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RATIONAL MECHANICS

CHAPTERS IN MODERN DYNAMICS AND ENERGETICS

BY

Lieut.-Col. RICHARD DE VILLAMIL, R.E. (RET.)

AUTHOR OF "MOTION OF LIQUIDS," "RESISTANCE OF AIR," ETC.

"I do earnestly beg that all be read with candour, and the defects in a subject so difficult may be not so much criticized as cleared up and generously made good by further essays on the part of my readers."

NEWTON'S Preface to the *Principia*

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WITH PROFOUND RESPECT

TO THE IMMORTAL MEMORY OF

NEWTON

THE FATHER OF MODERN MECHANICS

PREFACE

I HAVE been asked:

Why I call this a book on "Rational Mechanics"? and

Do I not consider all Mechanics as being "Rational"?

Using the word "Rational" in its Dictionary sense of "Agreeable to, or consistent with, reason," I consider that much of it is hardly "Rational."

Mechanics, as at present taught—and I refer more particularly to Hydromechanics—is generally divided into "water-tight compartments"; and what is taught in one compartment is, not infrequently, in direct contradiction to what is taught in another compartment.

In order to be precise, let me give some definite examples in illustration of my meaning, and in support of what may be considered a very strong statement. The student is taught, in every book on Mechanics, the Laws of Motion; the second of which is that, in a given time, Force [*i.e.*, Action, Resistance, etc.] is proportional to the change of Momentum.

At another time the student is instructed concerning the well-known Theory of the Discontinuous Flow of a fluid; a theory first imagined by Helmholtz and subsequently elaborated by Kirchhoff and Rayleigh. In this flow there is *admittedly* a resistance—the formula for calculating it is given—yet there is *no change of momentum* in the fluid. Thus, in this case, the change of momentum is always *nil*, whilst the Resistance may be increased to any extent desired!

How these two statements can be reconciled is not evident. In consequence, however, of the eminence of the great names I have quoted, this Theory has received an amount of attention which seems to me quite beyond its merits. I consider that no more paradoxical Theory of Resistance has ever been propounded in Hydromechanics. It is dangerously near Perpetual Motion.

Such Mechanics I consider "Irrational." Take another case—The student is put through a course of Vector Analysis. He here learns the difference between Vector and Scalar quantities, and he is told that they are quite separate and distinct in their nature; so different, in fact, that *in no case can they be equated.* Most ordinary text-books on *Mechanics* make no reference to Vectors at all; and, at the same time, quantities which are Vectors and Scalars are *very commonly equated*: *e.g.*, Momentum and Energy are, very ordinarily, equated. Such equations are, clearly, not homogeneous. There is no difficulty in finding such books; the difficulty is to find a Text-book on *Mechanics* where *this is not done.*

Such Mechanics I do not consider "Rational."

Still another case! The student is taught the "Principle of Similitude"; and by this Principle the Resistance which a submerged plate encounters when moving uniformly and orthogonally in a fluid is shown to be measured by the area of the surface of the plate. In Aerodynamics, however, he is taught that the Resistance increases *more rapidly* than the area of the surface of the plate. This is equivalent to stating that the Principle of Similitude is not correct. Can one consider this "Rational"?

The question of the Resistance of submerged bodies moving uniformly in Fluids is at present in a very confused state; if the motion is *accelerated* the confusion is still greater. This, I fancy, is largely due to the modern desire to express the Resistance by means of a Monomial Equation, involving v *only.** As pointed out by Newton, this Resistance can only be expressed by a Binomial, which involves the first and second powers of the "Factum" (*lv*). The result is that, at the present day, the mathematicians have to admit that they cannot calculate *directly* the Resistances of *even the simplest bodies*, all the (so-called) calculations having to be corrected by means of arbitrary "coefficients"—*derived from experiment.* This seems very like what schoolboys would call "fudging"! I cannot call this "Rational"—I should be more inclined to call it "Artful."

In these chapters I have confined myself to following our

* This violates the Principle of Similitude at once: but this is not thought of.

great Newton; calculation then becomes simple, and very accurate results can be obtained without the use of any "Fudging Coefficients."

I might give many more cases, but I will confine myself to pointing out another practice which I cannot consider "Rational." It is a very common practice (in Aerodynamics especially) to take a small portion of an *accelerated motion* and to treat it as if it were a *uniform motion.* If a motion is an accelerated one, it is not easy to understand how a small portion of it can be otherwise than an accelerated motion also!

Other reprehensible practices are criticized in this small book; especially the use of one word in a variety of senses, *in the same argument*: notably, the word "Force," which is commonly used in four senses. I have seen it used in three senses on one page! I trust that the Reader will find that no part of this book contradicts any other part; and that I employ all technical words *in one sense only*; and that this sense is carefully *defined.*

That this book may be considered as "contentious" I quite expect. I might even confess that it is intentionally so; my object being to stimulate the Reader to think for himself, and not, necessarily, to bow down to "Authority." I do claim, however, and very strongly, that it is not, in any sense, *Revolutionary*, since it is based on the solid foundation of the teaching of the greatest authors, from Newton downwards. Though there may not be very much that is either new or original in these chapters, nevertheless, there is a great deal that the student who studies only Text-books will find new, and, I trust, well worth study and attention.

My division of Mechanics into two branches—a *Deductive* and an *Inductive* branch—is, I fancy, new. In any case I do not know of anyone having definitely suggested it before, although its advantage—shall I say, its "Rationality"—appears fairly obvious. Certainly M. Gandillot in his writings (Gauthier-Villars, Paris) implicitly suggests such a division; and Sig. G. Casazza (*Einstein e la Commedia della Relatività*) also suggests it. These authors distinguish between what they call *Mechanics* and *Kinematics*; Sig. Casazza saying (p. 20) that "Einstein always confuses Mechanics and Kinematics"—*i.e.*, "Energetics and Pure Dynamics."

I have brought forward the Principle of Least Action, or Least Resistance, very prominently. This Principle, perhaps only second in importance to that of the Conservation of Energy, appears to be very little known, and is hardly referred to in Text-books, E. Mach's book on the *Science of Mechanics* being a brilliant exception.

I have only cursorily referred to the latest addition to Energetics, called the " Quantum Theory," or Theory of " Quanta " ; *i e.*, the Theory that all Energy is transformed by " jerks," or " jumps," at *regular intervals*, and in *constant quantities*. I have, however, pointed out that this is a *logical deduction* from my explanation of viscosity, and *how* the molar kinetic energy is transformed into the " shriller varieties " of Heat, Light, and Electricity ; these being generated in *Quanta*. The Theory of Quanta fits in perfectly with Newtonian Mechanics ; and is, indeed, a *logical necessity*. I even push the logical deduction further, and say that in Energetics there is no such thing as *uniform motion* ; that all motion is accelerated by minute " jerks." In Pure Dynamics, of course, uniform motion is *assumed* ; and there is an end of it. I have pointed out that Bertrand Russell holds that the Theory of Quanta cannot be reconciled and explained by Einsteinian Mechanics.

I have thus milked many cows, but the *cheese is my own*. My object has been, not to introduce innovations, but to try to introduce some system into the heap of facts discovered by others—but which have been neglected. My chief aim is to interest the student, and not to bore him.

Let me conclude with a few pleasant words. I wish to express my indebtedness to the Secretaries of the Royal Society for allowing me access to the Society's magnificent collection of Newtoniana and kindly giving me permission to photograph. My frontispiece is reproduced from a pencil drawing in that collection, made by G. P. Harding from the " ivory portrait " by Le Marchand, which hung in Newton's dining-room. I have also to thank my friend, Mr. Hastings White, for much helpful collaboration in preparing the MS. for the press and in revising the proofs.

R. DE VILLAMIL.

CONTENTS

CHAPTER		PAGE
I.	INTRODUCTORY	1
II.	DIMENSIONS ; SIMILARITY	13
III.	VECTORS AND SCALARS	26
IV.	DERIVED UNITS	37
V.	RELATIVITY	50
VI.	THE NOTION OF FORCE	65
VII.	THE NOTION OF FORCE (*continued*)	78
VIII.	ENERGETICS	96
IX.	VISCOSITY AND RIGIDITY	112
X.	RESISTANCE	126
XI.	RESISTANCE (*continued*)	140
XII.	DUBUAT'S PARADOX	155
XIII.	PRINCIPLE OF LEAST ACTION	169
XIV.	SHAPES OF LEAST RESISTANCE	183
XV.	SHAPES OF LEAST RESISTANCE (*continued*)	199
INDEX		212

RATIONAL MECHANICS

CHAPTER I

INTRODUCTORY—THE STUDY OF MECHANICS

In 1905, at Johannesburg, during the meeting of the British Association, there was a discussion on "The Teaching of Elementary Mechanics."

Professor Perry, who opened the discussion, commenced with the following, rather scathing, remarks: "I may disagree with other speakers in believing boys to be so uneducated at present that for one of them to learn Mechanics is almost miraculous. I mean to speak only of those boys of the future who may be fit to study the subject. Again, I shall probably differ from other speakers in what is meant by 'a knowledge of Elementary Mechanics.' I do not consider the boy who passes so easily the ordinary examinations in Mechanics, so quickly answers questions concerning Attwood's Machine, for example, to have the knowledge I speak of. He remembers a few formulæ, but he has no comprehension of the simplest mechanical principles, and when he forgets the formulæ, as he very soon does, he forgets everything, and dislikes the thought of mechanical theory."

* * * * * * *

"The average man who once upon a time was crammed for examination by formulæ is found to know nothing of kinetics, although he may retain some of his knowledge of graphical statics.

"I need not speak of a third kind of man, the very mathematical man; he often *does not know anything of*

Mechanics; it is the subject of Applied Mathematics that he has studied and that he cares for.

"Huxley prophesied what has since come to pass; that if the colleges of London became constituent colleges of one University, all true education in London would begin to disappear. How correct he was. *Initiative and originality are gradually disappearing from courses of instruction.* London University is a manufactory of B.Sc.'s, who resemble one another exactly as buttons do. As regards such a subject as Mechanics, the examination system requires that a student should have labour-saving rules for every kind of problem, enabling him to give an answer in a very limited time. Such labour-saving rules can only be remembered by a man who is always teaching the subject; they are soon forgotten by an engineer, who blames himself and *loathes the subject ever after.* Besides, *they destroy the power of thought.*"

Some of the remarks of other speakers were hardly less severe. Certainly, one of them said apologetically: "We must not always blame the teacher if the boys do not do as well as they might. We should blame the system which overloads the competent teacher with a lot of dull boys." To this, however, Professor Perry replied: "The man who cannot teach the average boy ought to be called an incompetent teacher, who dishonestly takes money from parents for doing work which is never done."

Professor Hicks said: "One of the great difficulties of students in dealing with Mechanical problems—I am speaking of the ordinary student—is that they will not realize how simple the Mechanical question is. They think it is something up in the air and out of reach, and they will not apply common-sense reasoning to it."

Professor Hicks, I venture to think, very clearly points out one of the chief difficulties of the subject, when he says: "With statics the first ideas are complex and sophisticated —a force is a thing which cannot be seen or realized when bodies are at rest. The student is led to think of force as *something which resists his will to move a body*—an idea which is the cause of *endless confusion* in his mind, and the origin of the dynamical nonsense and obscurity of view so often displayed by correspondents in engineering journals."

I cannot help thinking that this was illustrated by the next speaker who said: "Work is a process, energy is a faculty. Energy is the result as well as the source of work, but work is not energy, nor is energy work. If you once differentiate between work and energy, you want a wider definition than is furnished by the text-books. Work can always be defined as the product of alteration of space or rate of motion by another factor which corresponds to force. This may be linear alteration, in which case we have linear force; it may be alteration in volume, implied by pressure; it may be change of surface, implied by surface tension."

Also, "If Mechanics simply, in teaching the theory of force, takes account only of the conservation of momentum, it fails to take account of the second aspect of energy, *viz.*, what Lord Kelvin calls the dissipation of energy. In Mechanics we can take account of both of these principles if we divide force into two kinds: reversible and irreversible. A reversible force is the weight of a body. You do work in lifting the body, and in returning it does work. The pressure of an elastic liquid is of the same kind. You do work in pressing the liquid, and as it returns to its original state the liquid does work."

I am afraid that this collection of words conveys no clear impression of any kind to my mind. It is not evident, for example, what is meant by the expression "alteration of space"; nor how this is, apparently, equivalent to "rate of motion." Neither is it clear what a "reversible force" is.

W. H. Macaulay (King's College, Cambridge) made the following, very pertinent, remarks: "No one can master anything without thinking about it independently, and I should hesitate to smooth down any road which is laid out to students, in such a way *as to discourage independent thought.* But there are certain things, which we have all had experience of, as to which, when we come upon them by a laborious and circuitous route, we feel that surely, if this be true, we might have been told it at once; and I think cases of this kind occur in the study of dynamics. There is too much tendency to mask what is really known of the subject under a mass of somewhat complicated relations between things which might to a greater extent be each hung on its own nail."

Professor Armstrong certainly touched one of the weak points in education when he said: "Everything in education is arranged at present from the point of view of the grown-up who has *lost touch with childhood*; we do not administer what is *proved to be assimilable*, but what we in *our grown-up wisdom* think *should* be. . . . . Some schoolmasters finding boys unable to appreciate what they regard as reasoning, set them down as incapable of any form of reasoning."

Eleven years later (November 8th, 1916) Mr. James Swinburne, in a lecture on "Science and Industry," at King's College, showed that his views were very similar to those of Professor Armstrong, when he said: "Most people think that professors and schoolmasters are experts on education, but you might as well say that a shunting-engine is an expert on locomotion; it goes back and forwards along the lines for which it is designed.

"The teacher's position on a pedestal, his contact with the immature mind only, and his *immunity from contradiction* tend to foster the rapid growth of the germ which leads to the ruin of educational institutions. Teachers are drawn from the least practical and most pedagogical men; and the repetition of this process transforms an absolutely practical and sensible school into one which gets more and more senseless *until it gets down to the ordinary state.*"

I think I should sum up the faults in our education, in Mechanics and Dynamics, as follows:

1. The subject is not, essentially, a very difficult one, but young students are not encouraged to use common-sense and to *think for themselves.* They are usually only shown one side (the master's side) of a question, and are expected to accept this as gospel; whereas the subject may be a very debatable one. Let me give an example of what I consider *good* teaching.

A gentleman, who was head of a college, told me that he one day gave a lecture on some subject—geology, I fancy—where there were three separate and distinct theories. He argued the first out to the best of his ability; then did the same with the second; and also with the third. When he

had finished he said: "Now, boys, write down which theory you like best."

Some wrote down one, and some another. Later, however, one boy asked him: "But which, sir, are we to believe?" He replied: "Whichever you like." "But, sir," persisted the boy, "are you not here to teach us?" The master replied: "I am here for nothing of the sort, but to teach you how to weigh and consider evidence."

This master's teaching produced, as I can readily believe, excellent results. He taught his pupils the scientific spirit, and he *made them think for themselves.* This is really "education"; a "leading out," rather than a system of "cramming" with facts and formulæ.

2. The subject (and this applies very specially to both Mathematics and Mechanics) is generally presented to the young student in a manner which is repellent to him.

Remembering how I was taught, I trust I may be excused for quoting a personal reminiscence in illustration of my meaning.

Many years ago a young friend of mine commenced Trigonometry at school. As is not unusual, he wrote to his mother that "Trigonometry was all rot, that he did not understand it, and that he did not see the use of it." Half the term passed, when one day I got him alone and asked him where his difficulty was. He repeated that the subject was "all rot, etc." I asked him what he had learned, and he said: "Nothing." This was satisfactory, in a way, for he had nothing to unlearn. I then put a few questions to him, gradually getting correct replies. After about an hour he said: "Oh! But this is not the way I have been taught Trigonometry. This is quite easy; anyone can see that." I replied: "That is not *the way* you have been taught, but it is the way you *ought to have been taught.*" He went back to school, and at the end of that term got *very nearly full marks* in Trigonometry! I had, certainly, not taught him much, but I had shown him *how to learn*—which is the last thing that is generally thought of. I had *made him think for himself;* and he saw that the subject was neither so very difficult, nor so very uninteresting as he had, up to the present, imagined; he also *saw the use of it.*

3. I think boys are absolutely bored by the vast number of " examples " (which are to be found at the end of each chapter of most text-books), which examples they are compelled to work out. They get to loathe this monotony, and to hate the subject for ever afterwards. I know a distinguished professor of Mathematics who admits that he hates Algebra, because he had to do so much of it in his youth—and I am not surprised.

" There is a fallacious notion, founded upon pure want of observation, that human beings are unable to form ideas, or to think for themselves until they have been put through an elaborate course of mental gymnastics. A great deal of the process, misnamed education, is directed towards this end, with the result that in nine cases out of ten the brain is simply paralysed and rendered incapable of performing its proper function." (Harold E. Gorst, *The Curse of Education*, 1901.)

