case, and all physicians in cases of doubt make this examination or have it made. Special instruments like the hæmacytometer of Gowers or Thoma, or the hæmaglobinometer of Gowers, have been made for this purpose and can be purchased from all dealers in microscopical instruments

The disease known as Filariasis can be and is diagnosed by blood examination. The parasites causing this disease occur in the immature state in the blood, passing, as they mature, into the lymphatics. These parasites are truly remarkable from the fact that they are found in the blood only at night, being almost or entirely absent in the daytime; if, however, the patient sleep during the day this is reversed, thus showing that the condition of sleep is an important factor in determining the presence the organisms.

From these facts it would seem that the medical profession is not in quite as 'dense' a state of ignorance regarding the blood as Prof. Michels would have his readers believe, and that they do make use of blood examination in the diagnosis of disease.

JOSEPH F. JAMES.

WASHINGTON, D. C., Sept. 4, 1895.

SCIENTIFIC LITERATURE.

The Science of Mechanics. A Critical and Historical Exposition of its Principles. By DR. ERNST MACH, Professor of Physics in the University of Prague. Translated from the Second German Edition by Thomas J. McCormack. The Open Court Publishing Co., Chicago.

The Science of Mechanics is an English translation of the German treatise by Professor Ernst Mach, on The Development of Mechanics; a work whose ability and importance entitle it to critical attention. While not a complete history of the science, it deals with the subject by the historical method and purports to be a philosophical discussion of the nature, origin and relations of those ideas and principles in mechanics which, when thus linked together, give an intelligible and comprehensive view of the science as it now is, and of the sometimes tortuous way by which it reached its present state. The book as a whole is unique, and is a valuable addition to any library of science or philosophy.

The author's well-known psychological bent is here directed to getting rid of metaphysical obscurities that befog the discussions of the seventeenth and eighteenth century physicists. He presents mechanics as a physical rather than a mathematical science, employing mathematics to some extent, necessarily, but with care not to make of a proposition in mechanics a mere peg on which to hang mathematical formulæ.

After a brief introduction, the work is arranged in a historical view of the development of the principles of statics, to which a hundred and twenty pages are devoted; then about an equal space is given in the same manner to dynamics, this being the order in which the science actually grew up. These divisions overlap somewhat, the former being carried well into the eighteenth century, while the latter begins with Galileo in the seventeenth century, but the order is, on the whole, very satisfactory.

Although the subject-matter of the first chapter may be of less immediate interest than that of the next, yet the author's treatment of it and his philosophical discussion of the early investigators' work and methods of working is most interesting, while the manner in which he shows how a principle has been employed *in essence* by one and another such investigator in its application to special and apparently unrelated questions, before some one makes the happy generalization that gives it the force of a law, is admirable. As one example among others, it is shown how the principle of virtual velocities was made use of by Stevinus in the sixteenth century, and later by Galileo, Torricelli and others before 'the universal applicability of it to all cases of equilibrium was perceived by John Bernoulli,' early in the eighteenth century.

"They that know the entire course of the development of science will, as a matter of course, judge more freely and more correctly of the significance of any present scientific movement than they who, limited in their views to the age in which their own lives have been spent, contemplate merely the momentary trend that the course of intellectual events takes at the present moment.'' (p. 7.) The work exhibits this forcibly and repeatedly. Thus, by an extension of the principles employed by Stevinus in the study of hydrostatics, the author deduces a proposition which is now readily recognizable as a special case of Green's Theorem. ''We may accordingly,'' says Professor Mach, ''*see into* the force-system of a fluid in equilibrium, or, if you please, *see out* of it, systems of forces of greater or less complexity, and thus reach by a short path propositions *a posteriori*. It is a mere accident that Stevinus did not light on these propositions. The method here pursued corresponds exactly to his.'' (p. 109.)

