also one anemone as the central figure sitting on the top of the shell—containing the hermit-crab—with its column extending high above the shell and crab.

When these hermit-crabs with their associated anemones are caught fresh in the trawl it may be observed that although the anemones come up closed in no case are they sitting on the shell as is shown in the well-known figures; on the contrary, whether there is only one or as many as three anemones on a shell they are all found to be either hanging from the shell with the disc region towards the ground or are straining their bodies to reach their discs over the side of the shell towards the ground (see Fig. 1). In order to obtain more information, a collection of fresh hermit-crabs and anemones was obtained by trawling in September 1920 and a few experiments made in a tank. The hermit-crabs and anemones were isolated and kept without food for a few days, in the course of which most of the anemones closed. At 11.30 A.M., September 22, the crabs, carrying altogether 18 anemones, were fed with cockles and queens. At 12.5 P.M. all anemones were open with their discs and tentacles spread flat out on the bottom of the

FIG. 1.—Drawing from life of the Hermit-crab (*Eupagurus bernhardus*) in a shell of the common whelk with two anemones (*Calliactis parasitica*) shown in the natural feeding position, and with the commensal worm (*Nereis fucata*) in the act of taking food out of the jaws of the hermit-crab. (About half the natural size.)

tank (as is shown in Fig. 1) and being trailed about in this position by the crabs. At 10 A.M., before the feeding, two anemones were closed, three already had their discs on the ground, and thirteen were held horizontally from the apical region of the shell-house of the crab, and at 1 P.M., after feeding, many were again closed or with their bodies held horizontally. On September 29 the experiment was repeated, but this time fresh dredgings only were thrown into the tank. All the anemones soon put their discs flat on the ground, and those which were sitting horizontally bobbed their discs down on the ground within a few minutes, almost as though the order "heads down" had been given and obeyed. It was not possible to see whether the unusual movements of the crabs on the addition of food, or the smell of the added food, caused the anemones to react as they did.

On adding the food to the tank it was also observed that the worms (*Nereis fucata*) living in the shells inhabited by the hermit-crabs also came out to feed. The hungry worms came out cautiously some time after the hermit-crabs had begun to feed, and in one case a worm was observed to crawl alongside the body of the crab (see Fig. 1), over the active mouth appendages, and literally to take with impunity a piece of food from between the jaws of the crab and bolt it. There seems to be little doubt that this action of the worm is consciously tolerated by the hermit-crab, as it was observed that the crab can apparently control the exit of the worm from the shell. It was found, however, that strange

worms taken from other hermit-crab shells are not regarded in a kindly manner by either the anemone or the crab: worms fed to anemones are eaten, and worms straying in the neighbourhood of hermit-crabs were mercilessly torn up and tasted but rejected as food. The spectacle of a hermit-crab cleaning itself after feeding is a revelation of the value of spines and hairs and of the meticulous cleanliness of these animals, and cannot fail to impress the observer with the pleasure—and even mild intoxication—experienced by the hermit-crab from the feed.

It is clear that the anemone derives advantage from the hermit-crab by getting dragged about with its tentacles on the ground and being given opportunities for picking up pieces of food left or lost by the hermit-crab and for capturing other animals as food. The hermit-crabs were not seen to pass on pieces of food definitely to the anemones, but there would always be a good chance of an anemone getting some food from the table of the hermit-crab, owing to the habit of the latter of tearing the food apart.

The crab itself probably derives some measure of protection from attacks from fishes owing to the unpleasantness of its associated anemones as food, but it is well known in this laboratory that the common ballan wrasse (*Labrus bergylla (maculatus)*) will watch its opportunity to seize a large claw of a hermit-crab and shake it—like a dog worrying a rat—with the common result of extracting the whole hermit-crab out of the shell-house without touching the anemone.

The function of the worm in the shell can scarcely be guessed at, but the curious and constant wavelike motion of the whole body of the worm—which can be seen by making a window in the shell—will certainly keep up a strong current of water around parts of the body of the hermit-crab, and may assist the hermit-crab in this way in the aeration of its body or in the removal of effete products. The advantage to the worm of obtaining shelter and of partaking of the hermit-crab's food is obvious.

J. H. ORTON.

Marine Biological Laboratory, Plymouth,
November 9.

First Lessons in Practical Biology.

AFTER being encouraged by favourable criticism, both from the Press and from private individuals (not in all cases personal friends), I was somewhat surprised at the acerbity of the attack, published in NATURE, November 4, upon my unpretentious book "First Lessons in Practical Biology." Helpful criticism is welcome to an author, and the correction of errors can be the making of a second edition of a text-book; but adverse criticism in which personal bias of opinion is allowed to outweigh generally accepted beliefs can have little value either for the author or for the reading public.

If "the telson is not a segment" I am consoled by the thought that two such standard works as "Practical Zoology" (Marshall and Hurst) and "Biology" (Parker) contain the same heresy. If "the biramous appendage is not the primitive form of crustacean appendage" I have still to read a more convincing argument than that given in the "Cambridge Natural History (Crustacea)."