Mathematics and Mechanics would be vastly more interesting if they were taught more rapidly; passing from one subject to another when a student had got a fair smattering of the last one. This is not to be understood as recommending superficiality. Quite the reverse; I am a great stickler for *accuracy* (some critics may think too great). I think, however, that a superficial survey of the whole subject, *at first*, would create an interest; after which, all the links in the chain could be well riveted up—*and with pleasure.**

4. The omission of some teaching on the subject of " vectors " and " scalars," when a boy is learning Mechanics, is a very serious neglect; so serious, in fact, that I do not see how the student can *really understand* Mechanics unless he knows what vectors are. Not that I mean to suggest that he should, *necessarily*, be put through a course of " vector analysis." He should, however, most certainly learn what a vector is, so that he may always consider whether any quantity is a *vector* or a *scalar*. If half, or even less, of the time which is now devoted to " the parallelogram," or to working out " examples " in Algebra, say, were spent in learning a little about vectors, the result

* " Survey the whole before considering the parts."—Dr. Johnson.

would, I am sure, be beneficial. This was apparently Clerk Maxwell's view, since in his elementary text-book *Matter and Motion*, he plunges the young student very early into vectors; he, further, *never refers to the parallelogram at all!* The necessity for some such knowledge is shown by the fact that, even in some of the best modern text-books, it is not uncommon to find cases where vectors are equated to scalars; such equations are not *homogeneous*, and therefore, not rational.

5. An elementary knowledge of "Dimensions," and of "The Principle of Similitude," should be imparted early. The subject is not difficult, and the advantages to be gained from it are very great. Since I consider it of the first importance, I shall commence the next Chapter with a general survey of the subject.

6. In studying any science, one should always endeavour to know *exactly what one is referring to.* It is necessary, therefore, that all words, or expressions, should be *rigidly defined.* In Dynamics many of the definitions are excessively loose—with a consequent looseness in the language used. For example, the word "force" is commonly employed in four separate and distinct senses, almost indiscriminately. If the reader will excuse my burlesquing this a little—though it is not such a burlesque as it looks—one may say that:

(*a*) Force is a "horse," say—a "thing" which *causes*, or *tends to cause* motion of a carriage.

(*b*) Force is the "tension" on the traces of the harness a "stress," in fact.

(*c*) Force is "proportional" to the weight of the carriage and its motion conjointly.

(*d*) Force is the "rate" at which the horse can cause the carriage to attain a speed of, say, 10 feet per second.

Now, what definition will cover all these different meanings of one word?

Some words in Mechanics are *not even defined at all!* Take "viscosity," for example. What is the meaning of this word? It is sometimes *defined* as "viscidity," or "treacliness." This may sound very simple, but it is *no*

definition at all; we have simply *changed its name,* without, in the very least, indicating what any of the new names really *mean*—without, in fact, conveying *any sort of mental picture!*

"Fluid friction" is another very fashionable but *quite meaningless expression.* It is true that the expression conveys some sort of idea of "rubbing"; but since the molecules of water, say, *do not touch one another,* it is not easy to explain how they *rub one another.* I can only suggest that the word "friction" is used in a Pickwickian sense.

7. One of the consequences of the neglect of the study of "vectors" is that, in Mechanics, two sciences which should be quite separate are confused together; I refer to "Dynamics" proper (pure "Rigid Dynamics"), and "Energetics."

Dynamics has been roughly defined as the "Science of Force." It would perhaps be more accurate to speak of it as the "Science of Momentum"—what Newton called "*de motu.*" Now, since momentum is a vector quantity, it follows that *all purely Dynamical equations* must be *vectorial.*

In "Energetics," or the "Science of Energy," on the contrary, since energy is a *scalar* quantity, all the equations are *non-vectorial,* or scalar.

Although it does not appear to be recognized by the writers of text-books, Mechanics, I repeat, divides naturally into two very distinct branches, which we may call "theoretical and practical," or "abstract and concrete," or "conceptual and perceptual," or, even perhaps, "poetry and prose," or "Dynamics and Energetics."

These two branches are really quite separate and distinct; I might even say that they are *fundamentally different,* since the former is a *deductive science,* whilst the latter is an *inductive science.* In the former we start from certain arbitrary *assumptions,* or propositions, and from these we *deduce,* logically, certain other propositions. In the latter we start from *experience* and build up the science from *the results of observation.*

In Dynamics we *assume* the properties of bodies—or shall we say, the *absence of properties* of bodies; all bodies being supposed to be composed of particles *rigidly fixed together.*

Since rigid bodies could not vibrate, I suppose that we may assume, logically, that they must all be at *zero* temperature —if there actually is such a temperature. Since, also, Dynamics makes no assumption of any "machinery" for stopping moving bodies, it also follows, logically, that *motion is indestructible;* this is what is expressed as the "conservation of momentum."

As long as these rigid bodies are apart they are assumed to attract one another according to the Newtonian Law. When, however, there is impact between them, complications immediately arise. *How* one rigid body could push another rigid body surpasses comprehension. Still, Dynamics being an abstract science, it is *assumed* that they *do* push one another—and there is an end to all the worry!

In Energetics, however, we *assume nothing;* we simply try and find out, by experiment, what the properties of mundane bodies *really are.*

The equations, I repeat, of these two sciences differ, the former being *vectorial* whilst the latter are *scalar.*

Rankine formulated the Science of Energetics in 1855, but his paper does not appear to have attracted the attention it deserves.* As Rankine's terminology (which is, as one would expect from so clear a thinker, strictly logical) is so very different from that ordinarily employed in Mechanics, I have not adopted it, my object being to introduce *as few innovations as possible.*

I have confined myself to the use of the "fundamental dimension," *space,* for Dynamics—thus following Routh's *Rigid Dynamics* strictly; whilst the "fundamental dimension" *length* is necessary for Energetics. We have thus, for Dynamics, *space,* time and mass; and for Energetics, *length,* time and mass.

8. The subject of "Resistance" appears to me to be very imperfectly taught. This is probably due to the attempt to solve Hydrodynamical problems by the use of Rigid Dynamics! The result is, as one might expect, that the problems are often *not solved*—or, very frequently, when they are *supposed to be solved,* the results obtained do not

* The late Pierre Duhem was one of the leading modern exponents of Energetics.

agree with those obtained from experiment. In such cases the calculated results are multiplied by " coefficients," in order to make the results agree. This is a system which the schoolboy would call " judicious fudging."

Most of the problems of resistance of bodies in fluids are, frankly, declared to be insoluble, *since the equations are not integrable.* This, however, is not a correct statement of the facts, once one *thoroughly understands* what is meant by the term " resistance " ; if one looks at the subject from the point of view of " Energetics," the solution *can* be found, and the integrals are, generally, by no means exceedingly difficult ; in many cases they are even childishly simple. I make no pretensions to being a mathematician, yet I can solve a fair number ; and what one fool can do, I suppose another could equally. In my *Resistance of Air,* I have shown how to calculate the resistances of certain bodies whose " surfaces of presentation " are plane surfaces. Some day I hope to show how the resistances of bodies whose " surfaces of presentation " are curved, can be calculated—and *without extraordinary difficulty;* or even the use of " coefficients." All that is required is to calculate the " coefficient of shape " ; and this, for bodies of revolution, is comparatively easy.*

9. The subject of Mechanics is taught, at the present day, too much from an analytical, rather than from a geometrical, point of view. The subject is too much divided up into " equations." In fact, a friend has informed me that there are seventy-two fundamental equations ; and that anyone who knows these can pass any examination in Hydro-Mechanics.

Analytical work has, I cannot help thinking, a strong tendency to destroy imagination, since it is not necessary for the mathematician to have any mental " construct," or " model," in explanation of what is taking place.

D. Creswell, *Maxima and Minima,* Sadlerian Lectures, 1817, says :

* In my *Resistance of Air,* I *assumed* the coefficient of shape of a sphere to be 0·4 ; that is to say, that the resistance of a sphere was four-tenths of that of its circumscribing cylinder. I did not there *calculate it*—as I could very easily have done—since I did not wish to draw the reader's attention away from my main line of argument.

"There exists this manifest distinction between a synthetic proof in Geometry, and an analytic process in Algebra, that in order to comprehend the former *the whole chain of reasoning* must be kept in view, as it is continued from the beginning of the proposition to the end. Whilst in pursuing the latter method the attention is fixed only upon *each single step*, as each of them successively offers itself; and the conclusion is to be admitted independently of all but the last of them, whenever it is arrived at. Stronger *and more unceasing attention*, therefore, is required in the former case than in the latter, and the judgment, as well as the memory, is *called more urgently into action.*

* * * * * * *

"There is, besides, a *more absolute precision* in all the forms and in the language of geometrical disquisition. . . . Thus it appears that even when the same method is used in both, *Geometry affords a better exercise* than Algebra *for the mental powers*. . . . The great praise, which has been bestowed upon the Mathematics as conducing to strengthen the mind, has proceeded from men who lived when Geometry constituted the principal part of them."

Further than this, it not infrequently occurs that certain important and sometimes essential assumptions are never put into the "mathematical machine," and, perhaps still more frequently, the assumptions which are made *explicitly* imply *other assumptions*, which are thus made *implicitly*—and often *unknowingly*.

As an example of the latter case, one cannot select a better one than "d'Alembert's Paradox." M. Léon Lecornu in his classical *Cours de Mécanique*, Tome II, p. 505, 1915, says: "A perfect fluid offers no resistance to uniform motion of a solid.

"This very surprising result constitutes the Paradox of d'Alembert."

I have quoted from probably the leading French text-book on Mechanics; but the same statement may be found in almost any English book which treats of this question.

Now, why this result should be "surprising" is not exactly evident. Lagrange's equations (on which this statement is based) are commonly referred to as the "equations

of motion" ; I venture, however, to suggest that they would be better named " equations of continuity," since Lagrange always *assumes* " continuity." But when one assumes " continuity," one implicitly *assumes* " non-resistance " flow ! It would, therefore, be rather " surprising " if this assumption of continuity did *not* lead to a non-resistant flow.

A " perfect fluid " is, however, generally defined as one which is " devoid of viscosity " ; and it is thus commonly stated that a *non-viscous fluid* would offer no resistance to the uniform motion of a solid. This is not an accurate deduction from the *facts*. Viscosity, as we know, hardly affects resistance at all. A " perfect fluid " (if it is to behave as Lagrange's equations say it should) must be defined as a *perfectly continuous fluid.*

Very curiously, the only real liquid which has ever been made to flow in the manner described by Lagrange (non-resistant flow) is glycerine—a *highly viscous fluid.* In his well-known experiments Dr. Hele Shaw, of course, carefully arranged that the flow of his liquid *should be continuous.*

This and other cases will be referred to again later.

It cannot be repeated too often that you can only *get out of* a mathematical equation that which you have *previously put into it*—knowingly or unknowingly.

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CHAPTER II

DIMENSIONS AND DYNAMICAL SIMILARITY

MAX PLANCK, in his *Eight Lectures on Theoretical Physics*, delivered at Columbia University in 1909 (translated by A. P. Willis, 1915), remarked, very truly, that: "The material with which theoretical physics operates is *measurement*, and mathematics is the *chief tool* with which this material is worked up."

In recording measurements it is necessary to define them in terms of certain *units*. In Dynamics the *fundamental units* are Space, Time and Mass. The dimensions of any term can thus be expressed as having "so many" dimensions in Space, "so many" in Time, and "so many" in Mass. The meaning of this will be quite apparent shortly.

Now, what do we mean by "Dynamical Similarity"? Dynamical Similarity, or Dynamical Similitude, is a branch of the "Principle of Similitude." The aspect of it that I am giving here will, I trust, be sufficient to cover the subject as far as is here dealt with.

Stripped of all unnecessary verbiage, "Dynamical Similarity" means that in all Dynamical equations, all the terms on both sides of the equation shall be *dynamically* similar, or, as a mathematician might say, "the terms of the equation must be homogeneous." So far, so good; but what do we mean by their being "similar"? "That all the dynamical equations must be such that the 'dimensions' of the terms, *added together*, are all the same in Space, Time and Mass." (Routh, *Elementary Dynamics*.) In other words, that all the terms in the equation shall have *exactly equal* "dimensions."

"The mathematical processes of adding, subtracting, and equating possess intelligible meaning only when applied to quantities *of the same kind*. We cannot add or equate masses and times, or masses and velocities, but only masses

and masses, and so on. When, therefore, we have a mechanical equation the question immediately presents itself whether the members of the equation are quantities *of the same kind;* that is, whether they can be measured *by the same unit,* or whether, as we usually say, the equation is homogeneous." (E. Mach, *The Science of Mechanics.*)

Every schoolboy knows that you cannot add apples and marbles. He would also think it absurd to equate, say, ten cubic feet to so many poles or perches, *plus* so many yards of ribbon. One may say:

Volume A = volume B + volume C, but it is as ridiculous to say:

Volume A = volume B + surface C, as it would be to say:

1 cubic foot = 144 square inches.

QUOTIENTS AND RATIOS

" We must now digress for a moment to inform the reader of the precise sense in which we shall use the word ' ratio,' a word which will frequently occur in these pages. We draw a distinction, not ordinarily made by mathematicians, between *quotients* and *ratios,* and likewise we distinguish between the two operations—or rather, to be precise, the two *species of operations*—of which quotients and ratios are results; *division* and *finding the ratio* (or ratiofication, if we may so speak). . . . The justification for drawing this distinction between division and ratiofication is assuredly as great as that for drawing one between " subtertraction " and " detraction "; and it is of especial importance to those who, like ourselves, look askance upon ascribing the rôle of *multipliers* to *any quantities other than the abstract.* We would not, for example, speak of *dividing* one volume by another; for this implies that we may multiply *something by a volume* and obtain *another volume as product.* And we would not say that there was a *ratio of a volume to an abstract quantity;* for this implies that upon *multiplying the abstract quantity by something*—which certainly cannot be another abstract quantity—there is obtained *a volume* as product. From our point of view the only quantities that can be used as *divisors* are the *abstract quantities* (nothing but an

abstract non-zero being capable of dividing a non-zero dividend). . . . A consequent is thus *not necessarily* abstract; and any quantity whatsoever, abstract or denominate, zero or non-zero, may play the part of a dividend in division or an antecedent in ratiofication. The result of an operation of ratiofication, a *ratio*, is *always an abstract quantity*, while the result of an operation of division, a *quotient*, may be abstract or denominate, but will in any event *always be of the same sort as the dividend.*" (Robert P. Richardson and Edward Landis, *Fundamental Conceptions of Modern Mathematics*, p. 34.)

Let me give a familiar example. Suppose that in a restaurant there were 100 chairs and 25 tables. We might divide the *number* of the chairs (100) by the *number* of the tables (25), and get a *quotient* 4, which is abstract like the dividend. We might equally divide the 100 *chairs* by 25 —say into 25 groups of *four chairs* each—the *quotient* is, obviously, *four chairs*, which is a denominate quantity like the dividend. The abstract quantity—a 4 in the case of these chairs—is an *attribute* of the group; the corresponding *concrete quantity*—the four chairs—is the group itself.

The ratio, 100 chairs : 25 tables = 4 chairs : 1 table (since the abstract quantities 100 and 25, *divided*, give the quotient 4) is necessarily an *abstract quantity*.

Another example. If a man walks eight miles in two hours, we say he walks *at the rate of* four miles per hour. We do not divide the *miles* by the *hours*, but we take the *ratio* between the distance and the duration of the time; and we call this *abstract quantity*, the *speed* at which the man walks. This is commonly expressed, by symbols, as:

$$\text{Speed} = \frac{\text{distance}}{\text{duration of time}}$$

which is not an actual *quotient* but a rate, or ratio. We cannot multiply the duration of the time by 4, and obtain a distance of 8 *miles* as a product.

QUANTITIES

As Clerk Maxwell says (*Theory of Heat*) "every quantity is expressed by a phrase consisting of two components, one of these being the name of the number, and the other the

name of a thing of the same kind as the quantity to be expressed, but *of a certain magnitude agreed on* among men as a *standard* or *unit.*"

Put in other words, we may say that the statement of any quantity invariably contains two terms, called *quantitative* and *qualitative;* the latter expressing "what kind," and the former "how much." Thus, when we speak of a length as ten centimeters, we mean that we are to consider a *quantity*, ten, of the *quality*, length; the standard or unit of length being the centimeter.

If the arbitrary "unit" of the dimension be changed the numerical part must also be *correspondingly altered.* For example:

$$2 \text{ days} = 48 \text{ hours}.$$

The unit *day* has been reduced to one twenty-fourth—an hour; consequently, the number 2 must be correspondingly *increased* twenty-four times.

Arithmetic is a purely abstract science; the numbers, in the equations, are numbers of *anything*, or *everything*, $3 + 3$ is always equal to 6.

In Algebra and Dynamics the *numbers* in the equations —for terms are really only numbers—are *numbers of something.* The equation $a + b = c$ can only be *conditionally* true; the condition being that b and c are numbers of *the same determinate quantity* as a; if, in fact, they have the same "dimensions." You clearly cannot say:

$$a \text{ [cats]} = b \text{ [dogs]} + c \text{ [rabbits]}.$$

There are indeed cases where you *apparently* add quantities which are not of the same kind.

$$12 \text{ [cats]} + 24 \text{ [marbles]} = (6+6) \text{ [cats]} + (16+8) \text{ [marbles]}.$$

This is not a real *addition* of cats and marbles; it is really *two distinct equations*, which can be separated—as is conveniently done sometimes.

SPACE

When a body, or a material particle, is moved from one position to another we say that it has been "displaced"; this word implies that the "displacement" is *relative to*

some other body or *bodies*. For example, if a stone in a field be moved, our only means of knowing if, and how much, it *really has been moved*, is by referring its two positions to certain land-marks. It is not sufficient to say that it has been moved ten feet, or any other distance; we must further indicate *the direction of the displacement*—one land-mark is not sufficient. A "displacement," therefore, is a "directed quantity." We may consequently say that displacement is (length + direction).