The process from special cases to general principles is of course one of economy, and we might expect that any opportunity thus to economize would be at once seized upon. Says the author, '' economy of communication and of apprehension is of the very essence of science,'' and this economy, serving at first to satisfy mere bodily wants, becomes later a potent factor in the development of science in its more advanced and specialized forms. At many points in the book we are reminded of this thesis, but almost immediately after it is stated we are brought face to face with a feature in the history of science that seems in contradiction to it, for after recounting the points which Archimedes, in beginning his study of equilibrium, assumed as self-evident, and then presenting that philosopher's mode of establishing the law of the lever, we are introduced to a succinct statement of the different methods by which Galileo, Huygens, Lagrange and others demonstrated the same law. We may believe that, in part, various philosophers produced new demonstrations because they saw or thought they saw fallacies in the reasoning of their predecessors, but this, we think, is not the principal reason. The fact is rather an illustration of the other fact that, in olden times, a problem once stated, existed, in the estimation of many, for the purpose of bringing out *all the solutions that could be found*. Hence the multiplicity of solutions to various problems as, for example, the many proofs of Euclid's Forty-seventh. There does not seem to be much economy of time or labor in this. Professor Mach recognizes and condemns this tendency, calling it a 'mania for demonstration in science.' It is a fact that variety in the solutions of problems in mechanics led to the development of principles not before recognized, and thus resulted in an expansion of the science. This is shown by Professor Mach where the generalization of the principle of the lever by Leonardo da Vinci brings into prominence the principle of statical moments; and in like manner other advances are introduced, but it was not for the sake of these, nor yet in the interest of economy, that the new demonstrations were produced.

It is shown that the celebrated investigation of the inclined plane by Stevinus virtually involves the principle of the parallelogram of forces, and the principle itself is then stated and the fact commented on that Varignon as well as Newton determined it. The importance of the principle in both statics and kinetics is very properly recognized, but surely it scarcely needs pointing out that the statement and conception of the principle in connection with the parallelogram at this day is not most economical in mental labor or in manual application. It accords well with the cumbersome form in which many statements were made early in the development of science, and in their time the forms were excusable, but that a writer should continue to employ this principle now in the form in which it was enunciated by Newton is not an indication of any economical tendency. For the science has got beyond that. So soon as the idea is accepted that the result of several forces acting simultaneously upon a particle is the same, whether they are considered independently of one another or collectively, the graphic composition of the forces by vectorial addition becomes at once the simplest and most rational method. This, for two forces and their resultant, gives the triangle and dispenses with the parallelogram and diagonal idea altogether, besides serving equally well for three forces in equilibrium. As good a treatise on mechanics can be produced to-day without any reference to the parallelogram of forces as with it, and such is now the tendency. If the idea of 'economy of communication and of apprehension' is to prevail we must carry out this tendency, but it will be done, if at all, only after an unduly prolonged, wasteful adherence to the parallelogram.

In treating of the development of dynamics, attention is confined principally to the achievements of Galileo, Huygens and Newton. The exposition of the work of Galileo is excellent, marking out in clearest lines his superiority as a truly scientific investigator over all his predecessors and most of his successors. His greatest work, of course, was his determination of the laws of falling bodies, and consequently of uniformly accelerated motion. In everything concerning the relation of motion to the circumstances that affect it, Galileo had to make his way as a pioneer. After first examining whether the velocity of a falling body varied directly as the distance, and abandoning this for the assumption that it varied as the time, he was led to a correct idea of acceleration, and also to that of force as measured by the product of mass and acceleration. Owing to the physical limitations under which he was obliged to perform his experiments, it was necessary for him to make various assumptions, whose validity always had to be proven. For instance, he retarded the motion of falling bodies by causing them to descend inclined planes, and then examined the peculiarities of their motion upon the assumption that "a body which falls through the height of an inclined plane attains the same final velocity as a body which falls through its length."

The reasoning by which he felt warranted in in making this assumption brought him to the conclusion that if a body, in falling down the length of an inclined plane, acquired a velocity different from that gained by falling through its height, "a heavy body could, by an appropriate arrangement of inclined planes, be forced continually upwards solely by its own weight." But besides justifying the assumption logically he verified it experimentally. Both his reasoning and his experimentation were confined to the action of single bodies. Later, when Huygens solved the problem of the centre of oscillation of a compound pendulum he made use of a principle which, in its ultimate nature, was like that employed by Galileo, as follows: "In whatsoever manner the material particles of a pendulum may by mutual interaction modify each other's motions, in every case the velocities acquired in the descent of the pendulum can be such only that by virtue of them the centre of gravity of the particles, whether still in connection or with their connections dissolved, is able to rise just as *high* as the point from which it *fell*. Huygens found himself compelled, by the doubts of his contemporaries as to the correctness of this principle, to remark that the only assumption implied in the principle is that heavy bodies of themselves do not move upwards;" (p. 174), and this principle, as Professor Mach points out, is *a generalization of one of Galileo's ideas.*

