Lucina, Arca, Ostrea, Purpura, Tritonium, Trochus, &c.), all of which are at present living in the neighbouring sea. (2) Sambaquis, consisting almost exclusively of a small bivalve shell, the "Birbigás" of the Brazilians (*Venus flexuosa*?), exceedingly common in shallow bays or salt-water lagoas, the bottom of which is of mixed mud and sand. (3) Sambaquis, consisting exclusively of a species of Corbula, which I have not yet seen in a living state; all the Brazilians also, whom I asked, and who are perfectly acquainted with any edible animal of their marine fauna, are unanimous in affirming that this shell does not live now on our coast. From one of these Corbula-Sambaquis I obtained a specimen of a small Melampus, which I have found living near the mouth of some rivulets, where fresh and salt water are mingling in ever-varying proportions. When the lowlands of the Lower Itajahy and some of its tributaries were as yet beneath the level of the sea, they would have formed a large estuary, and here probably the Corbulæ lived. The fragments of human skulls which we found in one of these Corbula-Sambaquis were of truly astonishing thickness, whereas those I have seen from other Sambaquis are hardly thicker than our own. Among the tools which are to be found in the Sambaquis, stone-axes are by far the most frequent. But as M. Wiener will probably soon publish a full account of his researches, I will now no longer dwell on this subject.

Some time ago I sent to Germany for publication a note on the relation between our Imbauba trees (Cecropia) and the ants which inhabit their hollow stem. As there may be some delay in publishing, I will give you a short abstract. Mr. Belt has already stated that the ants farm scale-insects in the cells of the Imbauba stem, and he believes that their presence must be beneficial. This is no doubt the case; for they protect the young leaves against the leaf-cutting ants (Oecodoma). Now there is a wonderful contrivance by which, as in the case of the "bull's-horn acacia," the attendance of the ants at the right time and place is secured. At the base of each petiole there is a large flat cushion, consisting of most densely-crowded hairs, and within this cushion a large number of small white pear-like or club-shaped bodies (specimens inclosed) are successively developed, which, when ripe, emerge at the surface of the cushion, like asparagus on a bed, and are then greedily gathered by the ants and carried away to the nest. The object of the dense hair-cushion appears to be (1) to secure to the young club-shaped bodies the moisture necessary for their development; and (2) to prevent the ants from gathering the unripe bodies. In most cases it is by honey-secreting glands that the protecting ants are attracted; now Mr. Belt observed ("Nicaragua," p. 225) that the honey-glands on the calyx and young leaves of a Passion-flower were less attractive to the ants than were the scale-insects living on the stems; this would most likely be the case with the Imbauba, and it is probable that the use of the little pear-shaped bodies is to form an attraction stronger than that of the scale-insects, and thus to secure the attendance of the protective ants on the young leaves. As far as I could make out, the club-shaped bodies consist mainly of an albuminous substance. The ant colonies are founded by fertilised females, which may be found frequently in the cells of young Imbauba plants. Each internode has on the outside, near its upper end, a small pit where the wall of the cell is much thinner than anywhere else, and where the female makes a hole by which she enters. Soon after this the hole is completely shut again by a luxuriant excrescence from its margins, and so it remains until about a dozen workers have developed from the eggs of the female, when the hole is opened anew from within by these workers. It would appear that the female ants, living in cells closed all around, must be protected against any enemy; but notwithstanding a rather large number of them are devoured by the grub of a parasitic wasp belonging to the Chalcididæ; Mr. Westwood has observed that the pupæ of the Chalcididæ exhibit a much nearer

approach to the obdected pupæ of the Lepidoptera than is made by any other Hymenoptera" ("Introduct. to the Modern Classif. of Insects," Part XI., p. 162). Now the pupa of the parasite of the Imbauba ant is suspended on the wall of the cell by its posterior extremity just like the chrysalis of a butterfly.

I hope you will have received a paper on *Æglea*, a curious Decapod inhabiting the mountain rivulets of our Serra do Mar. Lately I obtained a large number of specimens of this *Æglea*, and among them a female with eggs in an advanced state of development. Thus I was enabled to satisfy myself that, like so many fresh-water and terrestrial animals, the marine allies of which undergo a transformation, our *Æglea* does not experience any metamorphosis.

FRITZ MÜLLER

Itajahy, St. Catharina, Brazil, Dec. 25, 1875

Prof. Tyndall on Germs

YOUR able correspondent "Inquirer" would hardly blame Horatius for taking his enemies one at a time. May I not, then, claim his indulgence for following, in an extremely humble way, the example of the gallant Roman? He may accept my assurance that during the last five months I have found Dr. Bastian quite enough for me.

Moreover, I do not think it likely that Dr. Sanderson and myself will ever cross swords upon this question. Our relation, I am happy to think, will be one of co-operation, not of antagonism. The experiments on pure infusions, not those on mixtures of solids and liquids, to which "Inquirer" directs my attention (NATURE, vol. vii., p. 180), are, in my opinion, too scanty, and too little in harmony with each other, to bear an inference of any weight. To Dr. Sanderson I prefer leaving the repetition of them, with the full confidence that the ability and candour for which he is so distinguished will lead him to a right result.

