



The Principles of Dynamics

Author(s): W. Larden

Source: *The Mathematical Gazette*, Vol. 3, No. 60 (Dec., 1906), pp. 385-394

Published by: Mathematical Association

Stable URL: <http://www.jstor.org/stable/3602513>

Accessed: 24-12-2015 00:57 UTC

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



Mathematical Association is collaborating with JSTOR to digitize, preserve and extend access to *The Mathematical Gazette*.

<http://www.jstor.org>

THE MATHEMATICAL GAZETTE.

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.; PROF. H. W. LLOYD-TANNER, M.A., D.Sc., F.R.S.;
PROF. E. T. WHITTAKER, M.A., F.R.S.; W. E. HARTLEY, B.A.

LONDON :
GEORGE BELL & SONS, PORTUGAL STREET, LINCOLN'S INN,
AND BOMBAY.

VOL. III.

DECEMBER, 1906.

No. 60.

THE PRINCIPLES OF DYNAMICS.

I. The object of this Paper.

(1) The writer is not attempting to construct a new system of dynamics, but merely to present in a clear form what he believes to be the principles of our existing system; and he ventures to hope that such a presentation may be of some use to those teachers of dynamics who are not specialists, and perhaps to older students. The paper is not intended for experts.

It has appeared to him that in the various text-books written on this subject, these principles are at times forgotten; with the result that there are to be found many faults, some due to incompleteness, some to vagueness, some to reasoning in a "vicious circle," and some to the attempt to make one equation give us two or even three unknowns.

(2) As an example of the last, it may be mentioned that in one text-book the second of Newton's laws appears to be regarded (i) as an experimental law giving the observed actions of forces (measured statically) on masses (measured independently of the observed effect); (ii) as defining force; (iii) as defining mass; (iv) as giving us a means of measuring time.

(3) So again there appears to be much vagueness as to the choice of origin and axes, and consequently much uncertainty as to what is meant by velocity and acceleration, by displacement, work, and kinetic energy.

II. The conditions to be fulfilled by a rational system of dynamics.

(1) *Force*. It is easy by means of spring-balances or by some other statical method to determine multiples of any arbitrarily-chosen unit of force. It may be objected that springs change in

R

properties as time goes on. But what more exact methods are there than statical methods of actually *obtaining* a series of forces of known relations to one another? One may *define* forces dynamically, but one does not *measure* them dynamically in practice.

Even those who profess to define and measure forces dynamically by means of Newton's laws do yet in fact refer back to the statical measurement. If a body does not "persist in its motion with uniform velocity," they are not contented to say "then there must, by definition, be a force acting on it"; but they look about for some other body exerting on the first a force measurable statically. In all statical mechanics we measure forces by the spring-balance or by some equivalent statical method; and in the dynamics of machines we measure statically the forces producing the motions considered. We should not be satisfied with, nor could we work with, any system of dynamics in which the measures of the forces estimated dynamically did not agree with their measures estimated statically.

For this reason, in discussing Newton's laws, the writer will assume that the relative magnitudes of the forces are determined statically. These laws once established, we may then avail ourselves of them to obtain a suitable *definition* of the unit of force.

[In this connection see Thomson and Tait's *Natural Philosophy*, Vol. I., Part I., §§ 207, 248, 258, third paragraph, 220 and 223 [ed. 1879]. Also see XII., (2) in this paper.]

(2) *Mass*. The root-idea of *mass* in dynamics is no doubt *inertia*; i.e. the passive resistance offered by matter to change of motion.

Yet—[See Thomson and Tait, Vol. I., Part I., §§ 208, 209, 216]—the Newtonian conception of *mass* was such that its measurement was regarded as independent of dynamical experiment. And there is no doubt that we should not be satisfied with, nor could we work with, any system of dynamics in which dynamical measurement did not give (*e.g.*) to 5 pints of water five times the mass that it gave to 1 pint.

The writer believes, then, that in discussing Newton's laws he is justified in assuming that the masses spoken of are to be thought of as measured, and their centres of mass as determined, independently of any dynamical experiments; though, these laws once established, masses may be defined dynamically.

(3) *Definiteness of meaning and significance*. A further condition to be fulfilled will be discussed in VIII. and IX. Suffice it here to say that we expect our system of dynamics to give us definite and significant results where the data are, as far as we can judge, definite and sufficient.

III. Velocities and angular velocities.

"All motion that we are or can be acquainted with is *Relative* merely." [Thomson and Tait, Vol. I., Part I., § 45.]

