acids of different strengths, and for different times. The results of this investigation were published in 1873, in the *American Journal of Science,* and in the *Proceedings* of the American Association for the Advancement of Science, having been read before that association. It was conclusively shown that there was a steady increase in the extraction of potassium for *five* days, remaining stationary afterwards, the amount extracted during the first twenty-four hours being about one half of the final figure, while phosphorus, lime and magnesia were fully extracted.

Notwithstanding this demonstration, fully published in two standard publications, a number of years later the "Official Chemists," in a meeting at Washington, hastily adopted, against my protest, the arbitrary ten-hours digestion proposed by Kedzie, as the official method to be used in state and government work.

It is no wonder that as a result of this irrational practise, chemical soil analysis became more and more discredited as a means of ascertaining the quality and permanent productiveness of soils. In cases where potassium was in abundant supply, it gave results corresponding to the field tests because of the complete extraction of phosphates, lime and magnesia during the ten hours' digestion. On the other hand, where potassium was deficient, no definite relation between the analysis and practise *could* appear.

But when Hopkins goes so far as to determine the potassium content by the fusion method, thus decomposing all the resistant silicates, feldspar-sand, etc., as well as the easily decomposable zeolitic minerals, he goes far beyond the limits within which any definite correlation between soil composition and vegetative action is to be expected; and whatever conclusions are based upon such analyses are practically groundless. Knowing as we do that the assimilation of inorganic substances from the soil by plants is mediated by *acid* solvents, whether derived from the air, from vegetable decay, from secretion by plant roots or bacteria, it certainly is most rational to ascertain how far *acid* action can go in the soils under examination. *This* limit, and no arbitrary rule of time, or ultimate analysis, must serve as the basis of judgment for practical comparison of soil values, or producing capacity. Hopkins's own experiments on the growth of plants in the undissolved residue from the "official" analysis simply corroborate what had been abundantly shown by Loughridge's work in 1873, but prove nothing against the practical value of soil analyses properly made. They do throw discredit upon the "official method," so far as potassium is concerned.

But soil chemists would feel additionally indebted to Hopkins if he would undertake to supplement the somewhat gratuitous proof he has given of the inadequacy of the official method, by growing plants on the residue from a digestion carried to the limit of acid-solubility; which in the case of the soil selected by Loughridge and myself we found to be five days for acid of the accepted sp. g. of 1.115. I have long desired to make this crucial test, but have not been able to find the time or means to do so. If an Illinois soil can thus be made to yield to any plant a practically important amount of potassium, it will be very desirable to know it and thus put an end to farther controversy in the matter; while rendering an important service to soil investigation and plant physiology.

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September 10, 1915

ELEMENTARY MECHANICS

To the Editor of Science: There have appeared in your pages recently a number of contributions by various authors to the discussion of the dynamical equation $ma = f$ or some of its possible variants. It seems as though it would be necessary, for a complete discussion of the relative merits of the different ways of introducing a student to the dynamical equation cited, to enter at least briefly upon the matter of the student's previous training in mechanics. We are all aware that it is at present somewhat stylish to begin the study of mechanics with kinetics and to treat statics as a special case in which the accelerations are zero, and impact as a special case in which large forces act through small periods of time. This, however, is a distinctly recent movement. The older method of procedure was to study, first, statics and problems in impact and thereupon to proceed to kinetics. The reason for this order was probably not wholly logical but largely pedagogic or historical. A student who has a small knowledge of trigonometry is quite fitted, mathematically, to study both statics and problems in impact; whereas, to obtain valuable training in kinetics a knowledge of the differential and integral calculus, including the simpler differential equations, is necessary. Moreover, as a matter of history, statics and impact precedes, I believe, kinetics. Let us suppose that the student has followed this historic and pedagogical order. In his statics he will have learned to deal with forces; these forces may be measured in any units that are convenient, provided only that all the forces are measured in the same units in the same equation; for the equations of statics are homogeneous in the forces. (I, of course, am speaking only of elementary statics, not of the theory of virtual velocities or of potential energy.) In studying impact the fundamental conception is that of momentum. The student learns that momentum is the product of mass by velocity; that momentum is resoluble as are forces; and that in impact the momentum of a system is conserved. He is then in a position to solve problems in inelastic impact of particles and, with an additional simple law concerning relative velocities, he can proceed to elastic impact. In the problems in impact the units of mass may also be anything, provided, again, that they are the same for all masses; for here again the equations are homogeneous in the masses.