In repeating these experiments, it would, I think, be well to bear in mind the remarks of Dr. Roberts (NATURE, vol. vii., p. 302), however unimportant they may seem to Dr. Bastian. I would also suggest the substitution, in boiling, of an oil-bath for the Bunsen burner, and, in sealing, the abandonment of the blow-pipe and the use of the simple spirit-lamp flame.

Experiments on milk and pounded cheese are, it may be observed, at present beside the mark. They shall be subjected in due time to the scrutiny already bestowed upon really liquid infusions. It ought not to be forgotten that the jungle we have entered has been growing umbriferously for the last six years, and it is only bit by bit that the sunlight can be let in upon it.

"Inquirer" may count on my sympathetic readiness to minister, however humbly, to the delight he takes in following "every investigation which tends to the development of science." If he cares to see my infusions, it will give me great pleasure to show them to him. Condensed abstracts only of my investigation have been laid before the Royal Society and the Royal Institution; a fuller account of it will follow by and by. Meanwhile, I hope "Inquirer" will accept the assurance that I have been strict—I might say abject—in my adherence to the conditions prescribed by Dr. Bastian in his books.

JOHN TYNDALL

Heathfield, Feb. 13

[The following letter has been sent us for publication by Prof. Tyndall.—ED.]

PERMETTEZ-MOI de vous dire combien je suis charmé que vous apportiez dans la question de la génération spontanée la grande autorité de votre esprit philosophique et de votre rigueur expérimentale. C'est tout à la fois un honneur pour mes recherches et une vive satisfaction personnelle que les conclusions auxquelles vous êtes arrivé s'accordent si bien avec celles de mes propres travaux, malgré la différence des méthodes que nous avons suivies. Le tour piquant que vous avez su donner à vos expériences les fera pénétrer plus avant que les miennes dans l'esprit de tout lecteur que n'égarent pas les idées *à priori*.

Dans le numéro du 5 février courant du *British Medical Journal* le docteur Bastian accepte sans réserve l'exactitude de toutes les expériences de mon mémoire de 1862 (*Annales de Physique et de Chimie*).

Il accepte également, sans nul doute, les résultats de celles que j'ai publiées en 1863 et en 1872 sur le sang, sur l'urine, sur le jus intérieur des grains de raisin, exposés, dans l'état même où la vie a formé ces liquides complexes, au contact de l'air pur,

privé de ses poussières flottantes. Dès lors je dois appliquer au docteur Bastian ces paroles de mon mémoire de 1862, pages 70 et 71 : "En présence de ces résultats (résultats que je viens de rappeler et qu'accepte le docteur Bastian), un partisan de la génération spontanée veut-il continuer à soutenir ses opinions ? Il le peut encore ; mais alors son raisonnement sera forcément celui-ci : 'Il y a dans l'air, dira-t-il, des particules solides, telles que carbonate de chaux, silice, suie, brins de laine, de coton, féculé . . . et à côté, des corpuscules organisés d'une parfaite ressemblance avec les spores des mucidinées ou avec les kystes des infusoires. Eh bien, je préfère placer l'origine des mucidinées et des infusoires dans les premiers de ces corpuscules, ceux qui sont amorphes, plutôt que dans les seconds.'" L'inconséquence d'un pareil raisonnement ressort d'elle-même et le progrès de mes recherches consiste à y avoir acculé les partisans de l'hétérogénéité. Lisez attentivement l'article précité du docteur Bastian et vous verrez qu'il se résume en effet, dans le raisonnement que je viens de reproduire. Le docteur Bastian me permettra de placer dans sa bouche ces paroles :—"C'est bien vrai, les expériences de M. Pasteur et celles de M. Tyndall m'ont acculé, moi Docteur Bastian, partisan de la génération spontanée, dans cette déclaration. Oui, je préfère recourir sans motif sérieux, à la croyance à une force résidant dans la partie amorphe des poussières en suspension dans l'air plutôt que de la placer cette force dans la partie organisée formée de corpuscules identiques d'aspect à ceux des germes des organismes des infusions." Parler ainsi n'est-ce pas avouer sa défaite ?

Quelles sont donc ces particules amorphes dont vous invoquez si gratuitement l'influence et de quel droit leur attribuez-vous les *primum movens* de la vie ? Pourquoi, si vous aviez raison, ne le trouverait-on pas ce *primum movens* dans les particules amorphes ou organisées qui existent à l'état naturel dans le sang frais, dans l'urine fraîche, dans le jus du raisin, quand on expose ces liquides dans l'air pur ? Voulez-vous que vos particules amorphes, douées du *primum movens* de la vie des infusions, sortent de matières déjà altérées, putrides, etc. . . . mais, pourquoi seraient-elles charicées par l'air sans être accompagnées des germes et des êtres vivants de ces infusions et, s'il en est ainsi, comment ne pas placer le *primum movens* de la vie dans ce qui est vivant, plutôt que dans ce qui n'a rien des caractères apparents de la vie ?

