not set aside for all time something which is not stable, something which is changing as the seasons change. It would be as sensible as to suggest making a park preserve out of a particularly brilliant stand of oaks because of their fall coloring as to propose such a reservation of a growing forest. Both are passing conditions. One is more transitory than the other, but the principle is the same.[1]

Of course individual trees grow old and die. So do human beings, but should they all be poisoned or asphyxiated as soon as they are mature? Is it not one of the basic facts of forest science and one that we might expect every one in the Forest Service, even if only one of the wood chopping engineers, to be perfectly familiar with, that a forest under natural conditions maintains itself, as the population of a human community does, the trees being of all ages, the younger individuals that have been growing up taking the place of the aged ones that die off from time to time? A forest may and often does maintain itself unimpaired century after century. If this were not so, why was a large part of this country covered with magnificent forest with trees several to many centuries old when the first settlers came? Can we doubt that it would still be so but for human interference?

And as for the individual trees, is it not true that most of our native timber trees are long-lived, able to live and grow in a flourishing condition for 200 to 300 years, some of them very much longer? No more pernicious nonsense can be disseminated than the idea that if we do not hurry up and cut the rest of our dwindling supply of timber the forests are going to fall down and rot like a crop of weeds. Were cutting down what our forests need, it would seem as though they had been getting it in plenty for nearly three centuries.

We shall never get any real conservation in this country until people wake up to a realization of how the tentacles of commercial interests have penetrated, not only the branches of our government, but also most of the conservation organizations.

WILLARD G. VAN NAME

NEW YORK

[1] *American Forests and Forest Life*, February, 1925, p. 71.

THE NEW SECRETARY OF AGRICULTURE AS A SUPPORTER OF SCIENTIFIC RESEARCH

GREAT satisfaction and pleasure have been manifested over the appointment of President W. M. Jardine to the position of Secretary of Agriculture. During the two weeks intervening between the time of the announcement of his appointment and the induction into office (fortunately for his welfare it was no longer) he has been hailed and extolled as the "cow puncher" who has come on up, farmer, educator, economist, level-headed citizen, Rotarian, golfer and all with approximately correct estimation and the warmest sincerity.

However, several of the most excellent, perhaps some of the most significant characteristics of the new Secretary of Agriculture have not appeared to receive the public notice that they deserved. One of these is his attitude towards serious scientific endeavor. During the thirteen years that he has been director of the Kansas Agricultural Experiment Station and then president of the college, science, even just for science's sake, has hardly had anywhere a more keenly open-minded and generous supporter. Whether the project called for the investigation of the fauna of the alimentary tracts of termites or the physics involved in a musical tone, if there was prospect of the exercise of energy and integrity in the prosecution of it, his support was enlisted. On the other hand, if the investigation gave promise of valuable economic results, either immediately, or remotely, it was not thereby "tainted" as science. An investigator might superintend the economically important rodent project calling for end results as rapidly as they could be secured, and, at the same time, diligently seek the causes of the absorption, during sexual activity, of the pubic symphyses of female pocket gophers.

The secretary, although most responsive to wholesome public sentiment, has not been deluded by that harmful myth to the effect that farmers, boards, governors and others are constantly bringing desperate pressure to bear on state institutions to secure exclusively results that are capable of immediate practical application, thus enforcing superficiality. Only this year he was able to say that difficulties of this nature had not been imposed upon him.

ROBERT K. NABOURS

SCIENTIFIC BOOKS

Principles of General Physiology. BY SIR WILLIAM MADDOCK BAYLISS, 4th Edition, 1924, Longmans, Green & Company, London.

THIS edition of a book unique among all its kind appears just a decade after the first, and only a few months after the author's death. The tautologous title is still retained and is still somewhat misleading. For treatises on the principles of any science heretofore have led us to look for a style almost stereotyped. One recalls Newton's "Principia," v.

Haller's "Elementa Physiologiae," the "Treatise on Natural Philosophy" by T and T′, or even a book as modern as Tigerstedt's late edition of his "Physiologie des Kreislaufs," in all of which the style is objective to the nth degree and above all utterly free from any concern for the frailties of poor human nature. Upon opening this book, however, one finds at first glance a work that is almost journalistic.

