

CORAL FORMATIONS.

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WHEN the old navigators, after long and weary voyaging, at last came upon the coral islands of the southern Pacific and gazed with delight upon the circles of green foliage lifted above the frothing breakers, verily they must have thought that they had found the Enchanted Isles.

Probably no portion of the earth's surface is more like fairy land or more wonderfully beautiful than a typical coral island. But the beauty of these islands, although so exquisite, is not their chief attraction, for as one learns how numerous they are, how unique their structure, how peculiarly they are distributed through the oceans, how unlike other reefs and islands is their form, elevation above sea level, and indeed everything that pertains to them, he becomes eager to know why they are as they are and by what processes they have come into being.

It soon becomes evident even to a very casual observer that the geological and chemical principles, which are sufficient to explain the existence of other reefs and islands, are not satisfactory when applied to these, and that some different conditions must be sought to account for the different results which have been attained in these singular structures.

What these conditions are or have been in the past it is not easy to determine, and, although from the time of the first navigator coral formations must have excited curiosity, no theory to account for them was made known until in 1837 Mr. Darwin, after his voyage in the *Beagle*, published his well-known theory.

From the first this theory was received with general approval, nor is this strange, for it is at once so simple and so apparently sufficient that nothing more seems to be needed. This theory was also greatly strengthened by the endorsement of Professor Dana after his return from the Wilkes Expedition in 1842.

For many years Mr. Darwin's theory was scarcely questioned, and certainly there is nothing improbable in any of the conditions which it requires. The only question is, Do the observed facts warrant its acceptance? As will be remembered, Mr. Darwin supposed that the whole vast area in which coral islands are found has been slowly sinking for a very long time, that islands of the usual sort, which formerly existed, have wholly disappeared through subsidence, and that about these islands there grew masses of coral which in time formed fringing reefs, that as the island sank the coral grew upward more rapidly on the outer or seaward side because there food was more abundant, and as the island sank the reef would presently be separated from the island by a strip of water; that is, the fringing reef would become a barrier reef. Finally, the island having disappeared, the reef, would become an island of more or less annular form enclosing a lagoon that is an atoll.

So long ago as 1851, Professor Agassiz, after studying the Florida reefs, declared that he found no evidence of subsidence, and that the structure of the southern part of Florida must be explained in some other way.

So also Semper, in the Pelew and Philippine islands, Dr. Guppy in the Solomons, and Dr. Bain in the Bermudas, had been studying coral formations, and all these observers found the old theory inadequate to account for the structural peculiarities noticed.

Later, and more important than these, are the observations of the naturalists of the *Challenger* at Tahiti, which have led many scientists to reject the commonly received theory.

However great the dissatisfaction with Mr. Darwin's

theory may have been, it was not given utterance until in 1880 Mr. Murray, one of the *Challenger* naturalists, published a theory quite unlike that which had been current. Although first stated by Mr. Murray, this theory is to be regarded as an outgrowth from the objections to the older one.

Mr. Darwin himself noticed that in some cases corals grew upon submerged platforms or banks and also that the growth was most rapid on the seaward side of a reef where food was most abundant, and all subsequent investigators have noticed the same fact.

Mr. Murray assumes a sufficient number of such platforms to afford foundations for coral growth, and that the peculiar form of reef or island would be determined by well-known conditions.

Of course the upward growth of the coral would be in a solid mass if growth went on equally, and this is sometimes the case, but usually because of the rapid increase of the outer zone of coral and because of the solvent action of the sea water upon the dead or weakly growing coral in the interior zone the forming island becomes annular, that is, an atoll.

Deep-sea soundings have proved that such submerged banks and islands as are demanded by this theory do exist and are more numerous than has been supposed, and also that by the accumulation of shells and all sorts of debris such foundations, if at first too deep, may be raised nearer the surface and into the coral-growing region.

On the other hand, mountain peaks, rising above the surface, may be worn down below it by erosion.

As atolls may begin as fringing reefs and may even at first be platforms of coral rock, so barrier reefs may begin as fringing reefs, and as they grow outward the solid coral be dissolved and worn away between the reef and the land, thus changing one into the other.

There are then these two theories at present before us. Which is to finally prevail? The naturalists of the *Challenger* expedition are fully committed to the new view and so are many leading English scientists.

If one seeking information should chance upon an article by the Duke of Argyll in the *Nineteenth Century* of September, 1887, or Professor Geikie's Presidential address before the Royal Physical Society of Edinburgh, he would probably conclude that the question had been finally settled in favor of Mr. Murray's views.

If, however, he should turn to the November number of the same periodical he would discover that no such result had been reached and that it is not probable that it soon will be.

Certainly when such an authority as Professor Huxley can write as he does in the latter article, "I happen to have spent the best part of three years among coral reefs, and when Mr. Murray's work appeared I said to myself that until I had two or three months to give to the subject * * * I must be content to remain in a condition of suspended judgment," it becomes us to be modest in expressing an opinion.

Should one still seek for information upon this subject he may find in the *American Journal of Science and Arts*, vol. XXX, 1885, sundry articles by Professor Dana in which there is very strong advocacy of Mr. Darwin's theory and opposition to that of Mr. Murray. While it would be idle at present to attempt to decide as to the value of either theory we may perhaps do well to consider some of the facts before us.

That there has been subsidence in great areas of the sea bottom cannot be doubted nor can it be doubted that there are great areas where there has been elevation, and in still other areas there does not appear to have been either rising or sinking.