I conclude that the critic was so pained by my restricted use of the term "embryo" (as applied to plants) that he failed to read to the end of the chapter; otherwise he would not have stated that "experiments on plant physiology are not reached until chapters 16 and 17." I agree that it is desirable to introduce plant physiology at an earlier stage in the course; but, with the exception of *germination* (which is introduced in the Easter Term), the experiments seldom yield good results in the winter months. The school year begins towards the end of September, and the arrangement of the chapters (as stated in the preface) was based upon this assumption.

E. W. SHANN.

Oundle School, November 10.

I REGRET that Mr. Shann regards my review of his book as an "attack," and yet more that it calls from him the word "acerbity." The need for brevity compelled, perhaps, a certain bluntness; and I beg him to accept my assurance that it was solely to my regard for space in your columns that any such bluntness was due. It was from like considerations that I was obliged to refrain from indicating the authority for and adducing evidence in support of some of my criticisms.

With regard to the telson and biramous appendage I adhere to my statement. If Mr. Shann will refer to p. 144, § 2 c of Marshall and Hurst (9th edition, 1920), he will see that the telson is spoken of as a "region" of which a "segment" is a part. On referring to the passage in my copy of the "Cambridge Natural History" I find that when I first (presumably in 1909) read its discussion of the relative claims of the biramous and foliaceous limb to be regarded as "primitive," I wrote in the margin "All the facts here stated, if taken in the reverse order, support the opposite theory." This is equally true to-day. If Mr. Shann will read H. M. Bernard's "The Apodidae" (Macmillan, 1892) I shall be astonished if he does not abandon the biramous as the "primitive" form of crustacean limb.

I duly noted that the course was arranged with the view of beginning in the Michaelmas Term; but as the very next sentence in the preface suggests modification of the order "at the discretion of the teacher," I felt justified in directing attention to the tardy appearance of plant physiology. The fact that some physiological experiments occur as early as chapter 14 does not seriously affect my criticism.

THE REVIEWER.

The Mechanism of the Cochlea.

IF I understand Dr. Perrett's letter in NATURE of November 11, p. 633, his objection to Yoshii's experiments (which would apply equally to those of Wittmaack and Siebenmann) is based on the assumption that the intensity of the stimulation of every part of the cochlea must be proportional to the amplitude of the vibration set up in that part. I think this assumption is unwarranted, as the intensity of the sensory impression may vary also with the rapidity and the rate of change of direction of the movement imparted to the cilia of the hair-cells; i.e. as the total energy of the stimulus, not its amplitude only. Even supposing Dr. Perrett's assumption were correct, still Yoshii's deductions are not invalidated. Take the case in which he found that after prolonged subjection to high-pitched noise the basal portion of the cochlea showed degeneration. He deduces the logical conclusion that a high-pitched note

stimulates the basal portion of the cochlea. It does not matter whether the stimulus thus applied were small as compared with that produced in the apical region by a prolonged low note or not. The apical region remained unaffected because it was not stimulated at all.

I cannot say that my model shows the shifting of the responses according to the intensity of the stimulus that Dr. Perrett says it should do, and possibly my knowledge of physics is insufficient to enable me to appreciate the reasons which lead him to look for this result. Personally, I have very little faith in the "crucial test" method of solving the problem of sound perception. The question has already been so long and so keenly debated, and so many "crucial tests" have been applied on both sides of the argument, that one almost begins to doubt the possibility of tone perception at all.

I have read Sir William Bayliss' letter (p. 632) with great interest. Naturally, it is very gratifying to me to find that my view of the mechanism of the cochlea has the support of so distinguished a physiologist. I am not very sanguine that my model will throw much light on the more refined details which he gives of the working of the cochlea. What the model actually shows is a definite, though not always well-defined, series of responses at different points along the "basilar membrane" for vibrations varying in frequency from about 100 to about 1000 D.V. per sec., the higher notes being at the proximal and the lower at the distal end of the scale. More than this I cannot claim for it. The mechanical difficulty of setting up a series of short threads, evenly spaced, evenly graduated in tension, and maintaining their spacing and tension unaltered during and after the processes of fixation, embedding and immersion in fluid, is so great that I have not succeeded so far in attaining anything approaching accuracy.

One need scarcely say that so imperfect an apparatus cannot, in its present state, throw much light on the more recondite points. If on the other hand we concentrate our attention on the more obvious, and more fundamental factors, I think the model does give some help. We recognise in the basilar membrane of the cochlea a threefold differentiation of its fibres, for length, tension and mass, and this differentiation is progressive, and in the same sense for each factor. We can embody those mechanical factors crudely in the form of a working model, and we get some sort of remote and inaccurate representation of what happens in the cochlea. The effects observed are undoubtedly resonance effects. It follows that the same resonance effects must take place in the cochlea. One cannot understand how Nature could evolve so elaborate a mechanism of resonance as we find in the cochlea, except by means of, and for the purpose of, increasingly accurate analysis of sound.

G. WILKINSON.

387 Glossop Road, Sheffield, Nov. 15.

An Offer of Nature Volumes.

THE writer has been entrusted with the disposal of thirty-three volumes of NATURE (unbound, as issued) which their owner wishes to present to some library in the war-devastated area. These consist of vols. 50 to 56, 74 to 92, 97 and 98, and 103 to 107. A few parts are missing. Should any reader of NATURE know of some one who may be communicated with for this purpose, the information would be gratefully received.

M. GHEURY DE BRAY.

40 Westmount Road, Eltham, S.E.9,
November 13.