Elle est inattaquable, cette conclusion que j'ai déjà formulée : dans l'état actuel de la science, l'hypothèse de la génération spontanée est une chimère.

Paris le 8 Février, 1876

Votre bien dévoué,

L. PASTEUR

Mr. Sorby on the Evolution of Hæmoglobin

IN the short notice in NATURE (vol. xiii. p. 257) of my paper on the Evolution of Hæmoglobin, in the *Quarterly Microscopical Journal*, it is said that my conclusions are mainly based on a small difference in the wave-length of the absorption-bands of the spectrum of the red blood of *Planorbis*. This is, however, a very small part of the question. The principal results are that hæmatin is first met with in the bile of many pulmoniferous molluscs in an abnormal state, quite unfit to serve the purposes of respiration, but easily changed into the normal, which could, and probably does in some cases, perform that function. Then in the blood of *Planorbis* we have a solution of a hæmoglobin, in which the hæmatin is combined with an albuminous constituent coagulating at the low temperature of 45° C., and finally we come to the normal hæmoglobin existing as red corpuscles, containing an entirely different albuminous constituent, coagulated at about 65° C. In all these changes in the condition of the same fundamental radical, the oxygen carrier becomes of more and more unstable character, and more fitted for the purposes of respiration, as we advance from lower to higher types, as though advantage had been taken of every improvement due to modified chemical or physical constitution.

H. C. SORBY

The Flame of Common Salt

IN answer to a question put by one of your correspondents (p. 287), allow me to state that the origin of the blue flame in question is still involved in mystery. Your correspondent will find everything that is known on the subject in a letter addressed to the editor of the *Philosophical Magazine*, by Prof. J. H. Gladstone (*Phil. Mag.* 1862, vol. xxiv. p. 417).

Prof. Schorlemmer and I are at present engaged in a joint investigation, which we hope will throw some light on the origin

of the flame. We have already obtained interesting results, and observed the flame under circumstances in which it has not been seen before, but we are as yet entirely unable to say what the flame is really due to.

ARTHUR SCHUSTER

Owens College, Manchester, Feb. 12

Science at Hastings

HAD we here a few more men like Mr. Alex. E. Murray, my paper on "Science at Hastings" would never have been written. But I fail to see in what way he has "vindicated the honour of Hastings." With the exception of one or two sentences which require qualification, his letter is simply an emphatic repetition of what I said in the Hastings and St. Leonards News. The substance of my paper may be given in one of its sentences : "With the exception of occasional debates among the members of the Philosophical Society and the few scientific lectures in the winter programme of the Mechanics' Institution, there is in Hastings no public encouragement or aid to science." As to the Philosophical Society, Mr. Murray admits that, "owing to a variety of circumstances, it is not at present quite so flourishing as we could wish." In point of fact, during the session 1874-75, four papers were read and a conversazione held. This Society is the only distinctively scientific one in the town, notwithstanding the "multiplicity" of institutions mentioned by Mr. Murray. Popular scientific lectures are occasionally given in connection with various associations for young men ; and the Mechanics' Institution also has a winter lecture session, but unfortunately the Committee find it very difficult to obtain lecturers, and are fain to eke out their list with musical evenings and readings. The Literary and Scientific Institution has for many years dropped the word "Scientific" from its name, and at present seeks merely to provide for a few of the older inhabitants of the town a quiet reading-room supplied with papers, a few reviews and magazines, and a box from Mudie's. Scarcely a new book has been bought for very many years. The meteorological instruments which the Institution "formerly possessed" were, with the exception of the barometer, broken long ago, and the barometer has since been sold. At one time—twenty years ago—I was in the habit of taking the observations in the absence of the gentleman whose special business it was ; but it must be at least a dozen years since any observations were systematically taken.

In conclusion, I claim to have fully recognised in my paper all that is being done in Hastings in the interests of science, and I sincerely regret that Mr. Murray has not been able to discover any omission on my part. We have no museum, we have no public library in which there are scientific books recent or numerous enough to be of any use to a student, except in a school or two ; we have no Naturalists' or Field Clubs ; with the exception of the Philosophical Society, all the existing institutions in Hastings have practically lost what scientific character they may at one time have possessed ; and the Philosophical Society itself is neither exclusively scientific nor exclusively local in its aims, and is unfortunately "not quite so flourishing" as could be wished.

ARTHUR RANSOM

Hastings, Feb. 5

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR R LEPORIS.—This highly-coloured star, the variability of which was detected by Schmidt in 1855, is calculated to be at a maximum on the 28th of the present month. The mean period appears to be about 438 days, 230 days being occupied in passing from minimum to maximum, and 208 days from maximum to minimum. Probably the irregularities of variation which have been suspected are to be mainly attributed to the difficulty attending comparisons of a star of such intensely red colour. With regard to the colour, however, there is something more than a suspicion that it has sensibly diminished in intensity since attention was first directed to it (Hind, 1845, October). We are almost wholly indebted to Schmidt, who makes such excellent use of the favourable astronomical conditions under which he is placed at Athens, for our knowledge of the law of variation in R Leporis.