"When we turn our attention to the process of change" . . . (in a system) . . . "itself, as taking place during a certain time and in a continuous manner, the change of configuration is ascribed to the motion of the system." [Clerk Maxwell, *Matter and Motion*. Art. xxiv., ed. 1876.]

(1) That it is not "slaying the slain" to point out how meaningless to us is the expression "*absolute velocity*" is shown by the recent appearance in a well-known scientific journal of "A plea for Absolute Motion." ["Nature," Vol. 73, p. 463.]

(2) No one can deny that our minds can set no limit to space; nor can we say "beyond this or that limit there is no matter." Our whole universe of visible stars may be only one of a countless multitude of such universes; the solar system may appear to "move" this way or that through our universe of stars; that universe of stars to "move" this way or that through a cloud of other universes; and so on to infinity. What meaning *can* "absolute motion" convey to our minds?

(3) *Rotation kinematically considered.* So again we can imagine system outside system, each with rotation relatively to its neighbours. But what kinematical meaning *can* we assign to the expression "absolute rotation"?

(4) *Rotation dynamically considered.* At first sight it would appear as though the phenomena of dynamics do give us a conception of absolute rotation.

We believe it to be established that when a mass of homogeneous fluid has rotation relatively to axes which are "fixed" with respect to distant stars, then it assumes, or oscillates about, a spheroidal form; and that when two bodies of matter rotate round each other relatively to the distant stars, in either direction, then the apparent attraction between them, as measured by the strain of a massless spring connecting them, would be less than it would be were there no such rotation.

But what more can we say than this? May not "rotation relatively to distant stars" mean "rotation *relatively* to the ether of our universe"? We cannot answer this.

One result remains. Dynamically we cannot define even arbitrarily what we shall call "absolute velocity"; but dynamically we *can* define what we shall call "absolute rotation," though we cannot deny but that the definition may be quite arbitrary.

(5) "*Fixed*" axes. In dynamics we shall define as "fixed axes," axes that are fixed relatively to the distant stars. Or, again, if a body of homogeneous fluid, assumed to be isolated, preserves a spherical form when it has no rotation relatively

to certain axes, we may call these axes "fixed." We believe these two definitions to be equivalent to one another; but in practice we always refer to distant stars as a criterion of rotation in its dynamical sense.

(6) When, in the sequel, we speak of "*the velocity, or acceleration, of any point relatively to any origin*," fixed axes through the origin will be understood. Without these axes the expression italicised would be meaningless.

[(7) Though somewhat irrelevant, it may not be without interest to enquire here the significance of the statement that "the earth rotates round the sun, and not the sun round the earth."

Kinematically the statement has no meaning; as is seen by taking earth and sun in turn as origin;—and why should we not?

Dynamically the statement *has* significance. The relative motions of sun and earth will come best under the rule of dynamical law if we refer to their centre of mass (which lies within the sun's surface) as origin, and regard the sun's centre as describing a small ellipse, the earth's centre a large ellipse, round this point. More significance than this there could not be, unless we can attribute meaning to the expression "a point fixed in space." See VI. (1).]

IV. The Experimental Nature of Dynamics.

(1) It is possible to build up such a system of dynamics that the results arrived at *must* be true; that "Dynamics," in fact, may become a branch of mathematics. Sooner or later, however, when the science is applied to realities, there will arise the question whether or no the forces and masses of this logical dynamical system work in with other and equally important conceptions of them; and we come back to experiment after all. [See II. (1) and (2).]

(2) It seems best, therefore, to start with the conceptions of, and measures of, forces and masses discussed in II., and to examine by means of direct and indirect experiment the motions produced.

(3) Other questions also arise, to be settled by experimental evidence. Is there any simple relation between the masses of bodies and their gravitational attraction for one another? Is either "mass" or "weight" affected by the shape or physical condition of a body? Has gravitational attraction a finite velocity of propagation? Can we, in dynamics, assign a meaning to "constancy in direction of velocity"?

(4) *The "Laws" of Dynamics.* Thus in dynamics as in physics we must regard our laws as based upon experiment. As in physics, they are "Laws" only in the sense that they

account in a simple manner for all phenomena hitherto observed, and enable us to predict other phenomena, so that they are themselves capable of being tested by experiment.

Our faith in them is justified, not by rigid mathematical demonstration, but by cumulative evidence direct or indirect.

If Uranus "go wrong," we look for, and we find, Neptune; and we have so much more evidence in favour of our laws.