The volume, an octavo, contains some 750 pages that, to the regret of all presbyopes and hyperopes, are made up of paragraphs of fine print alternating with paragraphs of still finer print; but interspersed throughout are so many full-page portraits, photogravures of things and places, reproduced kymograph records, diagrams and graphs that an additional ten pages are required in which merely to list them. Throughout the text the names and titles of so many authors and their works are referred to that another 106 pages (names in heavy-face type) are required for a special bibliography. A general index of twenty-five pages and a topical tabulation of contents covering eight pages, together with the special indices just mentioned, make the book one of ready reference for all it holds.

As one peruses the text, not the chapter titles, here about how best to varnish smoked paper, there about how best to smooth a curve of plotted results together, with a sympathetic expression of regret that physiologists must also know mathematics, followed by a friendly introduction into the subject, a definition of Cartesian coordinates, full-page portraits of two mathematicians and the birthplace of one of them; and, again, here a quotation from Virgil on crop rotation and there a biassed attack on Rowntree's "Poverty, a Study in Town Life," one feels however far removed all this may be from general physiology it nevertheless may be condoned as a method to maintain the interest of the jaded reader, who willy-nilly once he opens the covers, cardinal red in this case, must read on and on.

Of the total twenty-four chapters eight are on topics in physical chemistry and of those on nutrition, secretion, digestion, nervous system, respiration and circulation (seven in all) much, if not most, of the matter belongs to the field of special physiology. On the chapters proper to the field of general physiology those on "Catalysis and Enzymes," "Excitation and Inhibition" and "Tonus" are probably the best, although whatever any of them has failed to include or whatever bias of view anywhere is taken, the mass of material collected, marshaled into place and brought under review by the author is at all times a cause for wonder.

Throughout the text there is a certain unevenness of style. Whole pages here and there may be made up of faultless exposition all highly illuminating which finally trail off into a mere concatenation of authors' abstracts. Reading the chapters thus has the effect of kite-flying, the string of abstracts being tails that sometimes help and sometimes hinder the flight of the kite. The constant citation of names and dates indicates an earnest desire to give the prior author credit in mention of sources. But now and then one wonders how our author failed to do so. In many of these cases there is the understandable, if not always pardonable prejudice, in favor (a) of British investigators and (b) especially of all those whose work was done at the author's institution, University College, London. Seeming conflict even of these prejudices at times is in evidence, as on page 43.

Here the argument is developed to show how in some physiological processes it may be futile to apply the method of temperature coefficients as a touchstone for the presence of hidden chemical reaction. The argument is well taken, but the examples given to show how the method has failed are poorly chosen. One of these is an observation on the influence of temperature upon the rate of the isolated mammalian heart contained in work done at University College.

Now it happens that H. Newell Martin, a Britisher in an American laboratory, was the first not only to develop a method for maintaining the beat in an isolated mammalian heart, but, together with Applegarth, was also the first to observe that the rate of beat is a linear function of the temperature. Our author wishes to make the point that since the velocity of chemical reactions is a logarithmic function of temperature, and the rate of the heart beat a linear function, the temperature coefficient is no index of underlying chemical reaction leading to the inner stimulus to beat. That evidence exists, showing that the rate of the mammalian heart is a logarithmic function of temperature, the author does not state. Now since nowhere else in the book is this matter again taken up it appears that the author wished to keep the reader in ignorance of this newer and conflicting evidence.[1]

As another example of the futility of the method in question the case of the nervous impulse is taken up, and in support articles by British authors again are quoted to the exclusion of prior American work, a priority that the investigators quoted also ignored. Indeed so attached was Sir William Bayliss to the work of his countrymen that he does not see the fallacies in the experimentation and in the logic of

[1] See an article by the reviewer in *Zeitschr. f. allgem. Physiologie*, 1913, 15: 72, especially p. 82.

one of the articles quoted at this point. In this latter article the nervous impulse is compared to the propagation of the burning of gunpowder in a fuse, and experiments are reported in which the velocity of this propagation of burning gunpowder was observed at so-called different temperatures. No considerable difference in the velocity of the propagation of the combustion was observed. The conclusion was drawn that, since in this case chemical action was known to be taking place and yet changing the external temperature did not affect the velocity of propagation, it would be quite fallacious to use the converse case of a system whose internal action was unknown and to argue that because the effect of changing its external temperature was what one expected of a chemical action, therefore, the internal action of the unknown process must be chemical.