Geometry necessitates the conception of "space." We cannot define space; it cannot be explained in terms of anything simpler. A straight line may be conveniently considered to be a *space of one dimension;* a plane surface a *space of two dimensions;* whilst a volume is a *space of three dimensions*. We speak of viewing phenomena "in space"; or of viewing them "in time."

Geometrical or Mathematical space is *conceived* as being (1) "absolute," (2) infinite, and (3) infinitely divisible.

The space of our perception—*i.e., physical space*—is neither absolute, nor infinite, nor infinitely divisible.

If we continue dividing a line, or a plane surface, a time will come when we can divide it no more, since our power of sight will fail. Sam Weller's microscope—the "double-million-magnifying-gas-microscope-of-hextra-power"—might, indeed, enable us to push the limit a little further back, but there would always arrive a time when we should reach *a limit*. *Perceptual* space is *not* infinitely divisible; the limit of its divisibility being the limit of our power of *perceiving things apart*. Similarly, the expression, "The infinity of the heavens," is meaningless. Perceptual space, I repeat, is only our mode of *perceiving objects as apart*. "Conceptual" or Mathematical space may be imagined to be ruled with lines at equal distances apart; such lines being, conveniently, at right angles to one another. In a space of two dimensions, only two sets of lines are required; in three-dimensional space, three sets of lines are necessary.

The "dimensions" of unit of "displacement"—space of *one* dimension—being represented as $[S]^1$; in the same manner the "dimensions" of unit of "surface"—space of *two* dimensions—may be represented as $[S]^2$; and the

"dimensions" of unit of "volume"—space of *three* dimensions—as $[S]^3$.

I suggest that these symbols be read S, S *two*, and S *three* (and not S *squared* and S *cubed*), for space of one, two, or three dimensions. If read as "*squared*" the student might think that he was doing Algebra—and that S represented *a number*—so that he could write $[S]^2 \div [S]^1 = [S]^1$. To avoid this error it is customary to put the dimensions within squared brackets; as I propose always doing.

I have, further, adopted the notation employed by Sir Joseph Larmor in his article, *Dimensions of Units*, in the Encyclopædia Britannica, by putting $[S]^2$ instead of $[S^2]$; and also in putting $[M]^1\ [S]^1\ [T]^{-2}$ (the dimensions of "force") instead of $[MST^{-2}]$. Thus, we may say that "force" has one dimension *in Mass*, one *in Space*, and *minus* two dimensions *in Time*.

Number, being pure abstract quantity, has no "dimensions"; consequently any term multiplied by any number will not have its "dimensions" changed. Thus we may say:

Volume A = 2 volume B + 3 volume C, without violating Geometrical similitude.

Clearly, we can also say:

Volume A = volume B/2 + volume C/3.

TIME

It will be unnecessary to define "Time"; everyone knows what is meant by the word, and I am afraid that any attempt at a complete definition would only tend to confuse our ideas.* We might even say that Time, Space and Mass being irreducible, cannot be expressed in terms of anything simpler, and are, therefore, indefinable. We only know of them that which our common sense tells us. As soon as, in order to define these fundamentals, we endeavour to go beyond what is acquired by ordinary experience, we meet with inextricable difficulties and end by acknowledging, as do the philosophers, that they are simply *creations of the mind*, and cover *completely unknown realities*.

Time is what is called in Logic, a *homonymous*, or *equivocal*

* Saint Augustine remarked: "What then, is Time? If no one asks me, I know; if I seek to explain it when I am asked, I do not know."

word; such words as are called by the same name, but used in different senses, or with a different definition or rational explanation.

Mathematical or conceptual Time is, like space, conceived as being (1) "*absolute*" (2) *infinite*, and (3) *infinitely divisible*.

Conceptual Time may be imagined to be a piece of blank paper, ruled with lines at equal distances apart, upon which we may inscribe the sequence of our perceptions, both the known sequence of the past and the predicted sequence of the future.*

A mathematician speaks of a Time as an *absolute* T_0 or T_1—*an instant*, an *epoch;* and when he refers to what may be called *Physical Time*, he defines it as $T_0 - T_1$, say; and he calls it the *Time-interval*. It is true, however, that for convenience in Dynamical *equations* the mathematician's "T" is invariably a Time-interval; though he also frequently employs this symbol to represent an *instant* of Time.

Physical, or Perceptual Time is neither *absolute*, nor *infinite*, nor *infinitely divisible*. It is essentially *an interval*, or a *duration*. If no Time interval can be *observed* between the occurrence of two phenomena, we speak of the occurrences as being "simultaneous."

It is very necessary to note the great difference between the "mathematical" (*absolute*) and the "physical" Time. The same word is used for both, but the meaning that the mathematician frequently attaches to it is not the same as that which the physicist always attaches.

To repeat again: "Space and Time are not realities of the phenomenal world, but *modes under which we perceive things apart*. They are not infinitely large, nor infinitely divisible, but are essentially limited by the contents of our perception." (Karl Pearson, *Grammar of Science.*)†

Father Boscovich, in his great classical work, showed *geometrically* that if we assumed a surface to be infinitely

* Newton, in referring to Space and Time, says: "It will be convenient to distinguish them into Absolute and Relative, True and Apparent, Mathematical and Common."

† Newton, *Principia*, Book I, says:

"All things are placed in Time as to *order of succession*, and in Space as to *order of situation*. . . .

"But because the parts of Space cannot be seen, or distinguished from one another by our Senses, therefore in their stead we use *sensible measures*

great, it would be quite easy to prove that *a part of this surface is twice as great as the whole.* Since such is manifestly impossible he concluded that *the universe cannot be infinitely great.* He says: "I do not admit anything *infinite* in Nature, or in extension, or a self-determined *infinitely small.*"

As regards "infinite divisibility," Galileo, in his *Dialoghi*, showed (also *geometrically*, and very prettily) that if one assumes the infinite divisibility of space, one can *prove* that *the circumferences of all circles are equal.*

In fact, if we once assume "infinities" and "zeros," we can easily be led into all kinds of physical absurdities. "Infinities" and "zeros" only *exist* in the very beautiful "mathematical fairy-land."

The "dimensions" of Time are written [T]. Time, as generally understood, is a *scalar* quantity—we cannot mentally conceive what might be called "side-ways Time."

MASS

What is the meaning of the word Mass? As commonly defined, even in the best modern text-books, the word Mass is used to denote "quantity of matter." Sir R. Glazebrook says: "If we consider the bodies with which we have to deal as composed of matter, then any body will consist of a definite quantity of matter. This quantity is usually called its Mass." (*Dynamics*, 1913.)*

Thus, "Mass" is defined as quantity of a supposed thing called "matter." What, however, is the meaning of the word "matter"?

Glazebrook frankly says: "We do not know what matter is; it may be that it has no phenomenal existence apart from our conception of it." (*Dynamics.*)

Hence, "Mass" may also have no "phenomenal existence"; the definition grows obscure.

Schelling said: "Matter is the general seed-corn of the

of them . . . and so instead of *absolute* places and motions, we use *relative* ones; and that without any inconvenience in common affairs; but in Philosophical disquisitions we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them. For it may be that there is no body really *at rest*, to which the places and motions may be referred."

* I select this book as typical of the best text-books; because its date is 1913, and it is published by the *Cambridge University Press*.

universe, wherein everything is involved that is brought forth in subsequent evolution." His master, Giordano Bruno, having previously declared that matter was not " that mere empty *capacity* which philosophers have pictured her to be, but the universal mother who brings forth all things as the fruit of her womb."

The only complete definition that I have come across (I suppose it must be complete from its length) is that of Hegel. " Matter is the mere abstract or indeterminate-reflection-into-something-else, or reflection-into-self, at the same time as determinate; it is consequently Thinghood which then and there is—the subsistence or substructure of the thing. By this means the thing finds in the matter its reflection-into-self; it subsists not in its own self, but in the matter, and is only a superficial association between them, or an external bond over them." (*The Logic of Hegel*, translated by W. Wallace.)

Such descriptions may be poetical and profound, but they are certainly too nebulous to be accepted as mechanical descriptions.*

I am afraid that we must now fall back on Thomson and Tait's *Elements of Natural Philosophy*, and say: " We cannot, of course, give a definition of *matter* which will satisfy the metaphysician; but the naturalist may be content to know matter as *that which is perceived by the senses*, or as *that which can be acted upon by*, or can *exert force*."

Even this explanation is hardly satisfactory, since, by this definition, ether must be " matter." If we believe that light (and why not heat?)† can exert pressure, it is clear that ether must have Mass—and so be " Matter "—though it cannot be " perceived by the senses." We have, also, Force here referred to as a quasi-personal thing, which *acts* on matter.

* Herbert Spencer's definition, though highly philosophical, is certainly more comprehensible: " Our conception of Matter, reduced to its simplest shape, is that of co-existent positions that *offer resistance;* as contrasted with our conception of Space, in which the co-existent positions *offer no resistance*. We think of a body as bounded by surfaces that resist; and as made up throughout of parts that resist."

† Laplace spoke of the repulsive Force of Heat. Sir William Crookes (*On the Action of Heat on Gravitating Masses*, Proc. Roy. Soc., 1873–4) also gives some very interesting results obtained from experiments made by him and bearing on this question.

As Karl Pearson (*Grammar of Science*), says: "So far then, our analysis of the physicist's definitions of matter irresistibly forces upon us the following conclusions: that matter, as the unknowable cause of sense impression, is a metaphysical entity as meaningless for science as any other postulating of causation in the beyond of sense-impression; it is as idle as any other thing-in-itself, as any other projection into the supersensuous, be it the force of the materialists or the infinite mind of the philosophers."

Since we cannot define "Matter" mechanically, we clearly cannot define "Mass." I trust, however, that the reader will have a reasonable idea of what is meant by the word "Mass," from Thomson and Tait's definition of "Matter."

It will be sufficient here to say that we consider Mass has "dimensions" [M].

The reader who wishes to go more deeply into the question should read carefully Karl Pearson's *Grammar of Science*, where he will find that if a body, A, is acting on (*i.e.*, accelerating) a *standard corpuscle, Q, and is being accelerated by it*, then:

$$\text{Mass of A} = \frac{\text{acceleration of Q due to A}}{\text{acceleration of A due to Q}}$$

Similarly, E. Mach (*The Science of Mechanics*), gives a definition of "Equal Masses": "All those bodies are bodies of equal Mass which, mutually acting on each other, produce in each other equal and opposite accelerations.

"We have, in this, simply designated or *named*, an actual relation of things. In the general case we proceed similarly. The bodies A and B receive respectively, as the result of their mutual action, the accelerations $-\phi$ and $+\phi^1$, where the senses of the accelerations are indicated by the signs. We say then, B has ϕ/ϕ^1 times the Mass of A. If we take A *as our unit*, we assign the Mass m to that body which imparts to A m times the acceleration that A in the reaction imparts to it. The ratio of the masses is the negative inverse ratio of the counter-accelerations. That these accelerations have always opposite signs, that there are, therefore, by our definition, only positive masses, is a point that experience teaches, and experience alone can teach.

In our conception of Mass no theory is involved; 'quantity of matter' is wholly unnecessary in it; all it contains is the exact establishment, designation, and denomination of *a fact.*

* * * * * * *

"When the negative inverse ratio of the mutually induced accelerations of the two bodies is called the Mass-ratio of these bodies, this is *a convention,* expressly acknowledged as arbitrary; but that these ratios are independent of the kind and of the order of combination of the bodies is *a result of enquiry.*

* * * * * * *

"To accomplish anything dynamically with the concept of Mass, the concept in question must, as I most emphatically insist, be a *dynamical concept.* Dynamics cannot be constructed with quantity of matter by itself, but the same can at most be artificially and arbitrarily attached to it. Quantity of matter by itself is never Mass, neither is it thermal capacity, or heat of combustion, nor nutritive value, or anything of the kind. Neither does 'Mass' play a thermal, but only a dynamical rôle.

* * * * * * *

"All uneasiness will vanish when once we have made clear to ourselves that in the concept of Mass no theory of any kind is contained, but simply *a fact of experience.*" (E. Mach, *The Science of Mechanics.*)

If it should later become accepted that Mass is a function of speed—as, indeed, some thinkers are inclined to believe—we may have to modify our conception of Mass. I say "if," for I do not believe that such a very revolutionary change in Mechanics will be necessary. There are certainly cases where the Mass *appears to increase*—where, in fact, the action is *as if* the Mass increased—but such increase is only apparent, and is the result of some acceleration.

The question of the "added Mass," as this is called, was first drawn attention to by the Chevalier DuBuat, at the end of the eighteenth century, but attracted no attention. It was next observed, forty years later, by Bessel, who generally gets the credit for its discovery. The effect is now well recognized by all those who carry out pendulum experiments.

The problem has been treated theoretically, and mathematically, by Poisson and Stokes; the latter showed mathematically that the apparent increase of Mass was 50 *per cent.*

DuBuat found, experimentally, that the increase was about 60 *per cent.* for the spherical bob of a pendulum.

Duchemin also, in 1842, showed analytically that the increase *for a sphere* was 60 per cent.; but that for other shapes it might very considerably exceed 100 *per cent.*

I have treated this subject at some length in my *Resistance of Air*, to which I would refer the reader.

M. Lucien Poincaré (*The New Physics*) says: "We have been led to suppose that inertia *depended on velocity*, and *even on direction*. If this conception were exact, the principle of Invariability of Mass would naturally be destroyed. Considered as a factor of attraction, *is Mass really indestructible?*"

M. Gustave Le Bon (*The Evolution of Forces*) goes a step further, for he says: "Not only does Mass vary with velocity, but it has latterly become a question whether it does not also *vary with the temperature.*"*

In this work I shall consider Mass as an unvarying Dynamical factor.

LENGTH AND DISTANCE

The foregoing are the three fundamental units for *Dynamics;* but, as previously stated, it is necessary to add a fourth fundamental unit for *Energetics;* and it is, further, necessary that this unit shall not be a "vector" quantity. Length, or distance, is really the scalar, or measure, of displacement—*i.e.*, the "how much" of the displacement. It is even sometimes called the "tensor" of the displacement; the direction being called the "versor." The length is the number of units of some *arbitrary measure* agreed on; which may be a foot, a centimetre, a mile, or any other unit which may have been previously agreed upon. The conception of "length" does not imply any *direction*, since

* I can easily believe that a hot body—whose particles are vibrating—would offer greater resistance to motion than a cold body; I should, in fact, *expect that it should.* That is not to say, however, that I think that its "Mass" (as I understand the word) has actually *increased.*

it is simply the distance between two points on a rigid standard.

The dimensions of Length are $[l]^1$.*

AREA

The measure of a " surface "—conceived as the size of a space of two dimensions—is called the " *Area.*"

The dimensions of Area are, therefore, $[l]^2$.*

CAPACITY OR BULK

In a similar manner the measure of a " Volume "—conceived as the size of a space of three dimensions—is called the *Capacity* or *Bulk.*

The dimensions of Capacity are $[l]^3$.*

Besides these fundamental units there are, what are called, *Derived units,* which will be referred to later.

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* The use of a small *l*, instead of a capital L, is purely a convenient " thought-saving " *convention;* the idea being that all vectorial dimensions shall be expressed in capitals, whilst for all scalar dimensions small letters shall be employed. By this means the student will see at a glance, whether an equation (or a quantity) is vectorial or scalar.

CHAPTER III

VECTOR AND SCALAR QUANTITIES

I HAVE previously employed the terms "vector" and "scalar"; and doubtless the reader will have a general idea of what was meant by these words. It is necessary, however (as I have previously insisted), that all expressions that we may make use of should be *rigidly defined.* This is the more necessary since, as I have pointed out, the subject of vectors is not taught in the schools, as I maintain it should be. I repeat that no one can understand Mechanics *properly* who has not clear ideas on this subject.

If we refer to the two text-books on *Mechanics*, which I have taken as typical examples, we find as follows: In Cox's *Mechanics* the word "vector" is found in two places only. It is in the Index—where, by the bye, the reference number is wrong—and at page 155, where we find the author says, quite casually: "Quantities which, like forces, depend for their effect on their direction as well as on their magnitude, are distinguished as *vector* quantities, while quantities which have only magnitude, such as a sum of money, or the amount of corn in a heap, are called *scalar* quantities."

These definitions are so imperfect that they cannot be accepted as at all satisfactory. The reason for this statement will appear shortly. The definition of a "vector" omits any reference to one of the *essential points* of a vector —without which it is certainly *not* a vector. The definition of a "scalar" is still vaguer. A definition should have a concise meaning and clearly defined limits; whilst here, a "scalar" is, apparently, any quantity which is *not a vector.* Nor are the examples happily chosen.

My sympathies are with the student!

In Glazebrook's *Dynamics* the words "vector" and "scalar" are neither in the Index, nor in the Text.

Professor Perry (*The Teaching of Elementary Mechanics*),

remarked that: "It may be said that at the age of fourteen he [a boy] has the experience and knowledge which will enable him to comprehend a vector subject, and he may now begin graphical and experimental work in *the addition and subtraction of vectors*, such as displacements, velocities and forces." This was, apparently, Clerk Maxwell's view, as will be seen by a reference to that little master-piece *Matter and Motion*, which is an *elementary text-book*.