V. Preliminary, rough, Experiments. Newton's Laws. Mass and Weight.

(1) It is by experiments performed on the surface of the earth that we are guided towards the general laws of dynamics.

We start with forces and masses, conceived of and measured as explained in II., (1) and (2); we refer velocities, accelerations and displacements to the surface of the Earth and to landmarks on it. Such experiments are rough, but they give us valuable results.

(2) If we deal at first with one body (free to move) at a time, we arrive at the conclusion that . . . (writing "mass," "force," "acceleration," for the measures of these quantities) . . .

(mass) \times (acceleration) \propto (force applied), and the acceleration takes place in the direction of the force.

(3) If now, still dealing with one mass (free to move), we apply several forces to it, we conclude that . . .

(mass) \times (acceleration) \propto (statical resultant of the forces), and the acceleration takes place in the direction of this resultant.

(4) If now we have two masses free to move, and cause a stress to be exerted between them, as (e.g.) by means of a light stretched elastic cord, we conclude that . . .

*the mass-accelerations are equal, and opposite in sign;
the centre of mass does not move.*

(5) *Mass and weight.* We recognise in matter two dynamical properties; viz. *inertia*, or passive resistance to change of motion, measured by "*mass*"; and *mutual attraction* between two bodies of matter. When one body is the earth, and the other is a relatively small body on or near to the earth's surface, we call the one side of this stress, or the earth's pull on the body, the "*weight*" of the body.

For some reason, unknown to us, *weight* and *mass* appear to be proportional to one another. Also, for some reason unknown to us, both appear to be independent of the shape and physical condition of any given piece of matter.

[It seems strange, to the writer, that there are to be found those who object to the use of *two* words, *mass* and *weight*, to designate two such entirely different properties.]

(6) Newton's laws being once established, it has seemed better to start with the fundamental units of time, mass and length, and to derive the unit of force. In the engineers' system, however, we virtually start with the units of time, length, and force (the latter being the earth's pull on a standard lump of matter at London); and derive the unit of mass.

As to the unit of time, we derive it from the earth's rotation relatively to the fixed stars. If in the lapse of years or centuries our laws appear to be changing, we ascribe this to a change in the earth's rate of rotation rather than to a change in the masses of sun or earth, or to a change in the attractive stress between them. Other standards of time have been suggested; but it would be out of place to discuss them here.

The view that the writer would lay stress on is that *we attempt to frame laws that shall make everything "hang together."* Being essentially experimental laws, they cannot be proved with mathematical rigidity; they are but working hypotheses.

VI. More complete statement of the Laws of Dynamics.

(1) Starting with the results suggested by experiments of the nature indicated above, we proceed to consider more carefully the question of the origin and axes to which, in any problem, velocities, etc., should be referred.

The conclusion arrived at is that Newton's laws hold good if we:—

(i) take as "fixed" axes those which are fixed relatively to the distant stars, and judge of change of direction of velocity by these;

(ii) choose as origin either the centre of mass (*c.m.*) of the bodies between which the reactions considered take place; or any origin with respect to which [see III., (6)] this *c.m.* has uniform velocity. For reasons given in VIII., (3), and X., (3) and (7), it appears to be better to choose as origin the *c.m.* of the reacting bodies when we are examining only the reaction between them.

If, then, we use "acceleration" in the more general (or vectorial) sense, we find that Newton's laws account for such motions as those of the solar system.

The writer would emphasise the view that *our laws of dynamics deal essentially with the system of bodies between members of which the actions considered take place, with an origin as specified above (preferably their centre of mass), and with fixed axes;* and not with "isolated bodies" and "forces acting on them."

(2) *Example.* Consider the case of two bodies of masses M and m respectively, there being between them a stress F that changes when the distance changes. And let their acceleration,

velocities, and displacements, all referred to their *c.m.* and to "fixed" axes be A and a , V and v , S and s , respectively.

Then our laws of dynamics—(whose principles are here being stated, not proved)—assert that

- (i) *the mass-accelerations are equal and opposite, and each measures F .*
- (ii) *the changes in momenta of the two are equal and opposite, and each measures the time-integral $\int_{t_1}^{t_2} F \cdot dt$ during the period considered.*
- (iii) *the changes in $\frac{1}{2}MV^2$ and $\frac{1}{2}mv^2$ measure the corresponding space-integrals $\int Fds$, or work done; the displacements of M and m respectively along the line of the stress being measured from the *c.m.**
- (iv) *From (i) and (ii) it follows that the mutual stress between M and m does not affect the acceleration or velocity of their *c.m.*, relatively to any other origin and fixed axes.*

(3) *A still simpler example.* Let us consider the two bodies above to be a smooth, un-rifled gun, of mass M , loaded with a charge of measurable potential energy, but of negligible mass, and a shot of mass m . And for simplicity let us assume that the whole has no rotation relatively to "fixed" axes [see II., (5)], and that the charge when ignited burns at such a rate that the stress exerted has a constant value F .