The answer to this argument is that it all depends upon whether the changes of temperature outside the system in the two cases actually changed the temperature of the reacting bodies inside the system. In the case of the nerve trunk no physiologist would doubt that experiments, properly planned and carried out, would effect a change of internal temperature, or that a change from 10° C. to 20° C. of the external surface of the nerve trunk would represent also a change of temperature of the same magnitude of the functional structures to within say one per cent. To physicists and chemists this would be all the more certain if they knew that the inner reaction progressed with little or no heat exchanges of its own.

Now the nervous action in its propagation is known to be accompanied by no measurable amount of heat changes.[2] On the contrary, the combustion of black gunpowder, for example, so-called British service powder, is observed to have a heat of combustion of 714 thermal units per gram, and the rise of temperature caused by its explosion is given as 2,200° C.[3] Indeed the temperature of burning powder may be calculated from the known specific heats and the observed heat of combustion of the powder. The calculated value of the latter, namely, 660 thermal units, is somewhat less than the observed value quoted above. The specific heats given for the reacting substances in no one case is greater than 0.3. The lowest figure therefore would be $\frac{660}{0.3}$ or 2,200° C. Now when packed in a heat impervious jacket with particles in close contact with one another, as is the case in such fuses, one sees at once that changing the temperature external to the jacket from 1° C. to 100° C., as was done in the experiment quoted, would have practically no effect upon the reacting system within. For the chemical reaction when once started would raise the internal temperature of the system at that point from 1,000 to 2,000 times above that of contiguous points and of the external environment. How much a burning front of 2,200° C. would be accelerated or retarded in its march by powder lying in its path at 100° or 0° C. may be seen at once by most of us, and perhaps may be calculated to a nicety by one familiar with the laws of cooling bodies.

It is inferred beforehand that the effect would be very slight indeed, as the author quoted found it to be. But it is equally certain that this author did not obtain the proper data upon which to base a calculation even for an approximate temperature coefficient of the propagation of powder combustion. Such data could only have been obtained had he varied the temperature of the powder say some 100 per cent. on either side of that of the combustion temperature, that is, from 0° to 4,000° C. For it is thus that the data for calculating a temperature coefficient of the velocity of the impulse of the frog's nerve is obtained. The nerve functions normally at room temperature, say 18° C., but in the experiment the nerve is cooled nearly to freezing and warmed as high as 33° or 36° C. This establishes a variation of about 100 per cent. to either side of the middle point. We are reasonably certain that the observed temperature is the temperature of the substance where the nerve action takes place, and the changed velocities observed are beyond all doubt the direct result of the changed temperatures. What is more, this changed velocity can be shown to be a logarithmic function of the temperature. On the other hand, it is clear that the gunpowder-fuse experiment failed to yield the data it was intended to yield and that the conclusion based upon the erroneous data that the experiment did yield is completely fallacious.

This page of the book in review is only one, but happens to deal with a subject of which the reviewer has expert knowledge. The query naturally arises, if all the pages were reviewed by experts on the subjects under discussion, would all be found to contain so many errors of judgment? Most probably not all, but if only one tenth of them were found so full of error one still would like to ask whether it is for this reason the book is called, as it is said to be in London, "a text-book for professors." The reviewer is rather inclined to say of the book what a distinguished colleague said when its first edition appeared, "It is a book of love."

Sir William Bayliss put into this book all the things that happened both to please and interest him,

[2] *Vide inter al.*, Professor Max Cremer, 1896.

[3] See Lewes and Brame, "Service Chemistry," London, 1913, pp. 250 and 315.

and his interests were deep and burning. He loved great works of science and made idols of the men who created them. The many full-page portraits in this volume so caressingly displayed, the exquisite care given in that spacious but ill-balanced bibliography, the excessive citation of the works of the very great and of many lesser lights who happened to win his favor, all attest this intense interest in the persons of science. He loved his countrymen, but the men of his college more; he read much and labored long; now and then he reflected on the manifold speculations concerning human life.