As previously pointed out, there are certain quantities, such as displacement, velocity, rate of acceleration, etc., which are what are called "directed quantities," "current quantities," or "vectorial quantities."

"As is well-known, we distinguish, since Hamilton's time, two kinds of physical magnitudes—*scalars* and *vectors*. The two kinds of magnitude are *essentially different* in their nature, and one can *never be represented by the other*." (Professor Ostwald.)

Sir William Rowan Hamilton, who was one of the first who employed the word, defined a vector as follows: "A right line AB considered as having not only *length* but also *direction*, is said to be a vector." (*Elements of Quaternions.*)

Messrs. P. Richardson and Edward H. Landis (*Fundamental Conceptions of Modern Mathematics*), in reference to this, say:

"Whether he regarded this statement as furnishing an adequate definition of 'vector' is not clear. His successors usually content themselves with a bare repetition of it. As a definition, however, it is obviously inadequate, since a *linear velocity at a point* is a vector, and, of course, a velocity is not a line. Further, not *all* straight lines possess the attributes requisite to vectors. With regard to their mode of generation, straight lines may be classed into those which have been *described* ["drawn," or "traced"], and those which have *not been described*; and only a straight line of the first class possesses an attribute of direction *of that type* which is of utility in quaternions. With a straight line which has been *described*, each point further from the initial point has come into existence *later than every point nearer the initial point*. We can distinguish between *two types of direction*, which may be called respectively *Geometrical*

direction and *Trigonometrical direction.* These cannot be more readily pointed out than by stating when two straight lines have like and when unlike *directions.* Two straight lines have like *Geometrical directions* when they are either *parallel* or *coaligned,* and have unlike Geometrical directions when they are *neither parallel nor coaligned.* Two straight lines have like *Trigonometrical directions* when they satisfy a set of *three requirements*: first, they have been *described;* second, they are *parallel or coaligned;* third, they are not of *contrary description.* . . . We may speak of Trigonometrical direction as ' currency ' ; and say that two described straight lines, or in general two vectors, are *concurrent* if they are alike as to *Trigonometrical direction.* The names ' currency ' and ' concurrent ' are due to Clifford. Some authors employ the term ' sense ' instead of currency, but this practice has no merits which justify it. ' Current ' and ' currency ' naturally suggest a running or flowing, but no such associations are called forth by the word ' sense,' which is used chiefly to refer to the meaning of a word, the mentality of a person, or the process of sensation. Two vectors, parallel or coaligned, but *not concurrent,* are said to be *contrary.* . . . For two vectors *of the same sort* to be *of the same kind,* it is necessary and sufficient that they should be *parallel or coaligned.* In order that they should be *of the same variety* it is necessary and sufficient for them to be *concurrent.* For two vectors of the same sort to be *equal,* it is necessary and sufficient that they be *concurrent* and of *equal magnitude.* . . .

" To recognize that a currency and a magnitude together constitute every non-zero vector does not appear to be sufficient to avoid confusion. Even so high an authority as Hamilton states that a right line is a vector; a view which is quite untenable. *The vector of a straight line,* which is composed of two *attributes of the line,* is as different from the line itself as the colour of a red body is different from that body. Suppose that a straight line has been described from a point A to a point B, the length of this line being 150 cm. This line possesses the *attributes* of a vector, but it is *not itself the vector.* For let the line be turned about the point A to a new position without undergoing any change

of length. It would still be regarded, and justly so, as the *same line* moved into a new position. But the *vector of the line* has changed; the line in the new position has *a new vector*, which is not even *equal* to the vector of the line in its old position. The *magnitude of the vector* of a line is, as we have already said, nothing more nor less than *the length of the line*, and is expressed in centimetres, metres, etc. According to the requirements laid down by the founders of vector analysis, for two non-zero vectors to be *equal*, they must not only be *equal in magnitude*, but must also be *concurrent*. The vectors of the line AB in its two positions do not meet these requirements. The two vectors are *non-concurrent*. Moreover, if we construe 'equality in magnitude,' in the proper sense of equality, *which excludes identity*—a distinction which was perhaps never plainly before the founders of vector analysis—then the two vectors are not *equal in magnitude;* the magnitude of the one vector is *the same* as the magnitude of the other vector. That the vectors of the line AB in its two positions have *the same* magnitude—not *equal* magnitudes—is an excellent example of identity, something so frequently improperly conceived, as to be in its misconception one of the most striking characteristics of the erroneous philosophical theories of mathematics. . . . The confusion between *equality* (and other cases of perfect similarity) and *identity* is one of the most pernicious errors into which a mathematician can fall. . . No entity is *equal to itself*. Every entity is *identical with itself*, and may be *perfectly similar* to other entities in various respects."

From the foregoing we see that *contrary vectors* may be considered to be of *the same kind*, although they are not *of the same variety*.

We also see that we must discriminate, most carefully, between the vector of a straight line and the line itself. The *vector of the line* is an *attribute*, or *property*, of the line, and not the line itself; any more than the "colour property" of a stick is the stick itself. If we paint the stick another colour, it is still the same stick, though its "colour property" has been changed.

DIRECTION

Direction, as was pointed out, is an equivocal word, since it can be used in either a Geometrical or a Trigonometrical sense. Some writers employ it in one sense, and some in another. Mathematicians ordinarily use it in its Geometrical sense, and hence, they associate with it the " sense " of the direction. Karl Pearson (*Grammar of Science*), besides employing the word " sense " (generally used by Mathematicians), varies it with another which is very expressive and conveys a very clear meaning. He speaks of " bearing " ; for example, he says the " combination of speed and bearing we term velocity." In some ways, this word is more expressive than even Clifford's " currency."

It would appear advisable, therefore, to discontinue the use of both " direction " and " sense," when referring to vectors ; and to, preferably, employ either of the words " currency " or " bearing," as being less likely to be misunderstood. These words comprise both " direction " *and* " sense."

Mr. E. F. Etchells has suggested to me the use of the words " clinure " and " trend," in the place of " direction " and " sense."

Professor Perry, certainly, uses the word " clinure " in his very excellent *Applied Mechanics* (1903) ; but I do not care for it, since it is not in the *Century Dictionary*, and it is not a familiar expression. It would, indeed, be preferable to employ the word " clinus," which the Dictionary defines as " bend, or slope." The word " trend " is a good old English word, signifying " a general course or direction ; inclination of the course of something towards a particular line or point." It would, therefore, appear to comprise both " direction " and " sense." We might, therefore, say that a vector was composed of a *magnitude* and a *trend ;* just as we now say that it is composed of a *magnitude* and a *currency*, or of a *magnitude* and a *bearing*.

We may put it briefly that *wherever a currency and a magnitude subsist together, there a vector exists.*

Professor O. Henrici and G. C. Turner in their *Vectors and Rotors*, define a vector thus : " A quantity which is

related to a definite direction in space is called a vector quantity, or simply a vector.

"The displacement of a point, velocity, momentum, acceleration, rotation about an axis, an electric current, are examples of vectors; none of these can be completely specified without some reference to direction."

This definition appears to be somewhat wanting in clearness and completeness, since no reference is made to "currency." To be accurate, also, a displacement of a *point* is not, strictly speaking, a vector. Translatory motion is not a species of motion of a *mathematical point.* It is a species of motion of *a body;* the point, therefore, must be a *material point.* Position can only be ubicated by Mass. Also, an "electric current" would not, ordinarily, be classed as a vector quantity. Electricity is a form of energy, and so would come under scalar quantities.

There is a very excellent description and explanation of vectors given by Maxwell in his *Matter and Motion.* In the Preface the author says: "To become acquainted with these fundamental ideas, to examine them under all their aspects, and habitually to *guide the current of thought* along the channels of *strict dynamical reasoning,* must be *the foundation* of the training of the student of Physical science.

"The following statement of the fundamental doctrines of Matter and Motion is, therefore, to be regarded as an introduction to the study of Physical science in general."

This little book is, in my opinion, the best book on Mechanics in the English language, and it reflects all Maxwell's genius. I am glad to see that its importance is now being recognized.

After a few articles explaining "a material system," and "configuration" Clerk Maxwell plunges the reader into "vectors," and the subject is pursued up to the very end of the book. Very curiously, also, whilst authors of text-books devote very many pages to "the parallelogram"—*ad nauseam*—Clerk Maxwell (I repeat) *never mentions the subject at all.* All work on the "composition of vectors," is done by the very simple method of the *addition* and *subtraction* of vectors.

In Article VIII, *Vectors*, we read:

"The expression $\overline{AB}$ in Geometry is merely the name of a line. Here it indicates the *operation by which the line is drawn*, that of carrying a *tracing point* in a certain direction for a certain distance. As indicating *an operation*, $\overline{AB}$ is called a vector, and the operation is completely defined by the *direction* and *distance* of the transference. The starting point, which is called the origin, *may be anywhere*.

"To define a finite straight line we must state its origin as well as its direction and length. All vectors, however, are regarded as equal which are parallel [or co-aligned] (and drawn towards the same parts) and of the same [? like, or equal]* magnitude.

"Any quantity, such, for instance, as a velocity, or a force, which has a finite direction and a finite magnitude may be *treated as a vector*, and may be indicated in a diagram by a straight line whose direction is *parallel to the vector* and whose length represents, *according to a determined scale*, the magnitude of the vector."

The reader will now have, I trust, a clear idea of what is meant by a "vector."

The *magnitude* of the vector is always a "scalar" quantity; but it is not correct to say that *every quantity* which is not a vector must *necessarily* be a scalar; and for the following reasons.

ATTRIBUTES

"Of all quantities the most primitive are the *natural numbers*. That a natural number is an attribute of a group of objects has long been more or less completely recognized. Indeed, for ascribing to a group of objects an *attribute of number*, we have a foundation perfectly similar to that for ascribing to a body an attribute of shape, or an attribute of colour—a *quality*. Every two bodies resemble or differ

* "The use of the word 'same' to signify, not *identity*, but *complete similarity* has crept into Mathematics, as well as into other branches of enquiry where precision and accuracy of thought are indispensable, and has been a fruitful source of confusion and error. It is a colloquial phraseology, rather than one suited to the requirements of an exact science." (*Fundamental Conceptions of Modern Mathematics.*)

Even colloquially, we would not speak of two houses which were *exactly similar* (having been built from the same plan), as "the same house."

from each other in manifold ways. To treat of these various modes of resemblance and difference we ascribe to them a *distinct attribute* for each mode of resemblance or difference. We have no other foundation for speaking of shape, of colour, and of all the other attributes of bodies. It is upon this basis that the theory of these attributes rests. It is likewise with groups of objects. They, too, resemble and differ from each other in manifold ways. And *one mode* of resemblance and difference is in that respect which enables us to say that a group of objects possesses an *attribute of* number." (*Fundamental Conceptions of Modern Mathematics.*)

Thus we may say that groups of three dogs, three cats, and three chairs, resemble one another in having an attribute of " three-ness." By this attribute they are distinguished from certain other groups which have an attribute of " four-ness."

APPLICATE QUANTITIES

" We must point out that certain attributes, which are *not abstract quantities,* constitute the *applicate quantities,* which may be very simple, as the attribute length, or very complex as the quantities dealt with in the theory of Electricity. For such an attribute to be an *applicate quantity,* it is necessary that it should be *amenable to measurement.*" (*Fundamental Conceptions of Modern Mathematics.*)

From the foregoing it will be clear that " applicate quantity "—as here defined—and " scalar quantity," are practically synonymous expressions; it being an essential property of a " scalar " that it should be *amenable to measurement.*

We see now why Mr. Cox's examples of " scalar quantities " were considered unsatisfactory. Neither a " sum of money," nor a " heap of corn " is *amenable to measurement;* they have no " dimensions " in Length, Time and Mass; they are no more *Dynamical quantities* than a man, or the sun. They would be classed as *concrete quantities.*

Knowing that a quantity is, really, a *Dynamical quantity;* a rough and ready rule for distinguishing between them is: If the quantities can be added arithmetically, the quantities are " scalar." If, however, they can only be added *geometrically,* the quantities are " vector."

We can *under no conditions* equate scalars and vectors; since, as was previously pointed out, the quantities cannot be transformed one into the other. Consequently, equations such as:

$$\text{momentum} \times \text{velocity} = \text{energy}$$

or $$\text{force} \times \text{distance} = \text{energy, or work,}$$

are meaningless. The equations are not *homogeneous*, and yet we find in Sir R. Glazebrook's *Dynamics:*

"Thus, let F be the force, let the point of application of the force be displaced a distance s in the direction of the force; the work done U is given by the equation:

$$U = Fs."$$

Besides this equation not being "homogeneous," I might point out that Sir Richard defines force thus: "*Definition. The rate at which the momentum of a moving body changes is called Impressed Force.*"

How does one *apply* a "rate of change of momentum" at a point? I pity the unfortunate student who might be called upon to explain this query, during an examination!

It only now remains to explain how vectors are added and subtracted geometrically.

ADDITION OF VECTORS

In ordinary text-books it is usual to commence with the "Parallelogram of Displacements." This is next varied by referring to the "Triangle of Displacements." This is next varied by the "Resolution of Displacements"; and the "Composition of Velocities"; including the "Polygon of Forces," or "Velocities"; winding up with the "Parallelogram of Velocities," and the "Composition and Resolution of Velocities."

This mode of procedure appears to me to be both clumsy and tedious; it is also very confusing for a young student. Let us see how much more simply Maxwell instructs the novice.

Commencing in his Article VII, he says:

"If you start from A and travel in the direction indicated by the line $\overline{AB}$ (Fig. 1), and for a distance equal to the

length of that line, you will get to B. This direction and distance may be indicated equally well by any other line, such as $\overline{ab}$, which is parallel and equal to $\overline{AB}$. The position of A with respect to B is indicated by the direction and length of the line $\overline{BA}$, drawn *from* B to A, or the line $\overline{ba}$, equal and parallel to $\overline{BA}$.

"It is evident that $\overline{BA} = -\overline{AB}$.

"In naming a line by the letters at its extremities the order of the letters is always that in which the line is to be drawn."

Turning next to Article X, *Addition of Vectors*, we read:

"The rule for the addition of vectors may be stated thus: From any point as origin draw the successive vectors in series, so that each vector begins at the end of the preceding one. The straight line from their origin to the extremity of the series represents the vector which is the sum of the vectors.

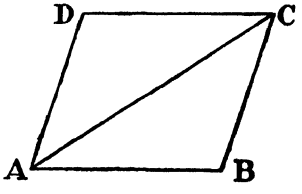

FIG. 1.

"*The order of addition is indifferent*, for if we write $\overline{BC} + \overline{AB}$ the operation indicated may be performed by drawing $\overline{AD}$ parallel and equal to $\overline{BC}$, and then joining $\overline{DC}$, which, by Euclid I, 33, is parallel and equal to $\overline{AB}$, so that by these two operations we arrive at the point C in whichever order we perform them.

"The same is true for any number of vectors, take them in what order we please."

Article XI, *Subtraction of one Vector from another.*

"To express the position of C with respect to B in terms of the positions of B and C with respect to A, we observe that we can get from B to C either by passing along the straight line BC or by passing from B to A and then from A to C. Hence:

$$\overline{BC} = \overline{BA} + \overline{AC},$$
$$= \overline{AC} + \overline{BA}, \text{ since the order of addition is indifferent.}$$
$$= \overline{AC} - \overline{AB}, \text{ since } \overline{AB} \text{ is equal and opposite to } \overline{BA}.$$

Or the vector $\overline{BC}$, which expresses the position of C

with respect to B, is found by subtracting the vector of B from the vector of C, these vectors being drawn to B and C respectively from any common origin A."

We see how very much simpler this method of Maxwell's is than the ordinary system adopted in the usual text-books.

There is one small caution, however, to which Maxwell does not refer, but which is important. In his equation :

$$\overline{AB} + \overline{BC} = \overline{AC}$$

or to put it in a more general form :

$$\alpha + \beta = \gamma$$

the sign " = " does not really mean " equal to." " The = must now be held to designate, as it does perpetually in Algebra, '*equivalent to.*'" (Kelland and Tait, *Introduction to Quaternions*, 1873.) For example, in Fig. 1 the vector AC is equivalent, but not equal, to AD + DC, in the arithmetical sense.

REFERENCES

Sir William Rowan Hamilton, *Elements of Quaternions*, 1866.

Kelland and Tait, *Introduction to Quaternions*, 1873.

Professor O. Henrici and G. C. Turner, *Vectors and Rotors*, 1903.

John Perry, *Applied Mechanics*, 1903.

John Cox, *Mechanics*, 1909.

CHAPTER IV

DERIVED UNITS

BESIDES the so-called "fundamental," there are also "derived" or compound dimensions.

VELOCITY

There is, unfortunately, some unnecessary confusion in regard to the definition of velocity. One of my selected text-books says: "Velocity is the rate of change of position." This, though strictly correct, is not as clear as it might be, and the Author adds: "A particle moving with *uniform* velocity describes equal spaces in equal times." But if a particle moves in a circle, it might be said to be "describing equal spaces in equal times," whilst its *velocity* would certainly *not be uniform*—although its *speed* might be uniform. It depends upon the meaning of the word "space," which is not here defined. *Uniform velocity must be in a straight line,* since if the *direction* of the motion changes, the *velocity* also changes. Velocity is a vector quantity, and consequently, if the direction is changed, although the "magnitude" of the vector—its *speed*—remains unaltered, the *velocity* is altered. All this will, I hope, be perfectly clear from the last Chapter.

