exist in both departments of research is a statement equivalent to the as yet undisputed assertion that human faculties are limited and human knowledge imperfect. Nevertheless, some of the difficulties raised by him are well worthy of thoughtful consideration. There can be no doubt, for instance, that the results of M. Gustav Hirn's experiments on the resistance to motion of air at different temperatures are gravely embarrassing to the kinetic theory of gases. The resistance *ought* to vary as the square root of the absolute temperature; it continues, nevertheless, practically unchanged at all temperatures, so long as the density remains the same (*Comptes Rendus*, t. xciv. p. 379).

Our author's strictures, however, although couched in terms of praiseworthy moderation and fairness, are somewhat too indiscriminate. His respect for thoroughgoing scientific work is evident and unfeigned; but it is almost impossible to take up the position of a sceptic *ex professo*, without at times incurring the reproach of perversity. We are totally unable to see the force of his reasons for preferring the obscure notion of a threefold spectrum containing chemical rays specifically distinct from thermal, and thermal from luminous, to the simple and intelligible view which substitutes for intangible differences of quality in radiations, measurable differences of wave-length. Nor can we believe that he would, on mature consideration, attempt to maintain the opinion that the planetary movements would remain unaffected by the progressive transmission of gravity. It is demonstrable that a species of aberration would thence ensue involving accelerative effects the insensibility of which so far proves (as Laplace has shown) that gravitative influence travels *at least* fifty million times faster than light. We owe the remark to Mr. William B. Taylor (*Smithsonian Report*, 1876, p. 212), that if one minute were spent on the journey from the sun to the earth, the consequent slight obliquity of traction, represented by an angle of $2''\cdot4$, would produce a shortening of the year perceptible while the sun was finishing a single round of the Signs.

Prof. Zanon appears to have bestowed much intelligent study on spectroscopic science. He carefully examines Mr. Lockyer's discovery of the independent "behaviour" of iron lines (amongst others) in spots and prominences, and though rejecting the inference of dissociation, he admits the necessity of assuming an *ad libitum* number of allotropic forms of the substance in question in order to explain anomalous appearances. He published moreover in 1880 a detailed analysis of Mr. Lockyer's "Studies in Spectrum Analysis," deriving therefrom confirmation in many particulars of his own views as to the nature of matter. Whatever we may think of these, there can be no doubt that our hope of penetrating the mysteries of molecular constitution must in future rest on the disclosures of the spectroscope. We do not in the least believe that these point towards the conclusion deprecated by Prof. Zanon of a fundamental unity of substance. On the contrary, Mr. Lockyer's striking observations indicate (if we may be allowed the expression) divergent, rather than convergent, simplifications.

It is perhaps inevitable in a work of the character of that now under consideration that the value to science of "working hypotheses" should be taken little account of. Yet the story of man's progressive knowledge of

nature is a story of tentative efforts to represent facts to thought. Without some method of provisionally co-ordinating phenomena, indeed, the advance from a lower to a higher stage of induction, which we call discovery, could scarcely at all be effected. The true investigator is he who is never weary of collecting particulars to fit into his framework of theory, and of reconstructing his framework of theory to match fresh particulars. Without the power of thinking appearances into shape, no diligence in amassing the details of those appearances is of the slightest avail.

There is, it is true, the danger of a working hypothesis hardening unduly or prematurely into a theory—of a mere track, struck out for surveying purposes, turning into a fenced highway, without exit save in a quagmire. Against this peril the activity of such critics as Prof. Zanon is a very effectual safeguard. Their assaults may sometimes appear vexatious, but are really directed in the best interests of science, as hindering false security, and leading to a juster estimate of probabilities.

A. M. CLERKE

A MANUAL OF MECHANICS

A Manual of Mechanics; an Elementary Text-Book, designed for Students of Applied Mechanics. By T. M. Goodeve, M.A. (London: Longmans, 1886.)

THIS little work is evidently meant for readers whose knowledge of mathematics is small. The amount of mathematical knowledge supposed may, perhaps, be best inferred from an example quoted from the book itself. In p. 32 we have the problem to find the resultant of two equal rectangular forces, P . If R is the resultant, $R = P\sqrt{2}$. "Now $\sqrt{2} = \sqrt{\frac{50}{25}} = \sqrt{\frac{49}{25}}$ very nearly; $\therefore \sqrt{2} = \frac{7}{5}$ very nearly. Whence $R = \frac{7}{5}P$." When the angle between the lines of action of the forces is 120° , R should be equal to P ; but it is proved in p. 33 to be equal to $P\sqrt{3}$, by an unaccountable error in the application of an elementary formula. However, $\sqrt{3}$ is found by the same process as before. Thus $\sqrt{3} = \sqrt{\frac{48}{16}} = \sqrt{\frac{49}{16}}$ very nearly, $= \frac{7}{4}$.

Now what naturally strikes one about beginners who have to be spoon-fed in this fashion is that it would be very much better to spend some time in teaching them trigonometry and algebra, than to push them into dynamics with the very slender knowledge of arithmetic and geometry which they possess. In our system of national education there is far too much effort employed in bringing branches of dynamics and physics *down* to the level of the ignorant, and too little in bringing students *up* to the standard of mathematical attainment required for a really intelligent knowledge of these subjects. The result of this system is that nearly all our elementary scientific books are very scrappy in character, presenting to their readers conceptions which they cannot possibly realise, and which must therefore be crammed into the mind as mere definitions devoid of real meaning.

In elementary scientific treatises it is necessary to insist, above all things, that the information given to the

reader shall be above reproach, so that if he further pursues the subject treated of, he may, at least, not be hindered or misled by erroneous definitions or ideas placed before him at the outset.