Referring to the centre of mass of M and m as origin, we may say that our laws of dynamics assert that (if we disregard *signs* here, since there can arise no ambiguity of meaning)

$$MA = F = ma \quad \dots\dots\dots(i)$$

$$MV = F \cdot t = mv \quad \dots\dots\dots(ii)$$

$$F \cdot S = \frac{1}{2}MV^2 \quad \dots\dots\dots(iii)$$

$$F \cdot s = \frac{1}{2}mv^2. \quad \dots\dots\dots(iv)$$

whence
$$\frac{V}{v} = \frac{m}{M} = \frac{S}{s}, \quad \dots\dots\dots(v)$$

where V and v are the final velocities, and S and s the final displacements of the masses M and m respectively, relatively to their *c.m.*, during the action, and t is the time of action (duration). Evidently also, $F(S+s) = \frac{1}{2}MV^2 + \frac{1}{2}mv^2 = \text{total work done by the charge on gun and shot together.} \dots\dots\dots(vi)$

VII. The work done by the gases.

(1) It is in considering *work* and *kinetic energy* that errors, due to neglect of definiteness as to the origin to be chosen, most

frequently arise. So we will consider this matter further here and in VIII., IX., and X. All will agree that $F \times (S+s)$ in (vi) above measures the total work done by the charge on gun and shot, and that consequently $(\frac{1}{2}MV^2 + \frac{1}{2}mv^2)$ will rightly be called the gain in K.E. of the system.

(2) Though the gain in K.E. of the system, and the total work done, are the main point, still it is not without interest to enquire in what real sense we may call $F.s$ the work done on the shot, $F.S$ the work done on the gun, $\frac{1}{2}mv^2$ the K.E. of the shot, and $\frac{1}{2}MV^2$ the K.E. of the gun, in the system. But in making this enquiry it must not be forgotten that our dynamics deals essentially with a system; and that "the K.E. of an isolated body" is as meaningless as "the absolute velocity of a body," and "the work done by a force acting through a certain distance" is as meaningless as "absolute displacement." [See Maxwell's *Matter and Motion*, § CIX.]

(3) If gun and shot *came to rest again relatively to their c.m.* by striking and being stopped by sandbanks of relatively infinite mass which are at rest relatively to the *c.m.* of gun and shot, it will be admitted that quantities of heat measured by $\frac{1}{2}MV^2$ and $\frac{1}{2}mv^2$ respectively will be given out. Hence in the system of gun and shot, the masses M and m may reasonably be said to have these respective shares of the total K.E. in equation (vi) due to the explosion.

(4) Or again, not to introduce these infinite masses which are not concerned in the reaction considered, let gun and shot come to rest relatively to their *c.m.* owing to an attractive stress between them, and then return (under this attractive stress) towards one another. When in the initial relative position let there be inelastic collision, a non-conducting mat being interposed. It will be admitted, the writer thinks, that the heats given out on the two sides of the mat will again be $\frac{1}{2}MV^2$ and $\frac{1}{2}mv^2$, respectively, as before; the sudden transition from motion relatively to the *c.m.* to rest with respect to it, being virtually the same in nature as in (3) above.

Here again we see that the masses M and m can reasonably be said to share the total energy of the explosion in the proportion $F.S$ and $F.s$, or $\frac{1}{2}MV^2$ and $\frac{1}{2}mv^2$, *i.e.* in the inverse ratio of the masses.

(5) So again if the two masses M and m oscillate with respect to one another under the action of a (massless) spring of perfect elasticity that connects them, though the total energy of the system is the one thing of which we can speak without ambiguity, yet we may reasonably speak of the spring doing work on the masses of amounts $F.S$ and $F.s$, and of the masses doing work on the spring of amounts $F.S$ and $F.s$, alternately; measuring displacements as before from the centre of mass.

VIII. Lack of significance, or even error, due to failure to refer to the c.m. of the reacting bodies.

(1) Let us now, in the simple case of VI., (3), *refer to an origin relatively to which the c.m. of the two reacting bodies has a constant velocity V .* [See III., (6).]