It is certainly true that this is the impression which the Author has endeavoured to convey, since he says: "To measure velocity we need to know the change of position, or displacement, per second; this change is determined if we know (1) the *distance* the particle moves through, (2) the direction of motion" . . . and later on: "In strictness, therefore, the velocity of a particle can be uniform *only when the particle is moving in a straight line* and passes over equal distances in equal times. The term *uniform velocity* is, however, often applied when *uniform speed* would be *more accurate.*"

Why the words, " In strictness," in this last quotation ? Is it intended as a kind of palliation of what can only be described as a slovenly practice ? This practice of confusing " velocity " and " speed "—which is certainly to be found in *most* text-books—is highly pernicious and reprehensible. It induces muddy thinking in the student, and frequently leads to equations which are not " homogeneous " ; vectors being equated to scalars. What would be thought of an author who described " red " and " green," and even pointed out the *difference* between them, saying later " the term ' red ' is, however, *often applied* when ' green ' would be *more accurate?*" Such " terminological inexactitude " is better suited to Parliament than to an accurate science like Mechanics.

The same confusion also arises from the use of the words " space " and " distance " as if they were synonymous and interchangeable.

These are good examples of the confusion caused by the want of discrimination between vectors and scalars. It shows the great want there is for some clear ideas about the " vectorial property." The reader will, I trust, carefully avoid such confusion, and not be satisfied with the " *less accurate* " practice of calling a quantity by one name, when he *really means* something else !

The " dimensions " of velocity are $[S]^1\ [T]^{-1}$; *i.e.*, one dimension in space, and *minus* one in time. Velocity is, of course, a vector quantity.

As previously pointed out, and to prevent any possible misunderstanding, I would like to repeat again that I do not mean to say that a velocity is a space, or " displacement," *divided* by a time—as is frequently stated in text-books—such a statement is senseless ; what is implied is that the velocity is the *ratio* (an *abstract quantity*) between the space, or displacement, and the time-interval ; that the velocity varies *as the space,* or displacement, and *inversely as the time.* We can no more *divide* a " space " by a " time," than we could divide cows by acres. One may divide the *numerical coefficient* of the space, by the *numerical coefficient* of the time, and the *quotient* will be the *numerical coefficient* of the velocity ; but one cannot *divide* any determinate quantity by any other one.

SPEED

In Energetics we speak of "speed." It is the ratio between a *length* travelled over by a particle, say, and the interval of time absorbed in the travel. It is not (as is frequently stated in textbooks) the *quotient* of the length divided by the time. What would be the meaning of dividing ten yards by two seconds?

Speed, in Dynamics, is the *magnitude* (scalar, or measure) of the velocity. Since the equations in Dynamics are all vectorial, the use of the word "speed" in this science is extremely limited.

The dimensions of speed are $[\,l\,]^1\,[\,t\,]^{-1}$; or one dimension in *length*, and *minus* one in time. It is, obviously, a scalar quantity.

ACCELERATION

Acceleration in its proper sense denotes *any change of velocity*, whether an increase, a diminution, or a change of direction. Clerk Maxwell, *Matter and Motion*, referring to "acceleration," says:

"The word acceleration is here used to denote *any change in the velocity*, whether that change be an increase, a diminution, or a change in direction. Hence, instead of distinguishing, as in ordinary language, between the acceleration, the retardation and the deflexion of the motion of a body, we say that the acceleration may be in the *direction* of motion, in the *contrary* direction, or *transverse* to that direction.

"As the displacement of a system is defined to be the change of the configuration of the system, so the *total acceleration* of the system is defined to be *the change of the velocities* of the system."

To give an example: if a train is moving, in a straight line, at a speed of 20 miles per hour, and later, the speed is 40 miles per hour, we say that the *velocity* has been accelerated; the acceleration being 20 miles per hour. In speaking thus, no mention has been made of the duration of the time, nor of the distance. The acceleration may have occurred in 30 minutes, or it may have occurred in 20 miles. All we know is that the *total acceleration*, which we have

observed, is 20 miles per hour; but whether produced in a short, or long time, or in a short, or a long distance, is not known. We might even know that it had occurred in 30 minutes, or in 20 miles, but that would not alter the *total amount* of the acceleration.

RATE OF ACCELERATION

Rate of acceleration may be defined as the *rate of change of velocity*. Maxwell, *Matter and Motion*, Article XXIII, refers to this as follows:

"We have hitherto been considering the *total* acceleration which takes place during a certain interval of time. If the rate of acceleration is constant, it is measured by the total acceleration in a unit of time. If the rate of acceleration is variable, its value at a given instant is measured by the total acceleration in unit of time of a point whose acceleration is constant and equal to that of the particle at the given instant. . . .

"As *rates of acceleration* have to be considered in physical science much more frequently than *total accelerations*, the word 'acceleration' has come to be employed in the same sense in which we have hitherto used the phrase *rate of acceleration*.

"In future, therefore, when we see the word acceleration *without qualification*, we mean what we have described as the *rate of acceleration*."

Notwithstanding this caution, in Article XXIV, under the heading, *Diagrams of Accelerations*, Maxwell says: "The diagram of accelerations is a system of points, each of which corresponds to one of the bodies of the material system, and is such that the line $\overline{\alpha\beta}$ in the diagram represents the *rate of acceleration* of the body B with respect to the body A."

The word "rate" is here inserted, when it might reasonably have been omitted after the caution in Article XXIII. This indicates special caution on Clerk Maxwell's part.

Karl Pearson, *Grammar of Science*, deals with this question from a slightly different, but very useful, point of view:

"Acceleration has *spurt* and *shunt* components.

"The spurt acceleration takes place *in the direction of motion*, and is measured by the rate at which the speed is being increased (or it may be decreased).

"The shunt acceleration takes place perpendicular to the direction of motion and is measured by the product of the curvature and the square of the speed.

"These two kinds of acceleration are usually spoken of as *speed acceleration* and *normal acceleration*.

"From these results we conclude that:

"1. If a point be not accelerated it will describe, with regard to the given frame of reference for which the acceleration is measured, a straight line with uniform speed. For there will be no spurt, and therefore, the speed must be uniform; and there will be no shunt, and therefore, the path must have a zero curvature; but the only path without bending is a straight line. Neither *uniform speed* nor *zero curvature alone* denotes *an absence of acceleration.*"

"Acceleration" (as ordinarily employed, but really, *rate of acceleration*) being defined as the "rate of change of velocity"; and velocity being the "rate of displacement," or "rate of change of position"; it follows that "rate of acceleration" is a *Rate of a rate;* what mathematicians would call a "second differential," or a differential of the *second order*. Its dimensions are, therefore, $[V]^1\ [T]^{-1}$ or $[S]^1\ [T]^{-2}$; *i.e.*, one dimension in space and *minus* two in time. In order to conduce to clear thinking, I would prefer to state this as one dimension in space, and *minus* one in Time, *twice*, or $[S]^1\ [T]^{-1}\ [T]^{-1}$; so that the student shall not be led to think of a *square of a Time*.

The practice of employing the word "acceleration" when "rate of acceleration" is meant, is most reprehensible, and should consequently be avoided. The same word should never be used in *two senses* in any argument. Since, however, it is comparatively rarely used in its proper *Dynamical sense* it is specially necessary for the student who wishes to think clearly, to note the remarks of Maxwell, which I have quoted. All authors are not as careful as Maxwell was in the use of language.

RATE OF SPURT

To quote Karl Pearson again (*Grammar of Science*): "If in a certain interval of time the speed of a point P changes from a number of miles per minute represented by the number of linear units IV_4 to the number of miles per minute represented by the linear units in IV_8, the speed has in this case *quickened*, or there has been what we may call a *spurt* in the speed . . . acceleration has thus two fundamental factors, the *spurt* and the *shunt*."

Now, if "spurt" is accepted as *change of speed*, in the same manner that "acceleration" is *change of velocity*, it will not, I trust, be unreasonable to employ "rate of spurt," for rate of change of speed. This suggested "rate of spurt" is the *magnitude*, or scalar, of "rate of acceleration," and its dimensions would, therefore, be $[l]^1\ [t]^{-2}$, or, perhaps better, $[l]^1\ [t]^{-1}\ [t]^{-1}$; one dimension in *length*, and *minus* one in *Time* TWICE.

POINT VECTOR

There is still another useful way of viewing this question and that is the one adopted by Clerk Maxwell in his *Matter and Motion*. If we employ his terminology and define "displacement" as a "point vector," then velocity would be defined as a "rate of change of point vector."

In the same manner we might define "speed" as the "rate of change of point scalar." Though Maxwell employs the term "point vector," he does not speak of point scalar—the *magnitude* of point vector—which is, therefore, an innovation.

MASS VECTOR

Clerk Maxwell also says: "We have seen that a vector represents an operation of carrying a tracing point [*i.e.*, a *material* point] from a given origin to a given point.

"Let us define a mass vector as the operation of carrying a given mass from the origin to the given point. The direction of the mass vector is the same as that of the vector of the mass, but its magnitude is the product of the mass into the vector of the mass.

"Thus if $\overline{OA}$ is the vector of the mass A, the mass vector is $\overline{OA}.A$."

MOMENTUM

In the same manner "momentum" (what Newton calls "quantity of motion") which is defined as "product of the number of units of mass into the number of units of the velocity with which it moves, may be represented as the rate of change of a mass vector." (Clerk Maxwell.)

Momentum plays a very dominant, if not the leading rôle in Pure Dynamics, which is largely based on the Principle of the "Conservation of Momentum"; that is to say, in other words, that in any Dynamical "system" the amount of the momentum always remains constant. This conservation is a logical deduction from the Second Law of Motion, which was first enunciated by Descartes, though it was undoubtedly known to Galileo, and almost certainly to Lionardo da Vinci. It was also known as the "Conservation of Motion."

When we turn from *rigid bodies*, from those bodies with the properties—or want of properties—assumed in Pure Dynamics, to *real bodies*, this principle is no longer true: momentum is *never constant*. In consequence of viscosity, deformation, friction, etc., motion is perpetually being destroyed; energy is being transformed into heat, etc., and so dissipated.

Newton (*Principia*, Book I, Cor III, "Laws") says: "The *quantity of motion* . . . suffers *no change* from the action of bodies among themselves." He is here speaking as a "mathematician" and referring to Pure Dynamics, where this follows logically from the assumptions.

In his *Opticks*, however, (Query 31, p. 373) where he is speaking as a "physicist," we read: "From the various composition of two motions, 'tis very certain that there is *not* always the same *quantity of motion* in the world. . . Motion is much more apt to be lost than got, and is *always upon the decay*."

Sir J. J. Thomson, *Matter and Ether* (Adamson Lecture, September, 1907), says:

"Take the case of two electrified bodies A and B in rapid motion, we can, from the laws of Electricity, calculate the forces which they exert on each other, and we find that, except when they are moving with the same speed and in

the same direction, the force which A exerts on B is not equal and opposite to that which B exerts on A, so that the *momentum of the system formed by* B *and* A *does not remain constant.*"

"Leibnitz recognized Descartes' error in thinking that, *in Nature*, the sum of the products of the masses and their respective velocities is constant, and substituted in it *the squares of the velocities*, so that the sum is what is called the *vis viva* of the system considered. But in impact, the *vis viva* is only conserved if the bodies are elastic." (Phil. E. B. Jourdain, *The Principle of Least Action*, 1913.)

Louis Trenchard More, *The Limitations of Science*, 1915, in referring to Descartes' work, says: "While his particular Law of the Conservation of Momentum was erroneous, yet it was undoubtedly the progenitor of the law finally enunciated by Von Helmholtz, and now generally accepted, that the *total quantity of energy* remains constant."

The dimensions of momentum are $[MST^{-1}]$, or $[M]^1\ [S]^1\ [T]^{-1}$; one dimension in mass, one in space and *minus* one in time. It is, obviously, a vector quantity.

PRESSURE

"Pressure" is another of those words which are, unfortunately, used in two separate and distinct senses. Sometimes it is taken to mean, what Newton called "action" and sometimes the measure of *the intensity of this action.* For example, we may suppose that a strut, or a column, has to withstand a pressure of 100 tons. If the material of the strut is such that it is unsafe to subject it to a stress greater than 1 ton *per square inch*, the engineer designs the strut suitably so that *this intensity of pressure* shall not be exceeded.

In the first part of this statement the "pressure"—sometimes called the "total pressure"—is 100 tons; in the second, the *intensity of the pressure* has been suitably reduced, so as not to let it exceed 1 ton *per square inch*; this "intensity" is *also called* "the pressure."

As in the case of "acceleration" (previously referred to) the word "pressure" is now generally used to signify (what should be called) *the intensity of the pressure*—pressure *per unit area.*

C. Hering, *Conversion Tables*, 1914, gives the " derivation " of pressure as $\frac{\text{force}}{\text{surface}}$. Its dimensions would, therefore, be $[M]^1\,[S]^{-1}\,[T]^{-2}$.

Pressure might thus be defined, in words, as " force per unit area." Since " force " is a " mass acceleration," pressure might also be defined as " mass acceleration per square unit "—per *square inch*, say. It is not easy to understand the meaning of " acceleration *per square inch!* "

Newton, most undoubtedly, used the word " pressure " to mean what is called the " total pressure "—or the " action; " a reference to the Third Law of Motion shows that he used " action " as synonymous with this " total pressure." *The intensity* of the pressure is not necessarily equal to *the intensity* of the " counter pressure." We can imagine the rope, by means of which " the horse pulls the stone," to be of different cross-section at different parts.

If, therefore, we are treating of " Newtonian Mechanics," it would appear to be reasonable to follow our great master in this. I, therefore, give the dimensions of pressure as $[m]^1\,[l]^1\,[t]^{-2}$. It is, of course, a scalar quantity.

STRESS

Maxwell (*Matter and Motion* Article LV) says that we use " the word stress to denote the mutual action between two portions of matter. This word was borrowed from common language, and invested with a precise scientific meaning, by the late Professor Rankine, to whom we are indebted for several other valuable scientific terms.

" As soon as we have formed for ourselves the idea of a stress, such as the tension of a rope or the pressure between two bodies, and have recognized its *double aspect* as it affects the two portions of matter between which *it acts*, the Third Law of Motion is seen to be equivalent to the statement that all force [? action, effort] is of the nature of stress, that stress exists only between two portions of matter, and that its effects on these portions of matter (measured by the momentum generated in a given time) are equal and opposite."

From the foregoing we see that Newton's " action,"

Rankine's "effort," as well as "pressure" and "tension" are all stresses. They are all, what one may call "two-ended"; you cannot *press* or *pull* any body if it does not resist you; you can, further, only press it *as much as it resists*. Action and reaction are *inseparable*, as well as *equal*.* Reaction is, in fact, a "provoked action"; you press a body, and so deform it; the body "resents" this (if I may so say) and endeavours to return to its original state. We call this "endeavour" the "reaction." If there is no action, there is no reaction.

The dimensions of stress are $[m]^1\ [l]^1\ [t]^{-2}$.

WORK AND ENERGY

"Work is the *act* of producing a change of configuration in a system in opposition to a force [? action, or effort] which *resists that change*.

"Energy is the capacity of doing work.

* * * * * * *

"Thus, if one pound is lifted one foot from the ground by a man in opposition to the force of gravity, a certain amount of work is done by the man, and this quantity is known among engineers as *one foot-pound*.

"Here the man is the external agent, the material system consists of the earth and the pound, the change of configuration is the increase of distance between the matter of the earth and the matter of the pound, and the force is the upward force exerted by the man in lifting the pound, which is equal and opposite to the weight of the pound.

"To raise twenty pounds of water ten feet high requires 200 foot-pounds of work.

"The quantity of work done is, therefore, proportional to the product of the numbers representing the force [? effort] exerted and the displacement in the direction of the force." (*Matter and Motion.*)

The above definition of Energy though a good "working definition" is not a very complete one. In fact it has not been found possible to give a definition of Energy which will cover all its different forms. Henri Poincaré, *La Science et l'Hypothèse*, 1912, says: "Dans chaque cas particulier

*As the old Proverb puts it:—"The anvil hits as hard as the hammer."

on voit bien ce que c'est que l'énergie et on en peut donner une définition, *au moins provisoire;* mais il est impossible d'en donner une *définition générale.*

" Si l'on veut énoncer le principe [Conservation d'Énergie] dans toute sa généralité et en l'applicant à l'univers, on le voit, pour ainsi dire, s'évanouir, et il ne reste plus que ceci ; *il y a quelque chose qui demeure constant.*"

" The progress of Physical Science has led to the discovery and investigation of different forms of energy, and to the establishment of the doctrine that *all* material systems may be regarded as *conservative systems,* provided all the different forms of energy which exist in these systems are taken into account.

" This doctrine, considered as a deduction from *observation* and *experiment,* can, of course, assert no more than that *no instance of a non-conservative system has hitherto been discovered.*" (*Matter and Motion.*)

That is to say that we do not *know*—we cannot *prove,* in fact—that the Principle is true ; we *believe it* only because we have *never found any exception.* It is only *probably* true ; though the probability is so very great that we are prone to treat it as if it were a *certainty.*

When work is done, energy is said to be " expended." This expression only means that a certain amount of energy has been *transformed.* By this we mean that the body *doing work* on the other, has its energy transformed and reduced by the amount of *work done* on the other—which consequently has *its* energy transformed and *increased*; the total amount of energy before the action being exactly equal to the amount of energy after. A certain amount of energy has been *transformed*—nothing more.