All this he has put into this remarkable book. Its title changed to "The Adventures of a Scientist," however inadequate, would have been a more descriptive title. For no treatise on the principles of a science ever turned out to be such a palpably human document.

Paraphrasing the words of the chief editor, the book must indeed now stand as a monument to the memory of Sir William Maddock Bayliss, and as a witness to the affection of his friends. But let the stranger outside the gate use as caption to every page the motto that adorns the title page,

"πάντα δοκῖμά ζετε
το καλόν κατέχετε"

which translated as Sir William expounds in his original preface reads: Prove all things, hold fast to that which is good, and beautiful, and therefore, even as Plato taught, of a necessity also true.

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CANCER IN PLANTS AND IN MAN

RECENTLY in *Klinisches Wochenschrift* (3 Jahr., Nr. 25) and in *Zeits. für Krebsforschung* (21 Bd., 5 Heft.) Dr. Ferdinand Blumenthal, director for some years of the Universitäts Krebsforschung Laboratories, connected with the great Charity Hospital, Luisenstrasse, Berlin, with two assistants, Dr. Hans Auler and Fraulein Paula Meyer, claim to have isolated several times from human breast carcinoma schizomycetes in pure culture with which they have been able to produce malignant transplantable tumors in white rats (carcinomas and sarcomas). One form of this organism, that used most successfully, they state to be indistinguishable from *Bacterium tumefaciens* isolated from plant tumors and studied for many years in the U. S. Department of Agriculture by Erwin F. Smith and his colleagues. With this form, known as *PM*, they have also produced repeatedly, in the hothouse and out of doors, tumors on plants indistinguishable from crown galls.

The rat tumors are now in the eleventh and twelfth generation of transplants. The rat tumor is often a mixed tumor, but sometimes it seems to be a diffuse carcinoma and at other times it looks like a pure sarcoma. I have slides showing this wide difference made from two gland metastases taken from the same rat, one day apart. The tumor metastasizes freely into the glands, the mediastinum, the lungs, etc., but, unlike common rat sarcoma, has not been found in the kidneys. So far the best results have been obtained with *PM* and *L*, both cultures isolated from breast carcinomas. Various other isolations have been made from malignant human tumors (carcinomas, sarcomas and epitheliomas) to the number of sixteen, but for want of assistants and experimental animals all have not been tested on animals. The two strains mentioned (*PM* and *L*) are the most like the crown-gall organism, especially *PM* (the first one isolated, two years ago) which has given remarkable results in both plants and animals, many of which I have seen. It should be noted, however, that isolations are not easy, and that only a small proportion of the inoculated rats have given persistent, transplantable, metastasizing tumors. In the greater number of rats (something not surprising) the tumors receded. It is also to be noted that the bacteria have not been recovered from the transplants.

At the great Actien-Gesellschaft Serumwerk, in Dresden, which I have also visited, the Berlin experiments have been repeated with the same results, *i.e.* (1) They have obtained in white rats numerous good, freely metastasizing tumors (I saw a dozen or more dissected rats and also live ones) by successive transplants beginning with a transplant tumor-bearing rat received a year ago from Dr. Blumenthal. This proves nothing, of course, as to the etiology of the tumor. But (2) they have also obtained two transplantable freely metastasizing tumors, now in the fifth and sixth generation, starting from pure-culture inoculations of *PM* to which was added Kieselgur (sterile diatomaceous earth), but no oedematous cancer serum or cancer juice, such as Dr. Blumenthal used, but which used alone or with Kieselgur, he says, did not cause any transplantable tumors. In the Dresden experiments, as in Berlin, only a small proportion of the bacterially inoculated rats gave persistent metastasizing tumors (two out of fifty), but it is the same type of tumor.

It begins to look, contrary to my belief hitherto, as if *Bacterium tumefaciens* might occur frequently on or in man and be the cause of some of his cancers.

I came here with great scepticism, but I have seen enough, here and in Dresden, to lead me to think that the Berlin experiments should be repeated, as now undoubtedly they will be, in many other cancer lab-