those that have made the acquaintance of a book, or for those that have not? For myself, I can answer that I care most for the reviews of those books that I have not seen. In conclusion, I wish to say that Mr. Gage is a stranger to me, and I have never had any sort of communication with him. Whatever one might say in his behalf, my remarks were not made for his benefit, but to point out what I believe to be one of the first duties of the reviewer of a scientific book to his readers. S. T. M.

Lexington, Va., June 13.

[The limited space at our command will not allow of extended analyses of the many text-books of science which are continually appearing. A short notice either of their general merit or demerit is all we can give. In the case of Gage's 'Elements of physics,' the reviewer used the book as a text to preach against the common custom of teachers in using the atomic theory in their explanations as if we knew definitely that atoms exist.]

Solar constant.

Prof. C. A. Young has kindly called my attention to an unintentional oversight in my article entitled 'Solar constant' (SCIENCE, p. 542). In the general equation sent me by him, *t* represents 'degrees of heat,' not 'quantity of heat;' and *m* represents 'time,' not 'unit of time.' H. A. HAZEN.

A zoo-philological problem.

On the New-England coast, where Mya arenaria is abundant, and known as the 'clam,' an annelid which is common in the same localities is called the 'he-clam,' and is believed by many fishermen to be the male of the mollusk.

In Norway, Mya arenaria is abundant in the fiords of the north. It has no economic uses; but its associate, an annelid, the 'pür' (said to be Arenicola piscatorum), is an important bait, and gives its name to the Mya which is called the 'pürschaal.'

Why should the common annelid and the common mollusk be thus associated in popular nomenclature in remote regions? It is interesting to observe that the form possessing commercial value in each instance gives its name to the one which is in lower esteem. G. BROWN GOODE.

The sun's radiation and geological climate.

In my objecting (SCIENCE, p. 395) to the assumption that the dissipation of solar energy from loss of heat diminishes the supply of sun-heat received by the earth, I said, that, so far as there has been any change in the supply, it has been in the direction of an increase, and hence cannot explain the undoubted decrease in the temperature of the earth's atmosphere. I think Professor Le Conte's criticism (SCIENCE, p. 543), taken in its entirety, corroborates my position. He shows that the quantity of heat incident normally on a unit of surface in a unit of time varies as the area of a great circle of the sun × heat-emitting power of each physical point of the sun: hence the quantity emitted would not increase, unless the heat-emitting power increased faster than the square of the temperature. He adds that "some physicists (Rossetti) make the latter proportional to the square of the absolute temperature, while others (Stephan) make it as high as the fourth power." If Rossetti is right, there has been no decrease in the amount of solar heat received; while, if Stephan is right, there has been a very great increase: for, on the assumption that the temperature is inversely as the radius, as stated in Professor Newcomb's article (Popular astronomy, p. 508), the heat-emitting power, if the solar radius is reduced to one-half, will be increased four times, and will just compensate for the great circle being reduced four times in area. If the emissive power increases, as Stephan claims, then a doubled temperature will increase it sixteen times, and, the area being diminished only to one-fourth, the earth will receive quadruple the heat.

It is true that the heat-emitting power of any (solid) body varies according to the area of its surface, providing all the other conditions are unchanged. In case of solids and liquids, very little change can be made in their density by any force that we can apply, — so little, indeed, that no appreciable effect can be produced; but gases are easily affected, and there is no difficulty in conceiving them reduced many times in bulk. Now, suppose two spheres, e.g., of hydrogen, of equal masses and of the same temperature, but one having twice the radius of the other. They will radiate equal amounts in equal times, as I shall try to show. I assume that the radiation goes on only from points of matter, — the atoms of the hydrogen. Conceive each sphere made up of a vast number of concentric layers, each one molecule thick. The number of layers will be the same, and the number of molecules in each will also be the same: consequently the heat-emission of the outside layer will be the same in both spheres. What would be true of the first layer would be true of all, unless the outer one intercepts some of the rays. So far as the outer layer is gaseous and elementary (it is very doubtful whether any chemical compounds can exist in the intense heat of the sun), it is a vacuum to radiant heat; for Professor Tyndall, in 'Heat considered as a mode of motion,' has shown (p. 362) this in reference to oxygen, hydrogen, nitrogen, and air, and, in general (see rest of the lecture), that elementary gases or vapors produce little or no effect upon the radiant heat that passes through them. It must be remembered, too, that the source of heat employed in his experiments was icy-cold in comparison with the sun, and that the penetrating power of heat-rays increases as the temperature of their source rises. It is therefore probable that the heat from the lower layers passes through the upper ones, so far as they are gaseous, with little or no loss, and hence that in gaseous bodies the heat-emitting power for any given temperature is proportional, not to the surface, but to the mass or density.

But suppose that diffused through the upper layers were molecules that were capable of stopping every ray that impinged upon them. Neither the absolute number nor the size of these bodies would be affected by shortening the radius, but only the space between them. If the radius were reduced to one-half, the apertures would be reduced in area to one-fourth, while the radiating molecules within any given distance would be increased eightfold: in other words, the chances of not passing out into space would be increased only four times, while the number of shots would be increased eight times; so that, in this case, the heat-emissive power would be actually increased by the condensation. If to this be added an increase of the same power from the rise of temperature (either as the square or the fourth power, *Rosetti* or *Stephan*), there can, I think, be no doubt that any change which has occurred in the earth's temperature from the sun's losing energy has not been in the direction of growing cooler.