The work before us, when it has been subjected to a slight revision, will fairly satisfy the conditions of an elementary text-book which is at once strictly scientific in its ideas and rich in practical applications.

Indeed, the practical portion of the work is, as was to be expected, very good. The portions dealing with mechanism are well suited to illustrate the scientific principles, and will be found very useful.

We shall confine ourselves to pointing out some of the blemishes which disfigure the theoretical portion.

P. 1. "When the tendency which force exerts to move a body is counteracted, so that the body remains at rest, the force is commonly called a *pressure*." It may be safely said that no such limited use is ordinarily made of the term *pressure*.

P. 2. The statement that two lumps of sugar placed in contact on a table do not adhere, because "the attraction emanates from their centres, which may be an inch apart," is not altogether conducive to sound notions.

In p. 19 we have the usual definition of the measure of a variable velocity, viz. "the space which would be described in a unit of time if the body retained throughout that unit the velocity which it has at the instant considered." When will people find out that this definition really defines nothing? It is truly Gladstonian in its elusiveness. Of course we find exactly the same "definition" of variable angular velocity (p. 62), variable acceleration (p. 99), and variable pressure (p. 176).

In p. 26 we have a most confusing exposition of Newton's Second Law. What, for instance, is the beginner to understand by this?—"Since we are dealing with a body in motion, it is clear that we may consider (1) the weight of the body to be constant, and its velocity to vary; (2) the weight of the body to vary, and its velocity to remain constant." The discussion of this law seems to show that its object is entirely misconceived by the author. For, while the law aims at giving a complete measure of *force*, the author merely *deduces from it* (by reasoning in a circle) a definition of momentum, his discussion ending with the words, "Hence, quantity of motion = (mass) (velocity)."

Again, in p. 28, "the explosive force of powder in a gun is *action*, and the momentum generated in the projectile is *reaction*." This is plainly open to the objection of equating force to momentum—a most mischievous notion.

In p. 48 the full meaning of the *centre of parallel forces* is by no means brought out, and the rule for finding its distance from a given line is incorrect: "Multiply each force into its perpendicular distance from a given line," &c. The rule ought to be enunciated with reference to a *plane*, and not to a *line*, and instead of the unmeaning term "distance of a force from the plane" we should say "distance of its point of application from the plane."

It is surely incorrect to say, as in p. 61, that "it is the universal practice to appropriate the Greek letter ω (omega) for expressing the unit of angular velocity."

It is very important to distinguish, in the mind of the beginner, between *force* and *power*. In p. 72 power is

correctly defined as [time-] rate of doing work; but the language immediately following loosens this idea. Thus, "the number 33,000 foot-pounds is the *unit* of horse-power." There is tautology in the expression "Watt estimated that the sustained work of a horse continued for one minute would raise 33 000 lbs. through one foot in one minute." Again, the term *power* is used in p. 94 to designate a force applied to a machine.

In p. 73 the principle of work as applied to equilibrium is thus enunciated: "If a system of bodies be at rest under the action of any forces, and be moved a very little, no work will be done." It may well be doubted whether this enunciation is explicit enough to give a useful idea of the great statical principle. Indeed, the cases selected for illustration bear out in no way the proviso that the bodies must "be moved a very little."

Passing on to p. 98, we find acceleration of velocity discussed, and we have the formal definition: "the unit of acceleration adds a velocity of 1 foot in 1 second," which, as the words stand, has absolutely no meaning. The beginner ought to be taught that acceleration has a double reference to time, and it should never for a moment be spoken of otherwise than as "feet per second per second," or "miles per hour per minute," or by some other equivalent phrase. In p. 102, *g* is spoken of as 32.2 feet per second.

Finally, in p. 113 the beginner is introduced to the discussion of a compound pendulum formed by a straight uniform bar. Its time of oscillation depends on a radius of gyration. What idea is our beginner likely to have of this? The definition of *k*, the radius of gyration of the bar *AB* about an axis through its centre of gravity, *G* (p. 114), is quite erroneous—"let *k*² be the sum of the squares of the distances of each particle of *AB* from the point *G*." This sum is, of course, infinite. What is meant by *k*² is the *mean* square of distance from *G*. But clearly such a question is wholly unsuited to beginners.

The book concludes with a large collection of examples, taken from the Science Examinations, which will be useful to students.

GEORGE M. MINCHIN

OUR BOOK SHELF

Mémoire sur les Volumes moléculaires des Liquides, avec un Avant-propos. By Hermann Kopp. Pp. 47. (Heidelberg: C. Winter, 1886.)

THIS is a *brochure* one-third of which is devoted to an "avant-propos" explaining why the paper does not appear in the *Annales de Chimie et Physique*, and making certain statements relating to M. Berthelot's recent work, "Les Origines de l'Alchimie." The remaining part contains a discussion of a paper on molecular volumes of liquids which appeared in the *Annales*. In 1869 Prof. Kopp published, in succession to his "Geschichte der Chemie," a volume of "Beiträge" to the history, explaining more fully the principal epochs in chemical history, more especially in relation to alchemy. In 1885 M. Berthelot published his work, which had occupied more or less of his attention since 1869. Its general character is very similar to the "Beiträge," though much less complete, and Kopp complains that no reference whatever is made to his book. Berthelot, in reply, states that he was unaware of the existence of the "Beiträge," yet he quotes references to works in which this "cahier" (as he calls it) is repeatedly mentioned. The oversight is a very grave one, when we remember that the "cahier" is a contribution of over