Energy exists in two conditions :—

1. *Kinetic* energy, or energy of motion ; in which is included the energy of heat, etc.

2. *Potential* energy, which has a great number of forms ; for example, energy of position, energy of strain, chemical energy, etc., etc.

In a rough kind of popular sense, one may compare " potential energy " to " *stored* kinetic energy." In the case of a man lifting a weight—as when he winds up a clock

—he may be said to have "*stored energy* in the clock." When we dam a stream, and so "store the water," we are, really, also "storing the energy" of the stream—which energy we can employ later for any suitable purpose. The kinetic energy has, apparently, *disappeared;* but we may say that it has only been "bottled." If we pay a sum of money into a bank, our personal "cash" will have been reduced by that amount; but it has only been *transformed* into "credit"; which we may call "potential cash." In fact, in business, one may paraphrase Clerk Maxwell's definition of Energy and say, "Credit is the capacity of paying one's debts."

Since energy "expended" is measured by the amount of work done; and work is measured by action × distance—*pressure*, say, through a certain *length*—the dimensions of energy are commonly weight × height, or

$$Mg \times l = Mlt^{-2} \times l = [M]^1\,[l]^2\,[t]^{-2}.$$

In treating of liquids (water, for example), the energy is most conveniently measured in terms of the *intensity of the pressure* and the *volume of the water*: consequently, the energy can be put in the form of

$$pV = ML^{-1}t^{-2} \times l^3 = [M]^1\,[l]^2\,[t]^{-2}.$$

These methods are very suitable for measuring "potential energy," but it is more convenient, when measuring "*kinetic* energy," to express the measure *in terms of the speed.* The relationship can be expressed by Rankine's fundamental equation in *Energetics* as

$$X\int dx = \text{energy} = M\int v.dv$$

or

$$X\int dx = M\frac{v^2}{2} + C$$

where C is the constant of integration.

Energy is a *scalar* quantity. Such equations as:

"Kinetic energy = . . . = ½ product of the momentum and the velocity"

are, therefore, non-*homogeneous* and meaningless.

It is *not true* that energy = ½ *vis viva.*

In Hering's *Conversion Tables*, 1914, however, I notice that the Author treats *vis viva*, energy, and work, as practically synonymous expressions. The dimensions of *vis viva* are given as "$W = \frac{1}{2}Mv^2 = L^2MT^{-2}$ = erg." Non-homogeneous!

POWER

Power may be defined as the "*Time* rate" at which work is done: Action being the "*Space* rate" at which this is done.

Power is sometimes explained in text-books thus: "Power is measured by *the product of the force into the velocity of its point of application measured in the direction of the force.* In other words, the rate at which work is being done on a particle is the product of its rate of gain of momentum and the component of its velocity measured in the direction in which it is gaining momentum." Non-homogeneous reasoning! Poor student!!

Power × time, being = work = energy, the dimensions of power are:

$$\mathrm{m}l^2t^{-2} \div t = [\mathrm{m}]^1\,[\,l\,]^2\,[\,t\,]^{-2}.$$

Power is, of course, a scalar quantity.

I hope that the reader will not think that I have laboured some of these points too much; but there is too much tendency, at the present day, to say: "What does it matter, if I can get the correct answer to a problem?"

In the first place, one does not always get the "correct answer"; and further, it is most necessary in Mechanics—as indeed in everything else—to *think clearly.* There is too much loose writing in most text-books on *Mechanics;* in fact, that is certainly one of the reasons why the subject is often considered *so difficult.* Velocity and speed are, as I have previously said, different conceptions, and so should never be confused together. Some of the best authors are by no means as careful as they should be in attending to this point. The consequence is that a good many of their equations are not homogeneous; vectors being frequently equated to scalars.

Even the very careful Maxwell nods. In his *Matter and Motion,* Article 113, "On Uniform Motion in a Circle," he says: "Hence when a body moves with *uniform velocity* in a circle, *its acceleration* is directed towards the centre of the circle, and is a third proportional to the radius of the circle and the *velocity* of the body."

Here we have Maxwell speaking of a "uniform velocity," which is being "accelerated"; *i.e.,* which is, consequently, *not uniform.* This slip should have been corrected in the 1920 Edition of this very valuable work

CHAPTER V

RELATIVITY

IN everyday Mechanics of real bodies we may say that there is one fundamental proposition underlying the whole subject, namely, that all the quantities are relative; *nothing is absolute;* everything is *relative to something else.*

Mr. Bertrand Russell, in his racy little book, *A.B.C. of Relativity,* says: "A certain type of superior person is fond of asserting that 'everything is relative.' This is, of course, nonsense [Why, "of course"?], because if *everything* were relative there would be nothing for it to be relative to."

This rather sarcastic remark is certainly not as clear as it might be, since it connotes the idea that things which are *relative* must, *necessarily,* be relative to things which are *not relative*—in other words, all things must be *relative to absolutes.* This involves a contradiction, since if a quantity (a speed, say), is relative to an *absolute speed*—and a speed can only be relative to another speed—it follows that the first speed becomes *also an absolute speed; i.e.,* it is both a "relative speed" and an "absolute speed" at the same time!

This statement of Mr. Bertrand Russell is not rendered clearer by his continuation: "it is possible to maintain that everything in the physical world is *relative to an observer.*" Now, what do we mean by the speed of a body being "relative to an observer"?

To make this sentence intelligible it appears to me to be necessary to assume that it is elliptical; and that what is really meant is that the speed of the body is relative to the *speed* of the observer. Thus, Professor Eddington (*Space, Time and Gravitation*), says: "The force measured with a spring-balance . . . depends on the *acceleration of the observer* holding the balance"; not, be it noted, "on the

observer," but on the *acceleration* of the observer. The weight being a "mass-acceleration" can only be relative to *another acceleration;* it cannot even be relative to a *uniform motion.*

If a quantity A is relative to a similar quantity B, it would appear clear that the quantity B is also "relative" to the quantity A. Therefore, even at the risk of being called a "superior person," I repeat that fundamentally every quantity is relative to other similar quantities.

This Principle is not new, being certainly as old as Aristotle; and it seems the only possible manner of considering natural phenomena. In all our conceptions, observations, and measurements of space, time, displacement, velocity, etc., it is necessary to remember that these are all "relative." We cannot, for instance, conceive a unique body in, otherwise, empty "space." We could not tell whether it was in motion or not; nor even specify *where it was.* All we can conceive and record is that any material point is at a certain distance, known or unknown, and in a certain direction, *from some other* initial material point. It is necessary, therefore, to have some "frame of reference," and such frame can only be formed in the presence of *other bodies.* Given these other bodies, we can then state that the first body is in such a position, *relative to them;* or that its velocity, say, is so much, *relative to that of some other body,* or bodies.

Since mundane space is of three dimensions it follows that, in order to define the *position* of a point, reference must be made to *at least* three other points.

In the same manner, in order to define the motion of a body, in our space of three dimensions, it is necessary to have *at least* three other bodies to which it may concurrently be referred. Thus all motion must be conceived as *relative motion;* motion detectable relatively to other bodies. As Leibnitz put it: "There is no motion when there is no *observable* change."*

In this quotation it is necessary to note that Leibnitz uses the word "observable," and not "observed." If the

* "Il n'y a point de mouvement, quand il n'y a point de changement observable."

motion produces no observable change, it is, clearly, outside the domain of Mechanics, which is essentially *based on measurement*, which is comparison of differences.

Similarly, time is *relative*. We say that a certain event happened before, or after, or at the same time as, some other event. We also " measure " the *time interval* between the two events, by comparing it with some *arbitrary standard*. Unrelated, or " *absolute* " *time* is meaningless to us.

Clerk Maxwell (*Matter and Motion*) referring to this point, says :

" Absolute space is conceived as remaining always similar to itself and immovable. The arrangement of the parts of space can no more be altered than the order in the portions of time. To conceive them to move from their places is to conceive a place to move away from itself.

" But as there is nothing to distinguish one portion of time from another, except the different events which occur in them, so there is nothing to distinguish one part of space from another except its relation to the place of material bodies. We cannot describe the time of an event except by reference to some other event, or the place of a body except by reference to some other body. All our knowledge, both of time and space, is essentially relative. When a man has acquired the habit of putting words together, *without troubling himself to form the thoughts which ought to correspond to them* it is easy for him to frame an antithesis between this relative knowledge and so-called absolute knowledge, and to point out our ignorance of the absolute position of a point as an instance of the limitation of our faculties. Anyone, however, who will try to imagine the state of mind, conscious of knowing the absolute position of a point, will ever after be content with our relative knowledge."

Obviously ! All that which we call " truth " can but be relative. Belief in the truth of a fact has no meaning, except in relation to some believer, real or supposed ; similarly, with unbelief. Some test, measure or criterion of the truth is always assumed necessary, and science recognizes no universal, unalterable or absolute criterion. Criterions change, and as they change, the truth changes in the same degree, and the old truth must be replaced by the new

truth. Again, no expression can be more relative than the term "*self-evident*"; what appears so to one man will often not to another, and may even appear altogether untrue. The term is, clearly, relative to the majority of ordinary educated persons.

As Paul Carus (*The Principle of Relativity*), says:

"Under all circumstances change modifies relations, and means 'transformation.' There is a transformation in the juxtaposition of things or their parts, and there is a succession of events. The scope of the former we call 'space,' of the latter 'time'; or, better, from the former we *deduce our notion of space*, and from the latter *our notion of time.*

* * * * * * *

"Time is *the measure of motion*, and space is *the scope of the motion.* Both time and space are presupposed in the idea of motion. *There is no time in itself, there is no space in itself.* What Newton and others with him call *absolute space* is 'space conception,' and what they call *absolute time* is 'time conception.'"

Clerk Maxwell (*Matter and Motion*), sums the matter up very clearly, as follows:

"Our whole progress up to this point may be described as a gradual development of the doctrine of relativity of all physical phenomena. Position we must evidently acknowledge to be relative, for we cannot describe the position of a body in any terms which do not express relation. The ordinary language about motion and rest does not so completely exclude the notion of their being measured absolutely, but the reason of this is, that in our ordinary language we tacitly assume that the earth is at rest.

"As our ideas of space and motion become clearer, we come to see how the whole body of dynamical doctrine hangs together in one consistent system.

"Our primitive notion may have been that to know absolutely where we are, and in what direction we are going, are essential elements of our knowledge as conscious beings.

"But this notion, though undoubtedly held by many wise men in ancient times, has been gradually dispelled from the minds of students of Physics.

"There are no landmarks in space; one portion of space is exactly like every other portion, so that we cannot tell where we are. We are, as it were, on an unruffled sea, without stars, compass, soundings, wind, or tide, and we cannot tell in what direction we are going. We have no log which we can cast out to take a dead reckoning; we may compute our rate of motion with respect to the neighbouring bodies, but we do not know how these bodies may be moving in space.

"We cannot even tell what forces may be acting on us; we can only tell the difference between the force acting on one thing and that acting on another."

Notwithstanding all that has been said previously, which implies that there is no such condition as *absolute rest*, we very commonly assume, in most arguments, that a certain point of reference is *at rest*. But the phrase "at rest" means, in ordinary language, "having no motion *with respect to that on which the body stands*." It cannot be made to mean more than this. We, therefore, in such arguments, assume, in order to simplify the question, that the earth is *at rest*.

In Paul Carus' words (*The Principle of Relativity*):

"We must bear in mind that the way of making knowledge possible in all the flux of being *is to ignore what has nothing to do with the problem* under investigation. Our method is based upon a fiction or, if you please, upon an artificial trick, *viz.*, to ignore complications and to consider a certain thing as fixed; but there are cases in which we must remember that we ourselves change and that the very position we assume is changing.

"This way of ignoring what does not concern us at the time is *an artificial process*, a process of abstraction and elimination, of cutting off all disturbing incidents, and in doing so the philosophically minded scientist will become aware of the fiction of arbitrarily laying down a point of reference which is treated *as if it were stable*, whilst in fact, like everything else, it, too, is caught in the maelstrom of cosmic existence.

"There is nothing wrong or harmful in this fiction; on the contrary, it is *an indispensable part of our method of comprehending things*."

Such is the ordinary theory of " relativity " in Mechanics ; but latterly a special form of relativity (of which there are several varieties) has been evolved, which, by its extra-ordinary implications, has created widespread interest and excitement. I am afraid I must confess my inability to understand it—some leading Physicists and Astronomers confess the same—but I must endeavour to give some slight account of it.

The " Principle of Relativity," as it is called, is chiefly advanced by Mathematicians. It appears to be essentially a deductive system ; that is, it is based on axioms and postulates. Some of these assumptions come into collision with the primary concepts of classical Mechanics. Some of the deductions from these assumptions lead also to most extraordinary statements.

As Paul Carus points out : " The old self-contradictory statements of the Eleatic School revive in modernized form, and common sense is baffled in its attempt to understand how *the same thing* may be *shorter and longer at the same time,* how a clock will strike the hour *sooner or later according to the point of view from which it is watched.*"

If we granted the assumptions of the new Relativists, and accepted the " Principle," it would be necessary to discard the Newtonian system of Mechanics, and to frame a new one ; the materials from which this could be framed are not, however, very obvious.

We shall have such terms as " transverse mass," and " longitudinal mass." Mass and time will both be functions of speed ; and we shall further have the very complex idea of " side-ways time." Light will also be a " complex of energy "—however we may interpret that expression.

Let me give an example which leads to what Norman Campbell (*Common-sense of Relativity*), calls : " paradoxical conclusions deduced from the Principle, which are sometimes elegant and entertaining, but more often fallacious."

Messrs. G. N. Lewis and C. Tolman (*The Principle of Relativity and Non-Newtonian Mechanics*), starting from the assumption of a stagnant ether, say :

" (1) *Absolute* uniform translatory motion can neither be measured nor detected.

" (2) The velocity of light in free space appears the same to all observers regardless of the motion of the source of light or of the observer.

" (1) and (2) constitute the ' Principle of Relativity.'

THE UNITS OF SPACE AND TIME

" The following development will be based solely upon the conservation laws, and the two postulates of the Principle of Relativity.

" The first of these postulates is that there *can be* no method of detecting *absolute* translatory motion through space, or *through any kind of ether* which may be assumed to pervade space. The only motion which has *physical significance* is the motion of one system *relatively to another.* Hence, two similar bodies having relative motion in parallel paths form a perfectly symmetrical arrangement. *If we are justified* [?] in considering the first *at rest* and the second in motion, we are equally justified in considering the second at rest and the first in motion.

" The second postulate is that the velocity of light as measured by an observer is independent of relative motion between the observer and the source of light. This idea that the velocity of light will seem the same to two different observers, even though one may be moving towards and the other away from the source of light, constitutes the *really remarkable feature of the Principle of Relativity,* and forces us to the strange conclusions which we are about to deduce.

" Let us consider the two systems, moving past one another, with a constant relative velocity, provided with plane mirrors *aa* and *bb* parallel to one another, and in the line of motion (Fig. 2). An observer A on the first system sends a beam of light across to the opposite mirror, which is reflected back to the starting-point. He measures the time taken by the light in transit.

" A, *assuming that his system is at rest* (and the other in motion), considers the light passes over the path *opo,* but believes that if a similar experiment is conducted by an observer B in the moving system, the light must pass over the longer path *mnm'*, in order to return to the starting-point. For the point *m* moves to the point *m'* while the

light is passing; he *therefore predicts* that the time required for the return of the reflected beam will be longer than in his own experiment. A, however, having established communication with B, learns that the time measured is *the same* as in his own experiment.

"The only explanation that A can offer for this surprising state of affairs is that the clock used by B for the measurement does not keep time with his own, but runs at a rate which is to the rate of his own clock as the lengths of the paths *opo* to *mnm'*.

"B, however, is *equally justified* in considering his system at rest, and A's in motion, and by identical reasoning has

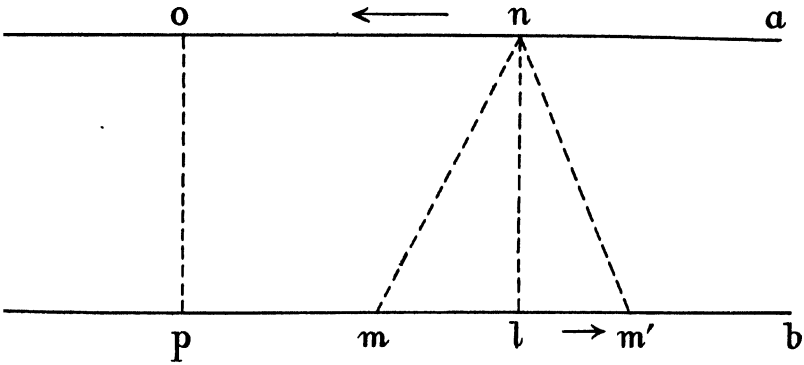

FIG. 2.

come to the conclusion that A's clock is not keeping time. Thus to each observer it seems that the other's clock is running too slowly.