The author regards Huygens as in every respect the peer of Galileo, a rank which perhaps few would deny him. The above principle which he introduced makes what we now call the *work* done on a body by gravity, the condition determinative of the velocity it acquires, and this, more than anything else, marks the difference between Huygens' point of departure and that of Galileo and of Newton. All three recognized the fact of accelerations which they ascribed to *force* as a cause whose nature was unknown. Says the author: "That which in the mechanics of the present day is called *force* is not a something that lies latent in the natural processes, but a measurable, actual circumstance of motion, the product of the mass into the acceleration." (p. 246.) But this product is only one way of measuring the mutual actions involved, for not only do bodies influence one another as to velocities, but also as to displacements, and either of these may be made the basis of measuring the force. "We may, therefore, as it suits us, regard the *time* of descent or the *distance* of descent as the factor determinative of velocity. If we fix our attention on the first circumstance, the concept of force appears as the original notion, the concept of work as the derived one. If we investigate the influence of the second fact first, the concept of work is the original notion. * * * In this case we know force only as the limiting value of the ratio which increment of work bears to increment of distance.

Galileo cultivated by preference the first of these two methods. Newton likewise preferred it. Huygens pursued the second method, without at all restricting himself to it.'' (p. 250.)

When we recollect that the adoption of ' work ' as the fundamental concept of mechanics by J. R. Mayer, scarce a half century ago, was the introduction of modern views and methods in physics, and that when Professors Clifford and Tait, in still more recent times, were wont to dwell upon the consideration of force as a space-rate of change in work or energy (or potential), their ideas were regarded as novel and rather disturbing, it is refreshing to find Huygens ranged alongside of the nineteenth century physicists, though chronologically sandwiched between Galileo, who founded the science of dynamics, and Newton, of whom the author says, 'since his time no essentially new principle has been stated.' But, as Professor Mach reminds us, Huygens' principle was by no means well received by his contemporaries, notwithstanding it was his chief performance.

Naturally the achievements of Newton come in for the largest share of attention. The extent of his achievements and the profound and lasting impression which they made upon science compel, in any critic, the most searching scrutiny. It is necessary, too, to distinguish those discoveries and reflections which are Newton's own from those which he accepted from his predecessors and made more available by his clear perception of their relation to physical science in general and by his lucid formulation of them in laws and principles. This distinction has been made many a time, and doubtless many a one has wished to protest against certain of Newton's views, but it must be admitted that except for the old fashioned form of his statements, and the geometrical form of the demonstrations, surprisingly little of his writings has been altered to advantage. So far as his investigations are confined to facts, with abstention from every form of speculation, that is, so far as he conforms strictly to his assertion that he does not frame hypotheses, Professor Mach finds little but pleasure in his great work, and only objects to its form. But in his famous view concerning absolute time, space and motion, Newton departs from a consideration of physical facts, enters into psychology and, in the estimation of the author, makes statements and distinctions that are not justifiable and which he criticises severely. Yet after reading the fifty pages or more that are devoted to the rather unfavorable consideration of Newton's fundamental statements in mechanics, one cannot help feeling that the last word on the subject has by no means been said. When the author says, ''We arrive at the idea of time, to express it briefly and popularly, by the connection of that which is contained in the province of our memory with that which is contained in the province of our sense perception,'' we feel that Maxwell's statement that the idea of time originated probably 'in the recognition of an order of sequence *in our states of consciousness*' is an improvement in form upon the author's, and is more satisfactory, while conforming much more nearly to the Newtonian conceptions. As substitutes for Newton's enunciations Professor Mach offers three experimental propositions and two definitions as being 'much more simple, methodically better arranged, and more satisfactory.' In so far as his criticisms are endorsed the substitute propositions might be approved in substance, but their form savors of pedantry and they have the defect of excessive conciseness; they are therefore technical, and in consequence they require, each on its own account, a good deal of explanation. They can only be called simple for those who are already pretty well aware of what they state, but they prepare us for the remark: ''We join with the eminent physicists Thomson and Tait in our reverence and admiration of Newton. But we can only comprehend with difficulty their opinion that the Newtonian doctrines still remain the best and most philosophical foundation of the science that can be given.'' (p. 245.)

