The example cited above raises several important questions. Do these 'habitat-varieties' interbreed? If so, what is their progeny like? If not, then physiologically they are distinct species, though morphologically they are so similar. It is probable that after a freshet there is a mixing up of animals living in different habitats in a stream, and that on such occasions these two 'habitat-varieties' might interbreed. It is also known that torrential animals resort to slow waters (either in pools and puddles in the course of the stream or near the banks), and thus probably different 'habitat-varieties' come together at the time of breeding. If the progeny of these fishes include both types of individuals, how do they become segregated into their respective habitats? If, on the other hand, the progeny are alike, do some of them lose their fins after segregation into different habitats?

SUNDER LAL HORA.

Zoological Survey of India,
Indian Museum, Calcutta,
July 14.

[1] Blyth, *Jour. As. Soc. Bengal*, **29**, p. 169.
[2] Günther, *Cat. Fish. Brit. Mus.*, **7**, p. 371; 1868.
[3] Day, *Fish. India*, p. 611, pl. clv. fig. 6; 1878.
[4] Vinciguerra, *Ann. Mus. Civ. Stor. Nat. Genova*, **29**, p. 220; 1890.
[5] Hora, *Rec. Ind. Mus.*, **22**, p. 31; 1921.
[6] Hora, *Phil. Trans. Roy. Soc. London* (B), **218**, p. 265; 1930.

Siliceous Shells of Protozoa.

WE regret that circumstances have prevented an earlier reply to Prof. T. D. A. Cockerell's letter published in NATURE of June 28. In our opinion, there can be no question of either convergent or divergent evolution in the case of organisms belonging to separate families as widely separated as *Miliolina* and *Miliammina*. We thought we had made their absence of relationship sufficiently clear by the statement that *Miliammina* was a siliceous isomorph of *Miliolina*, isomorphism being understood among rhizopodists, at any rate, to mean the development of similar structures in unrelated organisms owing to unknown physical or biological conditions.

Even the American school of rhizopodists, who are so addicted to the construction of more or less imaginary 'family trees', would hesitate before suggesting a common ancestor for the two genera. At any rate we should have to go a very long way back, and probably to a theoretical non-testaceous reticularian, to find an organism capable of evolution into such widely differentiated structures, for the porcellanous Miliolidæ have existed since Carboniferous times, and *Silicosigmoilina*, a close relative of *Miliammina*, occurs in the upper Cretaceous.

The difference in the shell structures of the two genera is fundamental. The Milioline shell is normally smooth and porcellanous, but there are many species which habitually incorporate mineral particles to a greater or less extent in the outer coating of their porcellanous tests. Such a Milioline test consists of three distinct layers:

(1) an inner chitinous membrane surrounding the protoplasm;

(2) the normal porcellanous test formed of $CaCO_3$, and

(3) the outer siliceous coating.

The application of even a very weak solution of hydrochloric acid is sufficient to dissolve the middle calcareous layer, and the test is resolved into a little mud in which the chitinous membrane may be detected.

In the tests of the Silicininæ, however, there are but two layers, chitinous and sandy. Immersion in strong acid produces little or no effect.

Chapman evidently regarded his specimens from their external form as *Miliolina oblonga* (Montagu), and, very reasonably assuming that it was a local variety which had assumed the agglutinating habit (not previously recorded in *M. oblonga*), added var. *arenacea*. The fact remains that in the absence of chemical tests they were regarded by him, and by ourselves and others afterwards, as *Miliolina oblonga*, and we maintain that in removing his specimens to a different genus and family, we acted more correctly in conveying the specific name rather than a varietal name, which then became tautological and applicable indifferently to any of the species in the new genus.

We do not attach a great deal of importance to Prof. Cockerell's designation of *Miliammina arenacea* (Chapman) as the type species, but we regret that he should not have communicated with us before publication. Had he done so he would have learned that further investigations since the paper was written had shown that the distribution of Chapman's form was somewhat distinctive, and that it was intended to raise it to specific rank in the *Discovery* Report now in course of preparation. In that report it will appear as *Miliammina arenacea* (Chapman), while the much commoner and more widely distributed species, which is the true genotype, will become *Miliammina oblonga*, H. A. and E. (*non* Chapman).

EDWARD HERON-ALLEN.
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Discovery Investigations,
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Aug. 27.

Transition of Kinetic into Vibrational Energy by Collisions with Particles.

IN the work of Leipunsky and one of us[1] it was shown that dissociation of hydrogen molecules by collision with positive potassium, sodium, and lithium ions occurs only with such energies of the ions that the part of their energy transmitted (according to the energy and momentum law) to the hydrogen molecule is not 4·3 volt (dissociation energy), but 12·4 volt (exciting energy).

Recently one of us has repeated this experiment, using a Pirani gauge, which increased the sensitivity of the method tenfold. The result obtained was the same as that mentioned above. According to the usual point of view, dissociation by collision with a massive particle is a result of transition of kinetic energy into vibrational energy of the atoms of which the molecule consists.

We are thus forced, by the results of the abovementioned work, to admit that such an energy transition occurs, if at all, very seldom. It seems that the inverse process, recombination of hydrogen atoms by a triple collision, also occurs, not indeed at every collision, as well as recombination of bromine atoms, which is, by the by, effected by the addition of nitrogen and oxygen (diatomic molecules, which can receive vibrational energy), and not effected by helium and argon. It follows, therefore, that the transition of vibrational into kinetic energy also is not a process which proceeds readily.

To prove the correctness of the assumption that molecules dissociate by collisions with ions only when previously excited, we have tried to get dissociation of nitrogen by collision with positive ions. The exciting potential and the dissociation energy of nitrogen are very near one another. Accordingly, we have not observed any appreciable difference between the critical energy of the ions and the energy of dissociation of nitrogen. This shows that the results obtained for hydrogen are not experimental errors.