We find that as regards mass-acceleration, change of momentum, and total work done by the explosion [see X., (1)], we obtain the same results as when we refer to the *c.m.* So that, as stated in VI., (1), (ii), any such origin will for most purposes do as well as the *c.m.* itself. But as regards the distribution of the energy of the explosion between gun and shot, it would seem that we obtain results capable of being interpreted reasonably [see VII., (3), (4), and (5)] when we refer to the *c.m.*, and somewhat meaningless results when we refer to an origin with respect to which the *c.m.* has uniform velocity. For it is clear that by choosing an origin for which V has a suitable value, we may have the shot appearing to retreat before the gases (*i.e.* to have work done on it), to stay still (*i.e.* to have no work done on it), or to advance on the gases (*i.e.* to have negative work done on it); the total work done on the system of gun and shot remaining the same as before. And in view of VII., (3), (4), and (5), such a view appears to be lacking in significance.

In the same way we might be led to speak of a gun of 45 tons being projected *from* a shot of 1 cwt.; of a man thrown by a stone; of steam, "kicking off from" a piston of small mass, driving the cylinder and framework of an engine and the earth to which the framework is fixed; or even of one side of a press doing work on a book, and of the book doing equal work on the other side of the press. And the discussion of the oscillating bodies of VII., (5), would suffer in significance for similar reasons.

In X. the advantage of referring to the *c.m.* as origin appears in a somewhat different shape; while in XI., (5) and (6), it is seen how such reference leads to the ordinary treatment of the cases, of very common occurrence, in which the one of two reacting bodies is of relatively infinite mass.

(2) Let us now, in the simple case of VI., (3) *refer velocities, etc., to an origin with respect to which the c.m. has an acceleration* [see III., (6)].

We now have not only lack of significance but also error; all is in confusion. Let us, for example, take the gun or the shot as origin; both having acceleration relatively to the *c.m.* during the action of the stress.

Referring to the gun as origin we should get, for the motion of the shot, $F = m(A + a)$; while, referring to the shot, we should get for the motion of the gun $F = M(a + A)$; where F should be one and the same force. And for the K.E. due to the explosion we should get either $\frac{1}{2}m(V + v)^2$, or $\frac{1}{2}M(V + v)^2$; different values.

Another error, an error in estimating the total gain in K.E. during the explosion, if this occupied a finite time, is indicated in X., (6). With such an origin, then, we have actual error.

(3) We see then that if we are to obtain results that are in accordance with the laws of dynamics we must choose an origin relatively to which the *c.m.* of the system has uniform velocity; and that if we are to give a *reasonable* account of the distribution of the energy of the system between the various masses composing it, we should refer to the *c.m.* itself as origin.

Hence it is better to refer all displacements, velocities, etc., to the c.m. of the system of the bodies between which the reactions take place when discussing these reactions; though for most purposes any origin with respect to which this c.m. has uniform velocity will do equally well [see III., (6)]. W. LARDEN.

(To be continued.)

NOTES ON THE THEORY OF THE REVERSIBLE PENDULUM. (PART II.)

[Part I. appeared in the May issue under the title "*On the Adjustment of Kater's Pendulum.*"]

7. We may now turn our attention to the questions which arise specially in connection with a reversible pendulum, such as Kater's. Of course the object in view is so to place two knife-edges on opposite sides of the C.G. that the period shall be the same from either; the two positions being selected according to the rule at the end of §4. But the usual procedure will be to decide on the distance between the knife-edges at the start, say one metre; then to clamp them to the more or less uniform bar of the pendulum, using a standard distance-piece to secure the proper interval between them; and finally to adjust a sliding weight in such a position as to make the period the same for both. We have seen in §2 how the period, or rather the length of the equivalent S.P., varies with the position of the sliding weight; but it will be convenient now to change the notation. Let I_1, I_2, \dots denote the moments of inertia of the various masses making up the pendulum, each about its own C.G.; M_1, M_2, \dots the masses; x_1, x_2, \dots the co-ordinates of the centres of gravity, referred to one of the knife-edges as origin; let the co-ordinate of the other knife-edge be d ; and let the letters I, M, x without suffixes refer to a moveable mass which is being adjusted. We shall at once see how much simpler it is to discuss the graph whose ordinate is the length of the S.P., than the one where the period is used; for the position which the moveable mass should occupy of course corresponds to the intersection of the two graphs relating respectively to the two