When, now, such a student comes to kinetics he is able at once to proceed to Newton's second law, namely, that the rate of change of momentum is equal to the force. Here, however, we have an equation which is no longer homogeneous either in the mass or in the force, and it is evident, or can be made so to any student, that he can not use arbitrary units of mass and force, but that the two units must be in some way correlated. Indeed we should state the second law in the Newtonian form: The rate of change of momentum is proportional to, or varies as, the force. We then write

$$\frac{d}{dt}(mv) = kf.$$

The constant k, like any factor of proportionality, is determined by substituting the known values for some special case. We naturally select the simplest; that is, a mass falling under its own weight. If now we measure mass in pounds, as we (probably) did in the theory of impact, and force likewise in pounds, as we (likely) did in statics, we find that the mass of weight W has, under the force of weight W, an acceleration g; hence

$$\frac{d}{dt}(Wv) = Wg = kW$$

or $k = g$. We therefore have the fundamental equation of kinetics in the form

$$\frac{d}{dt}(mv) = gf.$$

If we desire to use some other system of units for mass and force we should likewise have to determine a constant k.

It is, of course, true that a weight is not a definite constant thing from place to place, but I should not think of calling the student's attention very vigorously to this difficulty at this stage, particularly as it again is no difficulty at all, provided mass and force are both measured in weights at the same place. Nor do we need to mention that the equation which involves the momentum is one which can still be regarded as valid when the student reaches the theory of relativity and modern electrodynamics whereas the equation $ma = f$ or any equation involving accelerations leads to the ridiculously needless concepts of transverse and longitudinal (and an infinity of oblique) masses.

It has always seemed to me that the historic and pedagogical method of procedure was still the best, notwithstanding the above mentioned and modern style. It is quite true that from a logical point of view things proceed more simply when we start with kinetics; but logic is a very poor substitute for common sense, and it is probably logic more than anything else that makes trouble with our pedagogy in mathematics and, even more, in mechanics and physics—perhaps one would hardly try to be logical in theoretical chemistry. Or let us put it another way. There are various kinds of logic; one kind the mathematician's, which to a certain extent is adopted by others; the other kind of logic being the logic of everybody else; a biologist probably has a logic very different from that of the mathematician and very much more useful to him.

From the pedagogical standpoint strict logic, with all its beauties (which the student always misses) is the most illogical thing there is. The important thing for the student and his teacher is to keep as close to every-day life as possible, and any student knows what a weight of 4 pounds is, so that he can proceed to statics. Moreover, he finds no difficulty in measuring the mass or "quantity of matter" by weighing it, so that again he can proceed to problems in impact. The philosophy of mass or force will appeal to him much more after he knows something about mechanics. Our first problem is to get the student into a position where he can solve such simple problems in mechanics as he sees in the actual world on every side about him, and a certain amount of ignorance, which would be very lamentable on the part of myself and your other contributors, is highly praiseworthy in the student.

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THE END IS NOT YET!

MR. GERALD H. THAYER in a communication to SCIENCE for September 3, 1915, claims to have disposed of Cory's Shearwater, *Puffinus borealis,* by establishing it as a synonym of *P. kuhli.* He finds this identity first claimed by Saunders and later, finding that Godman in his "Monograph of the Petrels" takes the same view of the relationship of the two birds, he considers the matter settled for all time, adding: "It would seem unnecessary, not to say presumptuous, for us to question this determination, or wait to make further comparison of specimens." Ornithology would be in a sad state if we accepted all statements without attempting verification, and fortunately others have not regarded further investigation in this instance as "unnecessary" or "presumptuous."

Had Mr. Thayer looked into the matter a little more fully he would have found that in the *Ibis* for July, 1914, Mr. D. A. Bannerman questions the correctness of Saunders's and Godman's treatment of *Puffinus borealis* and later[1] he affirms its distinctness. Furthermore, Mr. Bannerman was, quite naturally, struck by the fact that the type of Gould's *flavirostris* came from the "Cape Seas" while the bird to which the name was applied by Hartert was a native of the Azores and other east Atlantic islands. Mr. Thayer passed this matter over without investigation, but Mr. Bannerman upon comparing topotypes of *flavirostris* with the Azores bird found that they represented two different forms and named the latter *fortunatus.* Now the interesting point in all this is that should the bird from our north Atlantic coast be regarded as identical with the Azores form the name *Puffinus borealis* Cory is the oldest name for it and must be used; while if they are regarded as distinct, then the American bird will still be known by Cory's name. In either case we shall retain Cory's Shearwater on our list!

Mr. Bannerman regards all these shearwaters as subspecies of *P. kuhli,* but this does not affect the distinctness of the forms, as the difference between a species and subspecies is not one of degree of difference, but of the presence or absence of intergradation along the line separating their ranges. It must in many cases be largely a matter of opinion, which rank a given form should take. Hasty action like that of Mr. Thayer's, without the examination of adequate material, is responsible for much of the shifting of names back and forth which has become such an abomination in modern systematic zoology.

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September 4, 1915

[1] *Bull. Brit. Ornith. Club,* May 26, 1915.