"This divergence of opinion evidently depends not so much on the fact that the two systems are in relative motion, but on the fact that each observer *arbitrarily assumes that his own system is at rest.* If, however, they both decide to call A's system at rest, then both will agree that in the two experiments the light passes over the paths *opo* and *mnm'*, respectively, and that B's clock runs more slowly than A's. In general, *whatever point may be arbitrarily chosen* as a point of rest, it will be concluded that any clock in motion relatively to this point runs too slowly."

From the foregoing it would not be difficult to show that,

to an independent observer, both clocks are going both faster and slower than one another *at the same time.* This may be *called* " relativity," but I should call it " *nonsense.*"

The first postulate is that *all motion* is only to be considered as " relative motion " ; and yet the authors calmly *assume* that both A and B have *absolute motion*— " relatively " to *nothing at all.*

The postulation of a " stagnant ether " is not perhaps unreasonable (in this argument), but in *reality* it conflicts with postulate I. The speed of light is assumed to be *constant.* This, also, is not unreasonable ; but, examined carefully, it can only be considered as *conditionally true ;* the condition being that *the density of the ether is constant.* The speed is, of course, *relative to the ether*—which has been postulated *at rest.* Essentially, therefore, the speed of the light is *an absolute speed.*

As Messrs. Lewis and Tolman very rightly say, A and B (in the example given) get into a hopeless tangle in consequence of each *assuming*—which, of course, he has no right to do—that he is at *absolute rest ;* that, in fact, *both* are animated by *absolute speeds.*

Their observed relative speed being v, say, the very utmost that they would have been justified in assuming would be that their speeds were $+v/2$ and $-v/2$, respectively. If A and B had made this assumption only, they would have found that there was no necessity to wrangle about the correctness of their respective clocks.

Directly one assumes any *absolute,* one becomes led into absurdities. Assume *infinite space* and it is quite easy to prove, *mathematically,* as Boscovich has done, that a *part* of this is *twice as great as the whole.*

Mr. Norman Campbell (*Relativity and the Conservation of Momentum,* Phil. Mag., 1911), referring to the example quoted, says : " ' The observer A, considering himself at rest, concludes that the real change in velocity, etc.' If A considers himself to be at rest, while he admits the Principle of Relativity, which states that he *can have no rational ground for such a belief,* his pronouncements on any subject must be received with considerable scepticism. When he proceeds to calculate the ' real change,' without

explaining what he means by the term, I have a suspicion that he is using words to which he cannot attach any signification whatsoever."

The same author (*Common-sense of Relativity*, Phil. Mag. 1911), also says: "Why, it may be questioned, do we drag in the velocity of light, rather than that of sound, or of the trains on the two-penny tube?"

Professor Louis Trenchard More (*The Limitations of Science*, 1915), also remarks: "Suppose a race of men to exist who are blind and have no knowledge of electro-magnetic radiation, but who wish to measure the lengths of moving bodies. They will undoubtedly be compelled to get this information and that of the synchronism of clocks by sound signals. It is evident that observations carried out under conditions similar to those imposed by Professor Einstein would indicate that *the length of a moving body underwent changes.* And while they could make corrections for some of the effects, because sound waves are largely affected by the motion of media and of sonorous bodies, yet they would undoubtedly come to the conclusion that the dimensions of a moving body depended to some extent on its motion. Now, if we should bestow sight on one of these men, he would be able to correct their measurements; as he could by his immensely more rapid light signals gain a much more nearly instantaneous value for synchronism. We are, at present, in the condition of this man. As we improve in our ability to measure the velocity of light under different conditions, we shall, Professor Einstein thinks, get closer to the knowledge of the absolute V, and to the relations for space and time which he has derived. But we may suppose men will some day find a kind of radiation which has a velocity greater than V (for example, the transmission of gravitation), and by its aid *remove the conviction remaining in our minds that motion affects length and time.* Calculation may show that material bodies cannot attain this velocity, but we are speaking of an immaterial radiation. To say that such a radiation is impossible is as futile, at least as unscientific, as for a race of the blind to say that there is no light."

M. Henri Poincaré, also, very pertinently, remarked, in

La Valeur de la Science, 1914: "What would happen if one could communicate by signals which would be no longer luminous and of which the velocity of propagation would differ from that of light? . . . And are such signals inconceivable, if we admit, with Laplace, that universal gravitation is transmitted *a million times faster than light?*"

Professor L. T. More also says in another place:—

"What clear idea is conveyed by Professor Einstein's definition that *vacuous space contains radiant energy*, which is *an entity of the same kind as matter?* Does he not add to the difficulty when he says further, that *the difference between a vacuum and the ether* is that the latter is *a vacuum transmitting radiant energy and possesses a light vector.* What right has he to insinuate into our minds that a vacuum *may contain something and still be a vacuum?* He does this by a play on the word 'energy,' which he permits us to think of in the ordinary sense as an attribute of matter, and at the same time states implicitly to be a *distinct entity*. . .

"But if we put this definition to a simple test we easily see how futile it is. Say to anyone, that a golf ball in its flight is not *a thing of rubber and paint*, but a *complex of energy;* and that this is true because the golf ball *has a motion vector*, and consequently *changes vacuous space into ether.* How quickly such a statement about a familiar action would be recognized as an absurdity! I presume that the reason why we like to indulge in these phantoms of the imagination is because we still *hate to confess our ignorance.* . . . He clinches the whole matter and explains the changes of the dimensions of moving bodies by introducing *the occult idea that light is an entity* which moves in space *with a constant velocity* . . . he does not hesitate to found physical science on the paradox that *motion cannot be absolute*, but *the motion of light is absolute.*

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"Lastly, Professor Lewis confuses scientific method utterly, by *arbitrarily assuming which quantities in an equation shall be treated as variable and which as constant.* Thus, he says, if the momentum of a body changes, *let us suppose* that this happens *not because its motion changes but because we shall consider its mass variable.* Of course, anyone

can say *let us consider the universe to act as he wishes.* But, after all, what is the use when no one believes it does."

On this question Lewis and Tolman say: "It has, however, been universally considered that the charge *e* [of an electron] is constant. In other words, that the force acting upon the electron in a uniform electrostatic field is *independent of its velocity.* Hence the observed change in *e/m* is *attributed solely to the change in its mass.* It might be well to subject this view to a more careful analysis than has hitherto been done. At present, however, we will *adopt it without further scrutiny.*"

M. Henri Poincaré (*La Science et l'Hypothèse*, 1912) points out that Messrs. Abraham and Kaufman "were obliged to admit an hypothesis; they *assumed* that all the negative electrons were *identical*, that they carry *the same, essentially constant, charge*, that the dissimilarities [dissemblances] which one observes between them proceed *solely and entirely from the different speeds with which they are animated.*"

In short, the whole explanation is based on a variety of certainly *questionable assumptions.*

By a similar style of arbitrary loose reasoning, if it were *assumed* (or "universally considered," if that sounds better), that *the mass is constant*—which is a fundamental assumption in *Dynamics;* and if a plebiscite were taken, the majority in agreement with this assumption would probably be very large—it could be shown that the charge *e* is *not constant*, or that what is called "the force acting upon the electron" is *not constant*—but that it *varies with the speed.*

The fundamental assumption in this argument, which necessitates the making of the other later assumptions in order to bolster it up, is the assumption of the *conservation of momentum.* This, which is absolutely true by assumption in pure Rigid Dynamics, is *never true* when we are dealing with real bodies; no motion can be changed without some small loss—some of the energy being necessarily transformed into heat.

Norman Campbell (*Relativity and Conservation of Momentum*), very pertinently says that Relativists "can make *any assumptions they like*, happy in the confidence that they will never have to make any more, unless they want

to do so, because their theory cannot possibly come within the range of experiment."

Some of their " conceptions "—*assumptions*—have no clear ideas connected with them. For example, having agreed, in Newtonian Mechanics, that actual space and time do not exist, but are really only conceptions, in the viewing of phenomena—which we view " in time," or " in space "—what are we to understand when we are told that gravitation is " a kink in space " ? We can understand a distortion of an *ether*, but not a distortion of *space*. How can we test, experimentally, whether bodies do or do not, travel in " geodesic lines " ? It is not even quite clear what " geodesic lines " *are*.

Einstein having postulated that all motion is only " relative " and that, consequently, the expression *absolute rest* is meaningless—(and in this, I apprehend we are all agreed)—says, in his *Relativity* (English translation), that an observer on a disc " *may* regard his disc as a reference-body which is ' at rest ' ; on the *general Principle of Relativity he is justified in doing this.*"

If the earth were a flat circular disc should we be justified in considering it as *at rest?* Is the property of " flatness " an essential to *absolute rest*, and why ?

In Einstein's Principle of Relativity the existence of the ether is sometimes inconvenient ; in such cases the ether is suppressed—or, in any case, put into reserve, until it is convenient to produce it.

These are a few of the difficulties. This new " relativity " is by no means universally accepted by men of science. Professor Magee, in his Presidential address to the *American Physical Society*, 1911, said : " I do not believe that there is any man now living who can assert with truth that he can conceive a *Time* which is a *function of Velocity*, or is willing to go to the stake for the conviction that his ' now ' is another man's ' future,' or still another man's ' past.' "

" The Principle of Relativity is not strictly a *physical law*, but the expression, in Mathematical symbols, of the general philosophical law of *the finite nature of the human mind*, which has been accepted for centuries . . . unless we believe that *something*, matter, energy, or both, *remains*

unchanged in appearance, and unless phenomena can be repeated, we have no certainty of knowledge and *no means of communicating ideas to others.*" (L. T. More, *The Limitations of Science.*)

In conclusion, " It is the great merit of the Principle of Relativity that it forces on our attention the true nature of the concepts of ' real time ' and ' real space,' which have caused such endless confusion." (Norman Campbell.)

My fundamental objection to these new varieties of relativity is that they appear to be based on assumptions, which sometimes contradict one another.

That *absolute* uniform motion could not be detected—much less, measured—is accepted by everybody. When, however, this statement is expanded, so as to mean that *there can be no method* of detecting any motion *relatively to the ether*, then, I think, we are asserting more than we have a right to. We may not have been able to do so up to the present, but to say that it is *inherently impossible* is pure *extrapolation.** Professor Dayton Miller's vast series of experiments tends to show that such relative motion *can be measured.*

The assumption of the *absolute speed of light* contradicts the first assumption: the expression " absolute speed " conveys no meaning to my mind; and Einstein has never defined what he means by the word " light."

That the speed of light *through the ether* or *relatively* to the ether, is *constant;* even this is, I think, only *conditionally true*—the condition being that *the density of the ether remains constant.* To assume this is *extrapolation.**

In any case, to base a theory of " relativity " on an " *absolute* " appears like a contradiction.

If we follow Newton (*Opticks*, Query 21), " The velocities of the Pulses of elastick mediums are in a sub-duplicate *ratio* to the Elasticities and the Rarities of the medium taken together,"

$$C^2 = 1/k\rho, \text{ where } \rho = \text{density of ether.}$$

Substituting $1/k\rho$ for c^2, in Einstein's equations, where he puts v^2/c^2, this would become $k\rho v^2$—which is a measure of energy. This is the exact amount required by Einstein for the shortening of his measuring rod; and it gives a Physical explanation of why this rod *should be Physically shortened.*

* Which should be abomination to a good Physicist.

If we consider the question carefully, we are irresistibly led to the conclusion that in dealing with real bodies, *uniform motion* of translation is absolutely *impossible;* since it could not be generated (except for a very minute instant) and it could not be uniformly retarded by any medium.

I will conclude this Chapter with an apposite quotation from Locke's *The Conduct of the Understanding:* "Where men have any conceptions, they can, if they are never so abstruse or abstracted, *explain them* and the *terms they use for them.* For our conceptions being nothing but ideas, which are all made up of simple ones, if they cannot give us the ideas *their words stand for* it is plain they have none . . . therefore, to obtrude terms where we have no distinct conceptions . . . is but an artifice of learned vanity to cover a defect in hypothesis, or our understandings."

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CHAPTER VI

THE NOTION OF FORCE

WE will now discuss the Dimensions of that much abused conception called "Force." In the first place what do we mean by the word "Force"?

Referring to Thomson and Tait, *Elements of Natural Philosophy*, 1885, we find: "183. *Force* is any *cause* which *tends* to alter any body's natural state of rest, or of uniform motion in a straight line. . . Forces may be of different kinds, as pressure, or gravity, or friction, or any of the attractive or repulsive actions of electricity, magnetism, etc."

In another part we also find: "The standard or unit force is that force which, acting on a national standard unit of matter, during the unit of time, generates the unit velocity."

I have quoted this definition, not because I think it is satisfactory, but because it is that adopted, implicitly, if not explicitly, in almost all, even modern text-books.

For example, John Cox, *Mechanics* (Camb. Univ. Press, 1909), defines force as "*anything* which *changes* or *tends to change* a body's state of rest or motion." He, also, calls pressure "a force"; tension "a force"; etc.

To my mind, this is exceedingly confusing to any young student, since he will, of course, consider "Force" as *an agent* or *thing*—something *objective*, and which has a quasi-personality—which *causes* or *tends to cause* motion. He will, further, confuse force, pressure, and tension; so as to think that pressure and tension *are* forces. Indeed, he is distinctly told that pressure and tension *are* "different kinds of force." This leads, at times, to hopeless confusion. For example, if he is taught later anything about "Dimensions," he will in all probability be taught that the dimensions of pressure are $[ML^{-1}T^{-2}]$; whilst the Dimensions of force are $[MLT^{-2}]$; also that Pressure = Force ÷ Area.

As will appear later, Force is only a *concept* of the mind and not a *percept;* it is purely *subjective,* and not *objective.**

In making the above quotation from Thomson and Tait, I must hasten to confess that I have not acted quite fairly to the memory of Professor Tait. It is quite true that this was his view in 1885, but his later and more matured judgment was very different. In his *Properties of Matter,* Fourth Edition, 1899, we read:

"If, for the moment, we use the word *Thing* to denote, generally, whatever we are constrained to allow has *objective existence—i.e.,* exists altogether *independently of our senses* and of our reason—we arrive at the following conclusions:—

"(*a*) In the Physical Universe there are but two classes of *things*—matter and energy.

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"The word *force* must often, were it only for brevity's sake, be used in the present work. As it does not denote either matter or energy it is *not a term for anything objective.*

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". . . The great majority even of scientific men, still cling to the notion of force as *something objective.*

". . . Force is a mere *phantom suggestion* of our muscular sense.

"I have seen *so much mischief done* by this quasi-personification of a mere sense impression that, even in an elementary book, I am constrained to protest against it. I feel assured that the difficulties which are now everywhere felt as to the great scientific question of the day, the nature of what we call electricity, are in great part due to the way in which our modes of thinking have been, by early training and subsequent habit, *encouraged to run in this fatal groove.*"

From the foregoing exceedingly powerful and clear statements—they could hardly be stronger—it will be seen that Professor Tait, towards the end of his life, did not consider Force as *a thing;* though he, himself, unfortunately (probably

* I suppose that we must recognize that the idea of "Force" as an objective entity has come down to us as a legacy at least from Aristotle, who deals with the "doctrine of force."

from long habit), occasionally employs the word in a quasi-personal sense; and this, at times, detracts from clearness. However, one must be a poor psychologist and have little knowledge of oneself not to know how difficult it is to liberate oneself from traditional views; and how, even after that is done, the remnants of the old ideas still hover in consciousness and are the cause of occasional backslidings even after the victory has been practically won.

It may be objected that Newton, in the *Principia*, employs the word Force in this quasi-personal manner: Force is "impressed," it "*causes* motion," etc. This is apparently, and most unfortunately, true. Profound, however, as may be our admiration for this extraordinary book, and the transcendent genius of its author, is it really necessary that we should, with our present knowledge, indulge in "Newton-olatry"? Of all the delusions of man, perhaps the most difficult to cast forth is an "olatry." Whether a man loves his idols or fears his idols, for some reason he is as unwilling to test them as he is unreasoning in his worship.

As W. Emerson, *Newton's System of the World; a Short Comment on, and Defence of, the Principia*, 1803, says: "When one person has another man's work to look through, he must be a great blunderbuss that cannot make some small additions: a man placed upon another's shoulders will see further than his supporter."

We should ever remember what Bacon said: "Credulity in respect of certain authors, and making them Dictators instead of Consuls, is the principal cause that the sciences are no further advanced. Let great authors, therefore, have their due, but so as not to defraud Time, which is the author of authors, and the parent of Truth." (*Advancement of Learning.*)

Is it *quite certain*, however, that Newton did conceive "Force" as *a thing* (something objective) which *causes* or *tends to cause* motion? I have very serious doubts whether this was his view. I am not aware that Newton ever specially *defined* "Force"; but his definition of "impressed force" is that it is *an action*. His actual words are: "Definition IV. An impressed force is an *action exerted* upon a body,

in order to change its state, either of rest, or of moving uniformly forward in a right line." (Motte's translation.)

Newton was most careful, however, to say in his Definition VIII: "I use the words Attraction, Impulse, or Propensity of any sort towards a centre, *promiscuously* and indifferently, one for another, considering those forces *not Physically but Mathematically;* wherefore, the reader is not to imagine that, by those words, I anywhere take upon me to define the kind, or the manner of any action, the causes or the Physical reason thereof, or that I attribute Forces, *in a true and Physical sense,* to certain centres (which are only Mathematical points), when at any time I happen to speak of Centres as attracting, or as endued with attractive powers." (Motte's translation.)