As a corollary of the above, I add, the radiant or heat-emitting power of a sphere of gas appears to be a function of mass and temperature, and not of surface and temperature.

This is of interest in the study of cosmic development. C. B. WARRING.

Poughkeepsie, N.Y., June 16, 1883.

Flight of the flying-fish.

The difficulties in the way of accurate observation of the flying-fish in motion are numerous and real. Seen always from above, usually at a distance which is constantly increasing, and while the observer himself is in rapid motion, it is not strange that such conflicting opinions exist, or that the mode of flight is so often spoken of as a mystery.

During a trip by steamer from New York to Rio Janeiro *viâ* the West Indies and Pará, and on the return trip coming directly from Rio to New York, I watched flying-fish nearly every day, and frequently all day, and satisfied myself on the following points: —

The fish usually leaps clear of the water at once, leaving it commonly at an angle of 45° or less. After leaving the water, no forward impulse whatever is received (except sometimes from the wind) until the water is again touched, when the tail may be used effectively without immersion of the rest of the body. Very soon after leaving the water, yet not instantly, the pectorals are spread, and an instant later the ventrals.

Both sets of fins are kept *quietly* extended so far as any *voluntary* vibration is concerned. Any similar, tensely stretched membrane would quiver more or less when cutting the air at such speed.

Ordinarily the two pectorals lie in about the same plane. They are never carried much below the body, but are frequently lifted considerably above it, especially when going before the wind, at which time the whole fish rolls from side to side, precisely as does a sailing-vessel under similar circumstances. The course may be a simple curve, as it commonly is in calm weather, or it may be undulating, as is usually the case in rough weather or over a heavy swell. I think the ventrals are used to direct the fish up or down, as they certainly work independently of the pectorals, and closing them would naturally drop the tail. Toward the close of the first stretch, and when the fish wishes to re-enter the water, the pectorals are instantly closed, and he shoots head foremost into the water with only a slight splash.

If, on the contrary, he wishes to continue in the air, the long lower lobe of the tail is allowed to drop into the water, and a few vigorous strokes send him upward and forward, sometimes enabling him to clear another hundred feet before repeating the action, which I have seen him do at least seven or eight times before finally entering the water for a fresh start.

Not unfrequently the tail is dropped, seemingly by closing the ventrals, and an undulating motion so obtained, even when there are no waves or swells to be cleared; and, although the tail may not then touch the surface, it looks as if the fish were *feeling* for the water, which I think is really the case. The poetic wetting of the wings in the crest of a wave so as to prolong the flight appears to be a harmless bit of imagination for all but the fish: to him it is disastrous. His tail alone needs wetting; and, when by mistake he takes the top of a wave bodily, it usually topples him over, or at least checks him noticeably. The drying of the wings would be rather favorable than otherwise.

I was not able to detect any voluntary change of direction to right or left while in the air.

Once a large fish rose quite close to us, and started directly toward the steamer. When within a few yards, he suddenly closed his pectorals, plunged into the water, and almost instantly issued again in a nearly opposite direction.

Examination of a Pacific species in alcohol (and I presume the same general structure holds good for the genus) shows that the pectorals are inserted at such an angle with the axis of the body, that, if the body be horizontal and in motion, the air striking on their lower surfaces must tend to raise the fish, although at the expense of a certain amount of forward motion. Evidently, then, any beating of the pectorals would only retard the fish still more, even if it did support him somewhat in the air. The conclusion seems inevitable, however, that the tail alone is the propeller, the other fins acting solely and passively as supporters. WALTER B. BARROWS.

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HEITZMANN'S MICROSCOPICAL MORPHOLOGY.

Microscopical morphology of the animal body in health and disease. By C. HEITZMANN, M.D. New York, *J. H. Vail & Co.*, 1883. 19+849 p. 8°.

DR. HEITZMANN, formerly of Vienna, now of New York, is well known as an unusually good histological draughtsman. Ten years ago he published some investigations on the minute structure of protoplasm. To his own researches on this subject he has long attributed an importance which scientific men of much greater experience and ability have failed to recognize. The present volume, a very well made and beautifully illustrated book, although it comes in the guise of a manual of normal and pathological histology, is obviously intended principally to bring forward the author's own theories, and to insist upon their fundamental character and great value.

The author so openly implies his conviction that he is a neglected grandeur, that he incites the critic to a severity of comment that a tone of modesty more commensurate with the real value of his researches would not have called forth. The general defect of the book is want of judgment on the author's part, and an exaggerated confidence in his own notions. Thus, being unusually skilful with his fingers, he scoffs at microtomes (p. 7), and closes a slurring paragraph upon them with, "The greater the complication, the less is the value of such machines." A man who makes such a statement without any limitation reveals a hopeless lack of comprehension of the indispensable requirements of many branches of histological investigation. The second chapter in the book discusses the general properties of living matter, and contains a number of characteristic loose assertions: for instance, "Life is evidently a peculiar kind of motion of the molecules (plastidules) of living matter, of a relatively short duration" (p. 14). This is