he regards as quite well-marked "acidosis." He defines acidosis, not as a state in which the hydrogen-ion concentration of the blood and tissues is abnormally increased, but as one in which the reserve of fixed alkali in the body is depleted. This depletion is shown by the fact that, whereas in a normal individual it only requires a small dose of sodium bicarbonate to make the urine alkaline, it requires a very large dose in the case of a patient suffering from acidosis.

The evidence which Dr. Sellards adduces to illustrate the soundness of this test for acidosis is very interesting and, in the main, new. Much of this evidence is from observations which he made in the Philippines in treating kidney inflammation in cholera. Acidosis may be due to abnormal flooding of the body with organic acids, as in diabetic coma, or to failure on the part of the kidneys to excrete acid, as in kidney inflammation. In either case the test proposed by Dr. Sellards indicates the condition. But the whole subject becomes much clearer if it is recognised that in serious acidosis there is an actual increase in the hydrogen-ion concentration of the blood, and that of this increase the increased breathing is by far the most direct sign. It appears to be the untrustworthiness of direct means of measuring small differences in hydrogen-ion concentration that has led some writers in this country to deny that dangerous acidosis exists at all in such conditions as diabetic coma or kidney disease, although both the greatly increased breathing and the favourable effect on the symptoms of large doses of sodium bicarbonate indicate the existence of what is, physiologically speaking, an extreme state of acidosis.

The confusion is rendered still greater by failure to distinguish primary acidosis due to flooding of the body with acid from the secondary acidosis which is an adaptive physiological response to lack of oxygen, and leads to increased ventilation of the lungs and, consequently, increased supply of oxygen to the blood. This form of acidosis is met with typically at high altitudes and in various conditions, such as poisoning by irritant gases, where the free supply of oxygen to the body is interfered with. Here the acidosis is brought about by diminution in the amount of fixed alkali or "alkaline reserve" in the blood, with the result that the breathing is increased. There is no directly measurable increase in hydrogen-ion concentration of the blood, nor, in view of what has already been said, could this be expected; but the increased breathing is, nevertheless, good evidence of the existence of an increase. The diminution in alkaline reserve in the blood is easily detected, either by titration or by a diminution in the capacity of the blood for combining with CO_2 ; and the latter method, particularly in the form given to it by Van Slyke, has been extensively used during the last year or two. The acidosis detected in this indirect way has, however, frequently been interpreted as in itself a symptom to be combated by alkalis, when it is in reality an advantageous compensatory reaction. For-

tunately, the body can usually dispose of even large quantities of alkaline medicaments administered through this misunderstanding. Dr. Sellards's test for acidosis would exclude a compensatory acidosis, but would also lead to a compensatory acidosis being missed, and perhaps, as a consequence, to the cause of the compensatory acidosis being overlooked.

To interpret the significance of increased breathing, diminished alkaline reserve in the blood, or any of the other symptoms which usually accompany acidosis, it is necessary to take the whole of any existing abnormal physiological conditions into account, just as in interpreting the significance of any one of the usual physical signs of disease it is necessary to consider the whole of the symptoms and their course. The confusion which exists at present on the subject of acidosis is largely due to neglect of this principle. Perhaps this confusion is rendered most evident by the quite recent discovery by Yandell Henderson that when an animal is dying of "alkalosis," produced by withdrawing carbonic acid from the body by excessive artificial respiration, the alkaline reserve in the blood is greatly diminished in a physiological effort of the body to preserve the normal reaction; and, conversely, that when an animal is suffering from extreme acidosis owing to a great excess of CO_2 in the inspired air, the alkaline reserve of the blood is greatly increased in a corresponding compensatory effort. A diminution in alkaline reserve of the blood is thus not by itself a certain index of acidosis, or of whether or not an acidosis, if it exists, is harmful.

J. S. HALDANE.

TROPISMS.

Forced Movements, Tropisms and Animal Conduct. By Prof. Jacques Loeb. Pp. 209. (Philadelphia and London: J. B. Lippincott Co., 1918.) Price 10s. 6d. net.

A PROLIFIC investigator does a great service to his brethren when, without waiting to write an elaborate treatise, he collects the gist of some considerable portion of his work into a book; and if the book be a small one, so much the better. This Prof. Loeb has now done, and we are immensely obliged to him. What is more, his volume is but the first of a series, by American writers, all dealing with the wide field of experimental biology, a field in which we at home have done comparatively little, but in which American biologists have greatly distinguished themselves. Among the promised monographs are one by Prof. Morgan on "Chromosomes and Heredity"; another, by Dr. Jennings, on "Pure Line Inheritance"; a third, by Dr. T. B. Robertson, on "The Chemical Basis of Growth"; and a fourth, by Prof. Osterhout, on "Permeability and Conductivity of Living Tissues." In every case (and there are many more besides these) the author has won, and more than won, his right to be heard, and in every case also we feel the need of an authoritative guide to the subject in question.

