneath, but I have in every case been still more struck by that general flat-toppedness on which special stress is laid by Dr. Geikie. The comparatively slight prominence given to these surface markings in Fig. 19 will be easily understood if w bear in mind the one point which that cut was intended to illustrate.

I may add that I am extremely sorry if any words of mine seem to imply that I grudge my old friend Salter the credit due to him with regard to the Durness fossils. The expression I have used could be made to bear this meaning, and I am much obliged to Dr. Callaway for giving me an opportunity of disavowing any such intention. A. H. GREEN.

Leeds, October 20.

A Hydroid Parasitic on a Fish.

DURING my studies the past summer at the Newport Marine Laboratory I captured a single specimen of an osseous fish, *Seriola zonata*, Cuv., which exhibits a most interesting example of parasitism or possibly commensalism. Upon the outer wall of its body an extraordinary hydroid was found to have attached itself. As this mode of life is unique for a hydroid, it is thought that a mention of it, and a statement of the peculiar modifications which the hydroid has suffered, may be not without interest to others besides special students of the jelly-fishes. The hydroid is new to science, and on that account the name Hydrichthys is suggested to designate it. The hydroid will later be described and figured under the name *Hydrichthys mirus*, gen. et sp. nov.

The colony of Hydrichthys is found on the side of the body and near to the anal fin of the fish, Seriola. It forms a reddish cluster or patch of bodies, and was at first mistaken for a fungoid growth. When it was examined by means of a microscope its animal nature was easily seen and its hydroid affinities clearly made out. The fish was kept alive in an aquarium and medusæ raised from the attached hydroid. The hydroid colony is composed of two sets of individuals. These two kinds of individuals arise from a flat plate formed of branching tubes, by which the colony is attached to the body of the fish. The two kinds of individuals noticed in the cluster are the sexual bodies (gonosomes), and the "filiform bodies" (structures of unknown function).

The sexual bodies have the form of grape-like clusters of buds mounted on small contractile peduncles, which branch from a central axis or stalk. The filiform bodies are simple, elongated, flask-shaped structures, destitute of appendages, with a central cavity and terminal orifice. Neither of these two kinds of individuals have tentacles around or near a *mouth opening*, nor any structures which can be compared with these bodies, which are almost universal among fixed hydroids.

The first kind of individuals are the gonosomes or sexual bodies. They arise from the flat basal plate of branching tubes, by which the union of the colony with the outer wall of the fish is effected. Each hydroid gonosome consists of a main stem with lateral branches. At the end of each lateral branch there is a crowded cluster of small buds, which are immature jelly-fishes in all stages of growth. Each gonosome resembles a bunch of reddish and orange-coloured grapes.

The filiform bodies are simpler in structure than the sexual clusters or gonosomes. They are destitute of tentacles and are flask-like, with a cavity and terminal orifice. They are very sensitive, and move about with freedom, never, however, being

detached. The fish, Seriola, was kept alive until the larger buds of the grape-like gonosomes separated from the hydroids. These buds are medusæ, different from any which I have ever seen, but with close affinities to common and well-known genera. A large glass aquarium containing several gallons of water was found to be swarming with these medusæ two days after the capture of the Seriola.

Each fully-grown medusa closely resembles the genus Sarsia. It has an oval bell, four broad unbranched radial tubes, and four long simple tentacles. There are no octocysts on the margin of the bell.

If the strange form of the hydroid was not known to me, it would have been very easy to call this medusa a near relative of Sarsia. The medusa belongs to a group, called by Agassiz the Tubularians, but its hydroid is different from that of any other member of the group.

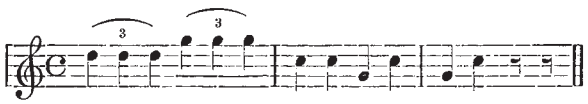
One other parasitic hydroid may be thought to be related to Hydrichthys. I refer to the Polypodium, described from the ova of the sturgeon. A description of Hydrichthys with figures of the fish (Seriola) to which it is attached, and of the hydroid with its medusa, will soon be published by me. As a discussion of its relation to other hydroids has little interest except to a specialist in the study of medusæ, a comparison of Hydrichthys with Polypodium and other genera is reserved until my complete diagnosis of the genus and species. J. WALTER FEWKES.

Cambridge, Mass., U.S.A.

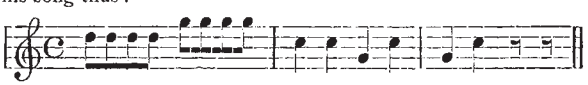
Music in Nature.

IN NATURE for August 11 (p. 343) there is an interesting article on music in Nature; the writer evidently being inclined to deny that true musical notes, and especially several notes in succession having a musical relation to one another, can be found in bird songs. However this may be in the Old World, we have in the New at least one example of a bird which not only sings, or rather whistles, pure and well-sustained musical notes, but has a succession of notes with such intervals as to form a simple melody. I refer to the scarlet tanager.