Evidence is continually increasing that in different coral-growing areas different processes have gone on and that since all coral islands have not been made in the same way no single, all-comprehensive theory is possible.

Dr. Guppy found at the Solomon Islands that, adjacent to the shore, corals grew vigorously, while outside of this zone there was a space where debris from the shore so fouled the water that no corals grew, while still farther out they grew finely. It is easy to see that the first zone would make a fringing reef, the zone affected by debris would be open water, and the outer zone a barrier reef, and thus these varieties of coral formation be produced without the conditions of either theory. Nor is it at all improbable that other methods of coral island making may be discovered as further investigations reveal new facts, and, while it may be regarded as most probable that Mr. Murray's theory will be held sufficient to explain the larger part of the coral formations of the globe, it is also probable that Mr. Darwin's views will never be wholly set aside, but will always be needed to account for extensive groups of reefs and islands, while here and there all over the region of coral island making there will be found phenomena which require other explanation because of special peculiarities.

THE PROTECTION OF OUR WILD PLANTS AND ANIMALS.

BY JOHN GIFFORD, SWARTHMORE COLLEGE, PA.

A FEW years ago an association for the protection of plants was founded in Switzerland at Geneva. Tourists, and even botanists, were guilty of such vandalism that many feared the extermination of certain rare plants. By the dissemination of seeds and other means, however, many species have been protected by this society in Switzerland and elsewhere.

Although we have forestry associations in this country we have as yet done nothing toward the protection of rare plants.

In south Jersey, for instance, there are many unusual and beautiful species, but owing to the action of winds, fires and voracious botanists they are becoming gradually scarcer.

Along the beaches of the seashore the forests are destroyed for the building of resorts, in other places they are buried by moving sand dunes. The *Schizaea pusilla* is a little fern, which is not found elsewhere in the United States. It grows in three or four isolated patches in the low pine barrens of south Jersey. One patch has already been almost wholly destroyed by forest fires, and from the others hundreds of specimens are carried away by greedy botanists every September. The extinction of this species is only a question of a very few years.

This applies to almost every locality in the United States. There are few places which cannot boast of a few rare species.

The writer knows of one instance where a class of young botanists exterminated a patch *Aplectrum hiemale*, in a region where it was very rare, by eating the corms.

In spite of game protective societies, owing to the thoughtlessness of sportsmen, many of our wild animals have disappeared. A few deer still linger in the pines of south Jersey, but every season their number is remarkably lessened. Had they a place of refuge where they could always remain unmolested, their extinction could be prevented.

It is hoped that the Government may set aside in every state a tract of guarded land. A few acres showing the nature of the country in the wild state will be appreciated more in years to come than at the present time.

*See Westwood's Modern Classification of Insects on Larval Mycetophilidæ.

There the trees may remain untouched, there remarkable and unusual plants may grow in safety, and there the wild animals may find a refuge. The advantages of such a scheme are too numerous to mention. The retaining of a typical portion of each kind of territory in every state, together with its plants and animals, guarded every day of the year, would not only delight the naturalists and lovers of nature, but would insure at least a small portion of forest country here and there, which tends to lessen in many ways the destructive forces of nature.

Dr. Charles Dolley and others of the American Association for the Advancement of Education have arranged to collect and preserve on their property at Avalon all the plants peculiar to the beaches of the Jersey coast. This is one of the objects of the association, and it hopes to control some land in the low pine barren region where no man will be allowed to botanize or hunt.

SILK SPINNING FLY LARVÆ.

BY H. GARMAN, LEXINGTON, KY.

IN a brief paper printed in *Science* recently a silk spinning cave larva was described by me and referred to the order Diptera. Its general appearance and its habit of making a thread are features in which it approaches the larvæ of Lepidoptera, a resemblance which has been commented on by others in conversation with me since. Yet the larva in question is unmistakably Dipterous, and it was part of my object in publishing the note to call attention in an indirect way to the fact long, but not very generally, known,* that larvæ of certain flies approximate the Lepidoptera, in spinning silken threads. In saying that they produce silk, I wish, however, to be understood as in no way implying that the threads have the exact chemical and physical properties of the silken fibres made by the silkworm. They are silk from the biological, not from the commercial point of view. They are produced by special glands differing little, if at all, from the silk glands of other insects, are employed by these larvæ for a purpose, and are not consequently to be compared with the trail of slime left by a slug or worm.

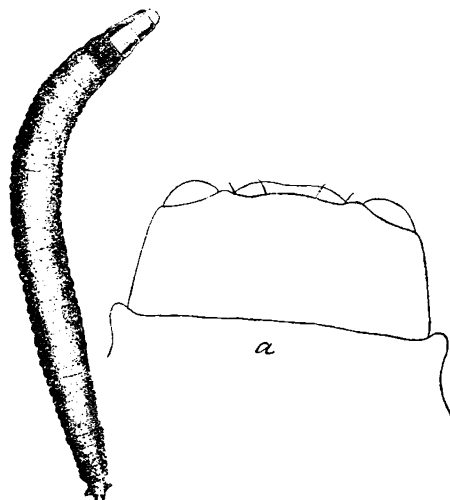


FIG. 1.

My attention was first attracted to such larvæ while making examinations of Kentucky caves. I have, however, been long familiar with other larvæ belonging to the same order, which habitually spin threads having a very important relation to their welfare. In small streams in McLean County, Illinois, occurs a larval *Simulium* which produces such threads. Another species is extremely abundant in rills in eastern Kentucky, where the rocks