Prof. Loeb is an out-and-out "mechanist," as we all know. He is impatient (as he tells us in the first words of his preface) with "the attempts of vitalists to show the inadequacy of physical laws for the explanation of life." He insists on the "quantitative methods of the physicist," and will have nothing to say to the romantic, or anthropomorphic, tales of "scientific popularism." His book begins with an account of the symmetry of the organism, as the starting-point for his theories of conduct; that is to say, he finds in the existence of bodily symmetry, whether radial or bilateral, a simplification of all further analysis. The symmetry is a dynamical as well as a morphological one; there is a symmetry which corresponds with the impulses from without, as to the reactions from within; muscular system and nervous system have their own corresponding symmetries, and the whole problem of action and reaction is simplified thereby. But it does not follow that all animals are symmetrical—at least, in this simple fashion. The spiral *Euglena*, for instance, is a harder case; and the experimentalist may convert the symmetrical into an unsymmetrical animal, as when he destroys one hemisphere of a dog's brain, or makes a beetle blind of one eye. The "reflex actions" of the physiologist are the reactions of parts, or isolated segments; a similar or analogous reaction of the whole is likewise assumed to be, or may be interpreted as, a phenomenon of a purely physico-chemical nature. And these reactions of the whole organism are what Prof. Loeb calls "tropisms."

Through such reactions, or tropisms, Prof. Loeb leads us, in connection with the various stimuli of light, heat, electricity, chemical action, gravity, contact, and so forth, and at last comes face to face with the more recondite themes of instinct and memory. Let us consider a single experiment out of the great number which this small book relates. Many animals, very humble ones included, tend to move towards the light, while some, on the other hand, retire into the darkness or the shade; some love the light, and some hate and avoid it, as we are apt, in our anthropomorphic fashion, to say. Suppose, now, that we illuminate the two eyes of a fly by separate beams of light, of equal intensity and similarly directed. The fly will not choose between the two lights, says Prof. Loeb, as a belated traveller might choose between the lights of two village inns; it will do something much simpler, and apparently more mechanical. It will travel straight along a line perpendicular to that which joins the two lights; it will follow the resultant of the two stimuli. Not only so, but if we alter the direction of the beams, or cause them to differ in intensity, so that in either case one eye receives more illumination, more "phototropic stimulus," than the other, then the creature will move along a perfectly definite line, which can still be simply calculated as the resultant of the two forces involved. The experiment is an ingenious and an elegant one, and, without for a moment doubting the results which Prof. Loeb and his pupils have repeatedly

obtained, we can honestly say that we should dearly like to see it performed.

We dare not attempt to discuss the great philosophic questions that are involved in the whole course of these experiments. We have a notion that "anthropomorphism" is not got rid of quite so easily, however much we change our phraseology, as Prof. Loeb would have us believe, and that, great as are the lessons of mechanics, they do not tell the whole story, after all. Be that as it may, the element of precision, the quantitative element, the strict, experimental method, is conspicuous in Prof. Loeb's work, and our knowledge is manifestly increased thereby. I think it was Liebig who said, in one of his letters to Faraday, that (in those days) a man might be an eminent geologist in England who knew nothing of physics, nothing of chemistry, nothing even of mineralogy. Change the wording, and the biologist may (or once upon a time might) have begun to feel uneasy. It is something to be taught or reminded by Prof. Loeb, and by the whole brotherhood of experimental biologists, that the naturalist cannot live alone, but works in a field inextricably connected, for better for worse, with the whole range of the physical sciences.

Prof. Loeb has a boundless wealth of ideas. In this book and in his other books and papers we seem to see them tumbling one over another. He has enough and to spare for all his pupils and fellow-workers, so that all who come to him may eat and be filled. Moreover, his manifold experiments all have the hall-mark of simplicity, and this is surely one of the greatest things that can be said of any experimenter. There is no parade of elaborate apparatus, nor does it ever seem to be required. *Simplex sigillum veri!*

The book concludes with a bibliographical list of nearly six hundred titles—a catalogue of books and papers on experimental biology, in the sense in which Prof. Loeb himself deals with it. In the first hundred and fifty titles (and I have gone no further in my analysis) sixty-three are German, forty-three American, thirty-eight French, and four more are Dutch or Italian. I shrink from doing the addition and subtraction which would reveal our British share.

D'ARCY W. THOMPSON.

OUR BOOKSHELF.

The Strawberry in North America. History, Origin, Botany, and Breeding. By Prof. S. W. Fletcher. Pp. xiv + 234. (New York: The Macmillan Co.; London: Macmillan and Co., Ltd., 1917.) Price 8s. net.

WITH marked success Prof. Fletcher has gathered into a comprehensive survey much information of great interest in the history and development of the strawberry. Though the book is primarily written for American readers, it appeals to the English gardener and student of horticulture, as English varieties and methods of cultivation had a strong influence on the improvement of the fruit in North America. Garden cultivation began