What is biology? Who are biologists? So far as I am able to judge, biology is commonly regarded as a side-show of natural history; and any zoologist and botanist is supposed to be, *ex officio*, a biologist. But biology is an interpretative science, and systematic zoology and botany are purely descriptive. They may furnish valuable evidence, but they do not necessarily do so. The zoologist or botanist trained in observation and description may interpret skilfully, but such skill is not a necessary outcome of his studies. Zoologists and botanists have themselves proclaimed the inadequacy of their evidence by founding the experimental and biometric schools, which began as violently opposed sects, and so continue.

To my thinking, biology is that science which sits at the hub of all the studies concerned with life—zoology, botany, physiology, psychology, medicine, bacteriology, embryology; anatomy, palæontology, sociology, even pedagogy and history—gathers evidence from them all, and deals especially with problems too big or deep for these individual studies, *e.g.* problems of heredity, evolution, development, and the like. If the biologist be controlled by the rules which ordinarily guide scientific procedure—for example, the rule that all verifiable and relevant facts (no matter how, or by whom, or when, or where collected) are equal before science, by the rule that all hypotheses must be crucially tested (*i.e.* so tested by *fresh* and *unlike* facts that every alternative hypothesis is rendered inconceivably as true), and by the rule that a fully established theory must be accepted as true regardless of all preconceptions—then a very splendid future immediately awaits not only biology, but also science in general; for the claim of science to the deciding voice in the settlement of numerous burning problems of immense importance will become irresistible.

By way of demonstrating that I am not vapouring, I shall venture to give one or two examples of evidence ignored and problems neglected by biology "as she is spoke" in a future communication. Meanwhile, there is a little more in Mr. Cunningham's letter with which it is necessary to deal. He says that naturalists would not admit that man, as an animal, is "higher" than an insect. It is pleasant to find him so careful of meanings; but will he please excuse the expression as "technical"? It is in common use and deceives no one. He declares that I give no evidence of the evolution of the power of developing in response to use. Is there any need? A man develops from birth to death mainly in response to this influence; does Mr. Cunningham believe that a butterfly develops in the same way to an equal extent? Consider mind. All learning, thought, intelligence, and reason depend on the growth of the mind through functional activity. Mr. Cunningham has done magnificent work on hormones. Does he think a beetle could learn what he has taught? What is intelligence but a power of developing in response to experience, of growing mentally in response to functional activity? What is stupidity but a "natural" or "acquired" incapacity so to profit? A human infant can learn, but has not learned. A human idiot cannot learn, and has not learned. A normal man can learn, and has learned. Almost all that separates the normal adult mentally from the infant and the idiot develops in response to use. The perfect idiot cannot even learn to walk or to speak. From the human point of view every dog is an imbecile, every cat an idiot, every beetle a perfect idiot. The beetle is more efficient than the human idiot merely because he is more completely equipped with instincts and instinctive actions, which, unlike human habits, habitual actions, and the rest, do not develop through use.

For example, the beetle does not learn to use his limbs. Does not the difference between man and the beetle indicate an evolution of the power of developing in response to use? What more evidence does Mr. Cunningham want?

G. ARCHDALL REID.

9 Victoria Road South, Southsea, April 23.

The "Flight" of Flying-fish.

IN NATURE of April 21 Prof. Wood-Jones presents some interesting observations on the "flight" of flying-fish made from an especially favourable vantage-point—the overhanging bow-sheaves of a cable ship.

While crossing the Gulf of Mexico on various occasions I made some observations on the same subject with the aid of powerful binoculars (Goerz prismatic, magnifying 12 diameters). With these I had been used to following birds in flight, and with a little practice found that I could keep flying-fish under continuous observation during their passage through the air.

I can confirm Prof. Wood-Jones's account in the following important particulars:—

(1) The initial impulse is always given by rapid lateral strokes of the tail as the fish leaves the water. Since the lower lobe of the caudal fin is elongated, the fish can continue to propel itself in this manner for some time while the whole of its body is out of the water. On very calm days the moving lower lobe of the tail leaves a track in the water in the form of an interrupted line. Presumably the interruptions represent the times of violent lateral motion. The uninterrupted sections of line are each 2–3 in. long, the interruptions rather longer, the whole line often continuing for 5–6 ft. After this, of course, the fish rises wholly into the air.

(2) The fish may regain impetus by again vibrating its tail when it has dropped far enough for the lower tail-lobe to be once more in the water. Fresh impetus may be gained in this way once, twice, or even three times in a flight without the body ever touching the water.

(3) The pectoral fins are usually held stiffly out, as Prof. Wood-Jones states, and act as planes. I have, however, on several occasions seen rapid vibration of the pectoral fins for a short period; but whether this was actual "flight," as I at the time supposed, or whether it was due, as Prof. Wood-Jones suggests, to a passive vibration caused by the air meeting the fin at a certain angle, I am unable to say. The impression made upon me at the time was that the *normal* means of propulsion in air was the tail, but that the pectoral fins *could* be used as supplementary flying organs on occasion. Of the truth of the first part of this impression I have no doubt whatever; Prof. Wood-Jones's anatomical studies make me doubt the second part. However, a good binocular in the hands of anyone trained to field observation will put the matter to the test.

JULIAN S. HUXLEY.

New College, Oxford, April 25.

The Concept of "Space" in Physics.

