It is not possible, within the narrow limits of a small text-book, to present an adequate picture of the tribes and orders of the Cryptogams, and this is especially true for the Algæ and Fungi. Hence the treatment accorded to the last-mentioned groups is of necessity somewhat sketchy. But, nevertheless, the author has managed to include a considerable amount of the most important information respecting them, illustrated in many cases by copies of Kny's admirable *Wandttafeln*. We notice, however, that the familiar drawing, after Sachs, of the structure of the mushroom is reproduced, in which the basidia are represented as bearing only two spores, although in the text the normal number is correctly given as four. It seems high time that this figure disappeared from our text-books ; its chief function at present is to show how difficult a matter it is to get rid of a fiction which has once managed to pass itself off as a genuine fact. Whilst we are on the subject of illustrations, we cannot forbear to remark on the surprising group of Moss-antheridia represented in Fig. 860. No doubt in future editions the author will replace this by a more adequate drawing. Both the Vascular Cryptogams and the Angiosperm are, on the whole, admirably treated, but the Gymnosperms hardly receive the recognition due to their important position ; we venture to think that the artificial key on p. 180 might well have been omitted.

In dealing with the Phanerogams the classification of Bentham and Hooker is adhered to, and much valuable information is given as to the uses and geographical distribution of the plants comprised in the various Orders.

But it is the physiological part of the book which impresses us most favourably. The student will find the most important facts and principles of this branch of the science most clearly and suggestively put before him. Nutrition is especially well handled ; and it is not necessary to add that the chapters on reserve materials and ferments form a most valuable epitome of our knowledge respecting them, since the author is well known as a distinguished investigator in connection with these matters.

The book is, altogether, one of the best of our English intermediate text-books, and it is certainly one which no student ought to neglect.

Wool Dyeing. Part i. By Walter M. Gardner, F.C.S. Pp. 108. (Manchester : John Heywood.)

THIS little book is a reprint of a series of articles contributed to the *Textile Recorder*. In spite of the title, the subject of dyeing is not dealt with, being reserved for parts ii. and iii., and only the operations previous to dyeing are treated of in the present volume. The divisions of this subject are : (1) The Wool Fibre ; (2) Wool Scouring ; (3) Wool Bleaching ; (4) Water for Technical Purposes. The treatment, although not exhaustive, is fairly thorough and quite up-to-date, and no important feature of these subjects, either chemical or mechanical, is left untouched. The book can hardly be described as attractive reading for an outsider ; but it will doubtless prove useful to teachers and students in technical classes, and should be helpful also to those engaged in the dyeing industry—happily a growing number—who wish to understand the principles underlying the operations they conduct, and who may be led by it to the study of some more exhaustive work.

It is to be regretted that in appearance the book is hardly worthy of its subject-matter ; the paper employed has a very uninviting aspect, and the few illustrations are of little value. This being the case, we must demur at the author's claim to cheapness. One more complaint : to those whose chemical knowledge is but slight, one or two misprints may cause perplexity, and we can imagine a student inquiring in bewilderment why the hardness of water should be expressed in terms of the $CaCO_1$ (*sic*) contained therein.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Osmotic Pressure.

IN the October number of the *Philosophical Magazine* will be found an interesting paper, by Prof. Poynting, which explains the phenomena of the osmotic pressure of solutions by the hypothesis of chemical combination between the solvent and the dissolved matter. I wish to direct the attention of your readers to one point in the paper, and to a development of it which seems to me to be worthy of notice. Any successful theory of solution must explain the fact that the osmotic pressure obeys the usual laws of gaseous pressure—those of Boyle and Avogadro—and, moreover, has the same absolute value as that of the pressure which the dissolved molecules would exert in the gaseous state, when filling a volume equal to that of the solution. It has always been clear that, whatever be the ultimate cause of the osmotic pressure, the gaseous laws must be obeyed by dilute solutions. The molecules of any finely-divided matter must be, in general, out of each other's sphere of influence, so that each will produce its effect independently of the rest. But this is all that is necessary for Boyle's law and Avogadro's law to hold, so that these, as well as the mere existence of osmotic pressure, are explained by chemical combination just as well as by molecular bombardment. On the other hand, no good reason has been hitherto given why chemical forces should be so adjusted that the osmotic pressure of the dissolved molecules should have the same absolute value as that of the pressure which the same number of gaseous molecules would exert when filling an equal volume.

Prof. Poynting supposes that each molecule of dissolved matter combines with the solvent to form unstable compounds, which continually exchange constituents. The molecules of solvent thus combined will be less energetic than the molecules of pure solvent, and thus may be unable to evaporate. Nevertheless, since they are always being liberated and re-combined, they will still be effective in retaining molecules of vapour condensing on the surface. The vapour pressure will, therefore, be reduced, and it follows that, if we make the additional assumption that one molecule of the dissolved substance unites with one molecule of the solvent, the fractional diminution of vapour pressure will be the same as that calculated from the osmotic pressure given by Van 't Hoff's law. We can, of course, working backward from this result, show that the osmotic pressure will have the gaseous value. If one dissolved molecule combines with two or three solvent molecules, the osmotic pressure would have double or treble its normal value. Although he does not explicitly say so, I fancy that Prof. Poynting means to suggest this as a cause of the abnormally great osmotic pressures shown by solutions of metallic salts and other electrolytes, and thus to do away with what he calls " the difficulties of the dissociation hypothesis."

Now the evidence in favour of the view that the opposite ions of an electrolyte are dissociated from *each other* is enormously strong, though there is no reason to suppose that the ions are not united to the solvent. Some of the chief points in favour of their freedom may shortly be summarised as follows : (1) The fact that the electrical conductivity of a dilute solution is proportional to its concentration ; whereas, if the ions moved forward by taking advantage of collisions between the dissolved molecules and consequent rearrangements, it would vary as some power of the concentration higher than the first. (2) The confirmation of the values given by Kohlrausch as the specific velocities of the ions, the velocity of each ion being, in dilute solution, independent of the nature of the other ion present. (3) The successful calculation by Nernst and Planck of the coefficients of diffusion of electrolytes, and of the contact differences of potential between their solutions on the hypothesis that the ions migrate independently of each other.

Thus we cannot lightly give up the idea that the ions are free from each other, and it seems to me that a very simple extension of Prof. Poynting's theory will enable us to retain that view. We have only to suppose that, in the case of electrolytes, the dissolved molecules are resolved into their ions, and that each ion so produced unites with one solvent molecule, or, at all events, destroys the mobility of one solvent molecule. A simple