While we were at The Thousand Islands early in the summer of 1886, one of these brilliant fellows carried on a courtship among the trees close to our cottage, repeating incessantly during the first two days that we heard him the following strain,



in a clear, bright whistle. After the first two days he changed his song thus:—



and during the three weeks that we heard him he made no other variation, except that he occasionally repeated the last two notes a third time, thus filling out the bar. The notes were taken down by a trained musician, and if whistled give the tanager's song exactly.

It may be mentioned that, though perhaps the most brilliant in plumage of our Canadian birds, the male tanager referred to made no attempt at concealment, but swept like a living flame from tree to tree close to the cottage, and when singing preferred to sit on the topmost bough of a pine near by.

A. P. COLEMAN.

Faraday Hall, Victoria University, Coburg, Ontario, October 8.

Swifts.

THE following facts relating to the habits of the swifts were observed by paying close attention to these remarkable birds during the past summer. For more than a month, *i.e.* from June 1 to July 12, we watched them here. On the fine evenings about forty of them (the males I believe), ascended high into the air at about 9 o'clock, and after wheeling about for a minute or two, screaming loudly, fled straight away, sometimes in one direction, sometimes in another. White, in the "Natural History of Selborne," notices that: "Just before they retire whole groups of them assemble high in the air, and squeak and shoot about with wonderful rapidity." But the most wonderful part of the proceeding is that they do not come down again that night. At all events I can show that they do not come down again before 10.30, at which time I do not think they would be able to find their nests under the eaves of the church. Between the dates above-mentioned there were only six days during which I did not see or hear the swifts ascend and fly off. Three of these days were rainy, and the swifts stayed at home, and on three other days I was not able to watch them. The churchyard adjoins the garden of this house, and numbers of swifts build in the church, which is but a few feet from where we sit out and walk about in the summer evenings.

After seeing the high-flying swifts safely off to the south-west at 9.10 one night, I sat on a tombstone under the north eaves where most of them build, until 10.30. Two swifts hawking low for flies entered their nests after 9.10, but one of them was flying low while the high-flyers were in sight, and the other came out of its nest after they had gone, and both had retired before 9.20. On the other side of the church my father (the vicar) and my brother, who both took a keen interest in the doings of the swifts, were keeping watch alternately, and only two low-flyers were out there after the others had gone. The high-flyers did not return. On several other nights we watched until 11 o'clock, though not quite continuously, but quite closely enough to make certain that none returned. I think it most probable that owing to the darkness they do not return until the break of day, and further, that they *remain on the wing all night*. This last feat, though sufficiently startling, will, I am convinced, not be deemed impossible by those who have had good opportunities (and made use of them) for studying the ways of swifts and their wonderful powers of flight. As far as my observation goes, the swift settles nowhere except at its own nesting-place.

I shall be very glad of any information tending to throw light upon the question, and I shall be very pleased to give any of your correspondents any further information within my knowledge concerning this curious habit of the swifts, and the proofs thereof, to set out which in this letter would take up too much of your valuable space.

White also says (p. 180, original edition) he has never seen the swift carrying materials to its nest, and suggests that it usurps that of the sparrow. This does not accord with my own observation here. I have repeatedly seen swifts taking bents of grass in their beaks to their nests, and I have again and again scattered feathers on the wind from the sound-holes in the steeple, and from the steps of the cross in the churchyard, and seen them eagerly seized within a few feet of my head by numerous swifts. Their nests are neat, small, and shallow, and very firm, the materials being glued together by the viscous saliva of the builders. AUBREY EDWARDS.

The Vicarage, Orleton R.S.O., Herefordshire, October 13.

Hughes's Induction Balance.

HAVING just made a Hughes's induction balance, I have, in the course of some experiments with it, observed what was new to me, for I have not seen it mentioned in any account of the balance. I take the liberty, therefore, of asking through your columns whether the explanation resolves itself into the difference between paramagnetic and diamagnetic substances. The apertures of my bobbins are 1½ inch in diameter; my primary current is from three Daniell's, and the break is a bent steel spring whose free point just grazes the surface of a mercury cup, so that the merest touch with a finger causes a series of regular breaks. Now, if I place an iron or steel disk, or ring, such as a key-ring, inside the aperture, the telephone sounds loudly if the plane of the disk or ring is at *right angles* to the plane of the coils; but very very faintly if it is *parallel* to the plane of the coils. On the other hand, if a disk, or ring, or coil of wire, of any of the diamagnetic metals—copper, brass, zinc, silver, gold, aluminium, lead—be used, the telephone sounds loudly if the plane of the disk or ring be *parallel* to the plane of the coils; but very faintly, if at all, when it is perpendicular to the plane of the coils. Further, if a short *bar* of soft iron, or of nickel, be inserted so that the length of the bar is parallel to the plane of the coils, almost no sound is heard; but if it be turned through a right angle so as to be perpendicular to the plane of the coils, the sound is a maximum. Have we in this simple instrument the ready means of distinguishing paramagnetic from diamagnetic substances? J. COOK.

Central College, Bangalore, S. India, September 26.