PROF. EDDINGTON (NATURE, April 14, p. 201) expresses well the properties that a substratum of matter, light, and electric force should have, and the reasons for combining space and æther, the two different, but always co-existing, substrata of the older physics, into one. What is not clear is why he stops there. The ancient rule, "Entities are not to be multiplied beyond necessity," is as applicable now as ever. If a physical æther is to be postulated, it is for those who advocate it to show their reason for doing so,

not for those who doubt it to offer arguments against it. So far as I can see, neither the old æther nor the new is more than a metaphysical concept of no utility, either in understanding natural phenomena or in predicting new ones, and accordingly neither forms part of the subject-matter of physics. In Prof. Eddington's own development of the theory he never makes any use of this concept. What he assumes is that physical laws can be expressed by differential equations with a certain mathematical property, and the whole of the verifiable results are deduced from this; but this assumption was chosen, not because it corresponded to any known property of space-time or æther, but because mathematically it was the simplest possible. The theory is not based on the concept of space-time, but on an unstated relation between physical laws and mathematical simplicity. Reasons why such an assumption is needed in any theory of scientific knowledge are given in a forthcoming paper by Dr. Wrinch and myself, and are independent of any views on the ultimate nature of the world, except that quantitative inference is possible.

Again, I must dissent from the statement of the interrelation of experimental geometry and mechanics. The essential feature of geometry, as the term is used by geometers, is that it is purely logical and not experimental. Consequently, "experimental geometry" is a contradiction in terms, and can neither have an outcome nor be one. The subject-matter of the mechanics of the world is the relations between the measured positions of bodies at different measured times; all the concepts involved in this statement are well-known physical magnitudes, and I see no use in trying to redefine them in terms of others that are either totally hypothetical or, at best, less comprehensible than those already in existence.

HAROLD JEFFREYS.

Meteorological Office, South Kensington,
S.W.7.

The Origin of "Churning at 62°" on Dairy Thermometers.

Will you permit me through NATURE to ask the following question: Why do the makers of floating dairy thermometers, both in the United Kingdom and in the United States, so mark their thermometers that 62° F. is said to be "churning temperature," when dairying experts in both countries are in agreement that it should be taken as 56° F.?

I recently had occasion to make myself familiar with the agricultural literature published in this country between 1831 and 1855, and found that where churning temperatures are given it is stated to be 50° to 55° F., and in doing so reference is usually made to experiments carried out by the Highland Society of Scotland in 1828 on the best temperature for churning butter. Various American authorities in dairying have commented on this curious marking of dairy thermometers, and have come to the conclusion that it is a "mystery" how dairy thermometer-makers arrived at the figure 62° F., and why they persist in recording on the thermometers they are making to-day that 62° F. is "churning temperature."

Perhaps some of your readers may be able to throw light on this "mystery."

R. HEDGER WALLACE.

April 12.

A FIRM of manufacturers of thermometers, Messrs. Pastorelli and Rapkin, Ltd., to which we submitted Mr. Hedger Wallace's inquiry, informs us that though
NO. 2687, VOL. 107]

they have supplied tens of thousands of dairy thermometers in recent years, they do not know the origin of the mark "churning at 62°," and no one has ever suggested to them before that this temperature is incorrect. Dr. W. Goodwin, principal of the Midland Agricultural and Dairy College, has favoured us with the following opinion upon the subject:

"I do not know that marking dairy thermometers with a churning temperature of 62° is such a common practice as Mr. Hedger Wallace indicates. Many such thermometers are just marked with the degrees only, and these are what we always recommend for our students. It is quite impossible to fix a churning temperature owing to the large number of factors which have to be taken into account. For example, thickness of cream, the degree of ripeness of the cream, the temperature prevailing at the time, the breed of the cow, and even such other factors as feeding and period of lactation, come into consideration. I agree with Mr. Hedger Wallace that 62° is generally too high, unless the churning is taking place in very cold weather, and I venture as an explanation that possibly this old custom dates back to the time when whole milk was churned, as this necessitates a higher temperature than in the case of separated or skimmed cream. It would be of interest to find how the churning temperature of 62° has arisen, but I can think of no justification for it. Probably on some popular make of thermometer this point was fixed, and has been blindly copied ever since."—ED. NATURE.

Young's Interference Experiment.

YOUNG'S interference experiment is a very difficult one to perform as he describes it. If slits are used for the apertures it requires a distance of two yards from the first slit to the double slit, and two yards from the double slit to the observer, and also a very bright source, the sun or the crater of the electric arc. For this reason the experiment is seldom performed, Fresnel's biprism or mirrors being substituted for it in laboratory courses.

If, however, the double slit is mounted on the table of a spectrometer the experiment can easily be performed with an electric incandescent lamp or a sodium flame, and the bands are considerably brighter than with the other arrangement, though not so bright as the bands produced with a biprism. The double slit can be made by painting a piece of glass dead-black and then drawing two parallel scratches on it with the point of a penknife. If the scratches are six-tenths of a millimetre apart, a natural distance to draw them, the separation of the successive bands is about three minutes in the field of the telescope, and ten or twelve bands can be counted. This method of performing the experiment is not mentioned in the textbooks, and so it appears worth while to direct attention to it here.

It should also be stated that the diffraction bands produced by a straight edge are undoubtedly more easily observed with a spectrometer than with the expensive optical benches sold for the purpose. The diffracting edge—the blade of a penknife, for example—is mounted vertically on the prism table, and the telescope object-glass removed. The bands are then seen in the field, the distance between the first two maxima being about four minutes with a spectrometer of average size. In the formula for their position the a becomes infinite and cancels, and the angular distance from the edge of the geometrical shadow is $\sqrt{(2n-1)\lambda/b}$.

R. A. HOUSTOUN.

University of Glasgow, April 16.