tried by students attending the lectures at the Finsbury Technical College, who, as is stated in the preface, written by Prof. John Perry, have worked through them and obtained "a real good working knowledge of the application of the principles of mechanics and machine design; . . . their knowledge was always ready for use."

The examples, as a rule, are thoroughly practical, and may be taken as illustrating Prof. J. Perry's book on "Practical Mechanics," and Prof. Unwin's book on "Machine Design."

To make the volume more complete, useful rules and constants, together with tables of sines, cosines, tangents, and cotangents, of angles from 1° to 45°, are added, concluding with a table of the squares, cubes, square roots, cube roots, and reciprocals of all numbers from 1 to 100, and of approximate fifth roots from 1 to 1000.

A Text-book of Physiology. By M. Foster, F.R.S. Fifth Edition. Part I. comprising Book I. (London: Macmillan and Co., 1888.)

THIS work was originally published in 1876, and it has become so widely known that we need not now do much more than note the appearance of the first instalment of a new edition. In this edition—the fifth—considerable changes and additions have been made. The changes, however, do not affect the character of the book; and Prof. Foster explains that the additions, with the exception of the histological paragraphs, are caused, not by any attempt to add new matter or to enlarge the general scope of the work, but by an effort to explain more fully and at greater length what seem to him to be the most fundamental and most important topics. He has introduced some histological statements, not with the view of in any way relieving the student from the necessity of studying distinct histological treatises, but in order to bring him to the physiological problem with the histological data fresh in his mind. Hence in dealing with the several histological points the author has confined himself to matters having a physiological bearing. This first part will be followed as soon as possible by the second and third parts.

The Analyst's Laboratory Companion. By Alfred E. Johnson. (London: J. and A. Churchill, 1888.)

DURING the past four years, Mr. Johnson has had in every-day use in the laboratory a manuscript book of factors and tables. The work grew by constant additions, made as required; and in the end, as he explains in the preface, it became complete enough to encourage him in the belief that it might prove useful to analysts generally. Accordingly he has issued the present little volume, and no doubt he is right in thinking that the large amount of labour involved in the calculation of the many original tables here published may be found to save much of the time otherwise required by the analyst in working out the results of analysis. For the convenience of students not well acquainted with logarithms, of which he has made free use, he has given an account of them, adding examples fully worked out and chosen so as to include and explain the difficulties generally felt in connection with this subject.

LETTERS TO THE EDITOR.

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Prophetic Germs.

I REGRET to find that I put an erroneous interpretation upon the phrase "non-significant organs," as used by Prof. Ray Lankester. I never doubted that it meant organs or structures which were non-significant in respect to actual use; that, in short, it was his phrase for what other men have variously called aborted or rudimentary organs. He now explains that "non-significant," in his terminology, means any variation from hereditary forms which is fortuitous—as unknown in respect to its origin as it is in respect to its actual or future use. Although I see no value in this phrase as descriptive of anything that exists, I see great value in Prof. Ray Lankester's admission that natural selection cannot act upon any structure which is not already developed up to the stage of actual use. This is really all I want for my previous argument, because all organs whatever do actually pass through rudimentary stages in which actual use is impossible. In no possible case, therefore, can selection explain the origin of any organic structure. I rejoice to find Prof. Ray Lankester denouncing as "an absurdity" the idea that "congenital variations are selected when they are not of any actual use." It must therefore be quite according to the admitted constitution and course of Nature that we should find organs "on the rise," as well as organs "on the wane." All germs must be prophetic of their future use, so long as they are in germinal stages; and, if evolution be true, the world ought always to have been full of them, and ought to be full of them now, unless the creative or evolutionary work has been arrested, at least locally, and for a time.

ARGYLL.

Inveraray, Argyllshire, October 8.

The Geometric Interpretation of Monge's Differential Equation to all Conics.

WITH reference to the remarks of "R. B. H." (NATURE, June 28, p. 197) on my interpretation of the differential equation to all conics, I wish to point out that the objections he seems to take do not appear to be well founded. The difficulty he finds is that the geometrical interpretation given amounts to the fact that "a conic is a conic." But it is easy to see that there is no peculiarity in this; it arises simply from the well-known fact that all the geometrical properties of any given figure are inter-dependent: one of them being given, the others may be deduced as legitimate consequences from it. "R. B. H." takes the proposition which constitutes my interpretation, and then, coupling it with the other theorem that the osculating conic of any conic is the given conic, comes to the conclusion that a conic is a conic, and, apparently, he takes it to be very strange; but, as a matter of fact, given *any two* properties of a conic (or of any other curve), we can only come to the conclusion that the conic is a conic (or that the given curve is what it professes to be). Take, for example, the geometric interpretation of the differential equation of all right lines, which is $q = 0$; it simply means that the curvature vanishes at every point of every right line, which is equivalent to the fact that a straight line is not curved, or that a straight line is a straight line. There is certainly nothing strange in this: it is the legitimate effect of the process employed. Would "R. B. H.," on this ground, reject the geometrical interpretation of the differential equation of all straight lines? Surely the process is nothing but a piece of quite unobjectionable verification. Similarly, the differential equation of all circles, $(1 + p^2)r - 3pq^2 = 0$, means that the angle of aberrancy vanishes at every point of every circle. Combining this with the self-evident proposition that the normal and the axis of aberrancy coincide in the case of a circle, we may come to the conclusion that a circle is a circle; but I submit that this is really a verification, and surely no ground for rejecting the interpretation. Indeed, the question whether such processes are to be regarded as verifications or not seems to me to be much the same question whether every syllogism is a *petitio principii* or not. But as I have elsewhere, in the papers referred to in my last letter (p. 173, *ante*), fully discussed what a geometrical interpretation properly ought to be, I need not enlarge further on this point.

As to the difficulty which "R. B. H." feels in drawing a curve at every point of which the radius of curvature vanishes, I may remark that this is a "limiting case," and the matter becomes clear when my interpretation is paraphrased thus: "If the radius of curvature of the aberrancy curve of a given curve vanishes at every point, that curve degenerates into a conic."

Finally, I fail to see why an interpretation is to be rejected simply because the property it enunciates happens to admit of an easy verification. The conic has an infinite number of properties, and the chief difficulty in discovering the geometrical interpretation of its differential equation has been to find out which