A chapter is devoted to the extension of the principles, in which the reader will find an interesting treatment of the controversy between Descartes and Leibnitz, with their respective followings, over the conservation of momentum and of *vis viva*, with D'Alembert's final adjustment of it. The merits of D'Alembert's principle are enlarged upon we think justly, it being shown to embody within it all that is involved in Gauss' principle of least constraint. The work abounds in such comparisons and analyses as, after an account of Clairaut's treatise on the figure of the earth, we learn that 'in the theory of Clairaut here presented is contained, beyond all doubt, the idea that underlies the doctrine of *force-function* or *potential*, which was afterwards developed with such splendid results by Laplace, Poisson, Green, Gauss and others.' (P. 398).

In the section on mechanical units, adapted to American usage by Mr. C. S. Peirce we notice the suggestion that the unit of acceleration be called a 'galileo,' as one more contribution to supply 'a long felt want.' The suggestion is at once adopted in the illustrations that follow.

Under 'The formal development of mechanics' is presented a view of the characteristic *classes* of problems that have arisen. This, together with a discussion of the various points of view, theological, animistic and mystical, of the great investigators, a section on analytical mechanics, and one on the economy of science, makes a most readable and enjoyable chapter.

The final chapter treats of the relations of mechanics to other departments of science, and is the least satisfactory one in the book. It opens with the declaration that "purely mechanical phenomena do not exist;" an arbitrary assertion which is explained by the equally arbitrary one that 'with dynamic results are always associated thermal, magnetic, electrical and chemical phenomena.' The statements are arbitrary because there is no proof of them. The author deprecates explaining all physical phenomena by mechanical ideas, saying, "we have no means of knowing, as yet, which of the physical phenomena go *deepest*, whether the mechanical phenomena are perhaps not the most superficial of all, or whether all do not go equally deep." Precisely; and for that reason, if for no other, we would take exception to the opening remark quoted above. Even if it were shown that no supposed mechanical phenomenon occurred without one or more of the other effects mentioned, the proposition would be by no means proven. Attraction, repulsion and strain are the very essence of mechanics and it is by no means certain that they are not the essence of other branches of physics also. There is nothing to show that magnetic, electrical and even chemical phenomena may not be ultimately and purely mechanical in their nature.

The translation is occasionally very free, but generally faithful to the meaning of the original, and only varied from it in form, to make the statements more lucid. This effect is heightened by the insertion of several brief notes by the translator.

Reproductions of quaint old portraits and vignettes give piquancy to the pages. The numerous marginal titles form a complete epitome of the work; and there is that invaluable adjunct, a good index.

Altogether the publishers are to be congratulated upon producing a technical work that is thoroughly attractive in its make-up.

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On a Collection of Mammals from Arizona and Mexico, made by W. W. Price, with Field Notes by the Collector. By J. A. ALLEN. Bull. American Museum Natural History, vol. VII., pp. 193–258, June 29, 1895.

This important paper is based chiefly on a collection of 1500 specimens of small mammals obtained by W. W. Price in 1894 in southeastern Arizona. Mr. Price contributes an itinerary and descriptions of localities at which collections were made—a useful feature too often omitted in faunal papers. He also attempts to define five life zones, but fails to correlate them with the zones now commonly recognized in the region. His *first* is wholly Lower Sonoran; his *second* comprises the upper part of the Lower, and lower part of the Upper Sonoran; his *third* is the upper or *juniper belt* of the Upper Sonoran; his *fourth* is the Transition, and his *fifth* the Boreal.

The annotated list of mammals by Dr. Allen, with Mr. Price's field notes, covers 58 pages and is a great addition to the published record of our knowledge of Arizona mammals. Several changes in nomenclature are made and one species is described as new (*Thomomys cervinus*, a pocket gopher from Phœnix). The other new forms were described by Dr. Allen in a previous paper. *Perognathus conditi* and *Perodipus chapmani* are allowed to stand as species, although it has not been shown how the former differs from *Perognathus paradoxus*, or the latter from *Perodipus ordi.*

All of the wood rats are lumped under a