We see very clearly, in his Definition IV, that Newton considered "impressed force" *an action*—a *state* of acting or being active—exerted, or impressed upon a body, to change its state of motion.

We also read that "This force consists *in the action* only; and remains no longer in the body, *when the action is over.*"

Besides this, and apparently in order to further explain himself, Newton gives examples: "Impressed forces are of different origins; as *from* percussion, *from* pressure, *from* centripetal force."

Reading this as an ordinary piece of English, we understand that Newton's impressed force is *in an action only;* and that it remains no longer *in the body* when the *action is over.* It is clear, therefore, that the impressed force is not *a thing*—it is not objective—since it *disappears when the action ceases.*

We see no mention in the definition of any *thing* which changes, or *tends to change,* a body's state. An "action" is not a *thing;* and the words "or tends to change" are not in the definition at all. This latter is a pure gloss, as was clearly pointed out by Tait, in his *Newton's Laws of Motion,* 1899: "It is particularly to be noted that Newton says nothing about forces which *tend* to produce change of momentum [a change from Tait's own view in 1885]. According to him there may be balancing of *the effects of forces,* but there is no *balancing of forces.*" Tait added later,

in square brackets: "[The whole of this trouble is introduced by the *anthropomorphic notion*, Force. All we can say is that no transference or transformation of energy takes place.]."

Reflecting on all this we must, I apprehend, come to the conclusion that Newton never intended to imply that "*impressed* force" was the action, but rather that it was to mean *the intensity*—or, *measure of the intensity*—of the action. In other words, that "force" was used in the same sense that we employ it when we speak of "*the force* of a blow," or "*the force* of an argument."

If we accept Tait's statement (*Properties of Matter*, Fifth Edition, 1907) that, "Newton's notion is, if there is a force at all, *it is doing something*"—a rather anthropomorphic view of force, perhaps—we see that if there is *no change of momentum* there *can be no force*. Consequently, as before, force cannot be objective.

We may examine the question from another point of view, that of Newton's Second Law of Motion.

A. E. H. Love, *Theoretical Mechanics*, 1897, in referring to this "Second Law," says: "That the Laws of Motion are of the nature of postulates is clearly recognized in Newton's word, *axiomata*, but it is now held that they *also partake of the nature of definitions*."

The latter part of this paragraph is not always sufficiently recognized. The view is not a new one, since it was pointed out by Playfair, *Outlines of Natural Philosophy*, as long ago as 1819. This Second Law gives a very clear *definition of force;* and it is to this view of the Law that I will refer.

The Second Law of Motion (Motte's translation) is as follows: "The alteration of Motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed."

Most of the authors of text-books are satisfied to repeat this without much comment; but the two whose works I have specially selected as characteristics of the best examples are exceptions. Newton's word "motion" here means, of course, what we now call "momentum." This is clear from his Definition II.

Cox, in his *Mechanics*, quotes the Law in Newton's own

words, and then he adds : " The word *proportional* in Law II is to be taken in the strict Mathematical sense ; *i.e.*, questions on the Second Law are to be worked out by the Rule of Three, or proportion."

Now everyone knows that this is *not true.* A *very small* " force " can generate a very *great momentum.* So they are not proportional. Mr. Cox knows this perfectly, since he continues :

" In measuring the *impressed force* we *must* [why " must " ?] take into account not only the magnitude of the force, but *also the time* during which it acts ; since *the longer the force acts* the greater the change of momentum *produced.*

" The total effect of a force in *producing* change of motion is called impulse (*i.e.*, total push).

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" 121. We can now state the Second Law as follows : *Momentum produced is proportional to the impulse of the force acting, and is in the direction of the force.*"

Thus Mr. Cox tells us (1) that momentum *changed* is Mathematically proportional to the motive force ; and then that this is incorrect, but that (2) momentum *produced* is proportional to something else, called " impulse."

This alteration of the Law does not, in my opinion, tend to clarity ; in fact, one does not recognize it as " Newton's Law," which speaks of " Force " and not of " Impulse." The substitution, also, of " momentum produced " for " momentum altered," is not a happy change ; since the momentum might be *reduced,* and not increased.

That Newton really intended time to be taken into account (although he does not mention it) cannot reasonably be doubted. This will be clear from a reference to the Scholium of Corollary VI (Book I, *Laws*) or to Prop. 24, Book II, where he states most definitely : " For the velocity which a given force can generate in a given matter *in a given time,* is as the force *and Time* directly, and as the Matter inversely. The greater the force *or the time,* or the less the matter, the greater will be the velocity generated. This is manifest from the Second Law of Motion."

This may have been " manifest " to Newton, but it is certainly not to a twentieth-century reader. I venture to

suggest that this Law was *unfortunately worded.* Taken as it is worded—and as a *general law*—it is not true. The Law is obviously a *conditional law* only ; the condition being that the *time must be constant.* This condition is, unfor-fortunately, *not stated in the Law.*

Newton is here endeavouring to ignore, as far as possible, any outside *impressing,* as opposed to *impressed* force, known by its observable action ; in other words, to keep clear of metaphysics. He does not say that " change of motion " is proportional to the *outside generating force* but to the inside *impressed force ;* which force persists as long as the change itself, and no longer.

I think that Newton's intention can be put beyond doubt, if we refer to his 7th and 8th Definitions.

" Definition VII. *Accelerative quantity* of . . . force is *measure of the same,* proportional to the *velocity* which it generates *in a given time.*"

" Definition VIII. *Motive quantity* of . . . force is *measure of the same,* proportional to the *motion* which it generates *in a given time.*"

Force, comparative force, *the* force, according to Newton, therefore, connotes the condition " *in a given time.*"

If this condition be inserted—and in Newton's own words—the Second Law would read as follows : " [In a given time] change of motion is proportional to motive force impressed, etc."

The Law, as thus stated, is a *general law ;* and *no alteration* has been made in Newton's wording of it ; the " condition," only, has been inserted, and this addition is also *in Newton's own words.*

My attention has latterly been drawn to W. W. Rouse Ball's *Essay on Newton's Principia,* where the author states Newton's Second Law as : " The change of momentum [per unit of time]* is always proportional to the moving force impressed, etc."

We have here the condition, which I have just referred to, inserted. It is, practically, the same as the re-statement which I have given above. At the same time I prefer my own, since I have inserted the " condition " in *Newton's*

* Square brackets in original.

own words. Besides this, also, if one speaks of " per unit of time," the fact that the Time is *an essential condition* in the statement is very apt to be overlooked by a student. If, however, one says " in a given time," this is much less likely to occur.

Sir Richard Glazebrook, in his *Dynamics*, first states the Second Law of Motion in Newton's own words. After some quite sound reasoning, which it is unnecessary to repeat, he says :

" We may thus re-state the Second Law :

" Law II. *Rate of change of momentum is proportional to the impressed force and takes place in the direction in which the force is impressed.*"

This statement cannot be objected to (in fact, it is the special point which I am driving at), but *it is not Newton's Law.* Newton makes no reference to the " rate of change " ; but only to the " change." It is, however, quite a logical deduction from the Law, and from Newton's remarks on it. It is really *a definition of Force.*

Why Glazebrook retained the words " proportional to," is not clear ; they tend to obscure the meaning of his definition. In another place he puts his meaning beyond doubt, *e.g.*, " the rate of change of momentum *has received a name*, it is *called force.*"

The *Time*-Rate of change of momentum *is* Force, and consequently it cannot be " proportional to it." A thing cannot be *proportional to itself.*

" Force, as *a cause of motion*," Glazebrook says, " we have not here to consider ; it will suffice for us to define it as [time] *Rate of change of Momentum.*" Also, " Force is often looked upon as *something* external to the body, acting on it and *causing it to move.* Now when we say that a body is moving under the action of a force, all that we can observe is *a change in the momentum of the body.*"

I would prefer to word this as follows : " Now when we say that a body is moving under the action of a *stress*, all that we can *observe* is the change of the momentum of this body. The *Time-Rate* of this change is the measure of *the force*, or the *intensity* of the stress."

We are here observing the motion " in time " ; if, how-

ever, we are observing the motion " in space," as we almost invariably do when observing *real bodies*, and not the imaginary, propertyless bodies of Dynamics, what we *do* observe is the *transformation of energy*.

If we accept Glazebrook's definition of " force " as : " The [time] rate of change of momentum " ; (and I cannot see how we can refuse to do so), we may, also, enunciate it somewhat differently, and say : " Force is the time-rate of mass-acceleration." This is the measure of the *intensity of the action*.

RATE OF MASS-ACCELERATION

In the same manner that " mass-velocity " has been called " momentum," we may say that " rate of mass-acceleration " has been called " force."

We have, in Glazebrook's definition, force defined *in terms of momentum*. We might, similarly, define momentum *in terms* of force, and say : " Momentum is the time-integral of force."

Since momentum is *a rate*, a ratio, *i.e.*, in Mathematical language, $Md\mathrm{S}/d\mathrm{T}$, it is clear that Force must also be a *rate* —an *abstract quantity*—and not a *Thing*. In fact, force is *a rate of a rate*—$Md\mathrm{V}/d\mathrm{T}$, or $Md^2\mathrm{S}/d\mathrm{T}^2$—a differential of the second order.

The dimensions of force are $[\mathrm{M}]^1\ [\mathrm{S}]^1\ [\mathrm{T}]^{-2}$.

RATE

I will make a small digression here and refer again to the meaning of " rate."

As was previously pointed out, the word " rate " is practically synonymous with " ratio." Mathematicians have, however, got into the habit of using the word " rate " as if it were synonymous with " time-rate " ; in fact, a professor told me that " rate " was *always a time-rate*. This is an arbitrary restriction of its meaning, which is hardly justifiable, and is frequently confusing. We can, and do, speak of " space-rate," or " distance-rate," etc. In fact, Descartes gives a definition of Force, which is a " space-rate."*

* " Force : c'est le nombre qui, multiplié par la dimension de l'espace parcouru, *donnera un produit constant*" !

There is no obscurity about this : force is a *pure abstract quantity*—not a *thing*—but *a measure of the intensity of a stress*.

Rate of interest, rate of exchange, rate of discount, birth and death rates, and local rates, are all *rates;* but they are not *time-rates.* It would tend to simplicity and clarity if authors would always specify (once, at least) whether they refer to " time-rate," " space-rate," " distance-rate," or any other kind of rate.

When a Mathematician refers to an integral he, almost invariably, refers to it as a " time-integral," or " space-integral." It would not be unreasonable to suggest that the same practice should be adopted when referring to a rate.

After this small digression let us see what Karl Pearson says about Force, in his *Grammar of Science.* Referring to the Definition and Second Law of Motion, he says :

" This is a veritable metaphysical somersault. How the imperceptible *cause* of change of motion *can be applied in a straight line* surpasses comprehension ; the only straight line that can be conceived, or, as some physicists would have it, *perceived,* is the *direction of the change of motion.* We may assert that the imperceptible has this direction, but to postulate that the imperceptible *will determine this direction* for us seems to be pure metaphysics.

" We come down to our feet again, however, when we interpret this Law as simply indicating that, physically, force is going to be taken *as a measure for some change in motion.* . . .

" This glib transition from *force as a cause* to *force as a measure of motion* too often screens the ignorance which it is as much the duty of science to proclaim from the house-tops as it is its duty to assert knowledge on other points."

We see here a metaphysical difficulty in understanding the Second Law very clearly pointed out by this exceedingly clear thinker and lucid writer. We are also shown the way out of the difficulty.

Karl Pearson develops his argument later. Assuming that two bodies A and B are acting on one another —*accelerating one another*—he says :

$$\frac{\text{" Acceleration of B due to A}}{\text{Acceleration of A due to B}} = \frac{\text{mass of A}}{\text{mass of B}}$$

" Hence it follows that

mass of A × acceleration of A due to B
= mass of B × acceleration of B due to A.*

" We will then give a name to this product of *mass into acceleration;* we will term the product of mass A into the acceleration of A *due to the presence of* B, the *force of* B *on* A. This will be considered to have the direction and sense [currency] of the acceleration of A due to B, whilst its *magnitude* will be obtained by multiplying *the number of units* in the acceleration of A due to B by the *number of units* of the mass A. Thus the proper measure of a force will be its *number of units of mass-acceleration.*"

This is, in simple language, the measure of *the force* of the " action," or its intensity.

Now, it is clear that if we say that " the direction of the *change of momentum* is the same as that of *the force,*" it is the same as if we said that " the direction of *the force* is the same as the direction of the *change of momentum.*" What we really mean is that they are both vectors.

Now, we must always reason from the known to the unknown ; *i.e.*, we ought to express the *unknown* " Force " in terms of the *known,* and easily measurable " Momentum." I suggest, therefore, that the Second Law would be best re-stated, in more modern language, as :

> " In a given time the force impressed is proportional to the change of momentum, and it has the same currency."

This in no way alters Newton's meaning in his Law, and I have, as far as possible, retained *Newton's own words.* There is no theory, and no metaphysics, in this statement. It is simply the record of a fact of experience.

In considering the Definition of Force, and the Second Law, as to which should have precedence, Dr. Whewell pointed out that : " In Science a definition and a proposition commonly enter side by side—the definition giving exactness to the proposition ; the proposition giving reality to the definition." The two are intimately connected.

* For " acceleration " read *rate of acceleration ; the measure of the intensity* of the action of B due to A.

Playfair, in his *Outlines of Natural Philosophy*, 1819, says: "It (the Second Law) seems to be, in fact, a *definition* rather than a theorem. We have no distinct idea attached to the word Force, which we can compare with that conveyed by the formula dv/dt, in order to see whether there is any necessary agreement between them or not. But as the quantity dv/dt is of great importance, and frequent recurrence, in Mechanical investigations, it is convenient *to have a term to denote it.* Though any term might be employed for this purpose; yet as the thing called Force is conceived to be always greater, the greater the change of velocity it produces in a given time, or to increase and diminish just as dv/dt does, we may without diverting the word Force from its usual signification, employ it to denote the quantity dv/dt, or the momentary increase of the velocity, divided by the corresponding increment of time. Force, in Dynamics, has, in reality, *no other signification than this;* the one expression may be everywhere substituted for the other, and thus an entire treatise of Dynamics might be written in which *the term force would not once occur.*"

This view of Force was not very new even in 1819, since d'Alembert, in his *Traité de Dynamique*, 1743, said: "All that we can see distinctly in the motion of a body is that it traverses a certain space, and that it takes a certain time in traversing it. It is, therefore, from this single idea that one should draw all the Principles of Mechanics when one wishes to present them in a simple and precise manner; hence it will not be surprising, that in consequence of this reflection, I have, so to say, turned the view away from *the motive causes* in order to consider solely *the motion which they produce.*"

The force is not *the cause;* but we may suppose that some "agent" produces an "action," and that *the force* (or *intensity* of this action) is measured by the amount of the "mass-acceleration" produced in a given time.

I have said that I had doubts as to whether Newton ever intended "Force" to be considered as *an agent.* He certainly employs the word in this sense on some occasions. When he does so, however, I am inclined to think that he looked on "cause" as something supernatural, and consequently not

measurable, nor coming within the domain of Physics, or Mechanics. Many of his letters betray a supernatural outlook; and so lend colour to this view.

Besides this, in the long scholium to the Definitions, Newton definitely says: "Wherefore relative quantities are *not the quantities themselves* whose names they bear, but those *sensible measures of them** (accurate or inaccurate) which are commonly used instead of the *measured quantities themselves*. And if the meaning of words is to be determined by their use, then by the names time, space, place and motion, these *measures are properly to be understood*,* and the expression will be unusual, and purely Mathematical, if the measured quantities themselves are meant. Upon which account they do strain the Sacred Writings, who there interpret these words for the measured quantities. Nor do those less defile the purity of Mathematical and Philosophical truths, who confound real quantities themselves with their relations and vulgar measures."

To sum up, the Second Law: [*In a given time*] force impressed is proportional to the change of momentum; and it has the same currency.

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* *i.e.* [*Numbers* of some arbitrary *unit*. R. de V.]

CHAPTER VII

THE NOTION OF FORCE (*continued*)

IN the last Chapter we examined " Force " from the point of view of the Definition and of Newton's Second Law of Motion. There is another view, which is not uncommonly put forward in text-books, and that is of Action and Reaction, illustrated by reference to Newton's Third Law of Motion.

This Law is stated as : " To every Action there is always an equal and contrary Reaction : or the mutual Actions of any two bodies are always equal and oppositely directed."

This Law appears very simple, as stated by Newton. The *explanation* of it, as usually offered in text-books, is not, however, so easy of comprehension ; and this I attribute to the confusion caused by considering a pressure, or a tension, as a " Force." There are great difficulties in accepting this use of the word. I remember, as a boy, being taught (as I should not have been taught, but as probably many boys are still taught) that " if anyone presses a stone with his finger, his finger is pressed *with the same force* in the opposite direction by the stone." (Thomson and Tait's *Elements of Natural Philosophy*.)

This I could, of course, believe, since I had been taught that equal and opposite forces would balance, or neutralize, one another. When, however, I was told that " a horse towing a boat on a canal is *dragged backwards by a force equal to that which he impresses on the towing rope forwards*," (Thomson and Tait), I simply could not understand the proposition ; nor do I understand it now.

If the horse and the boat were engaged in " a tug-of-war," and *their " forces " were equal*, the boat and the horse ought to remain immovable. Since, however, the horse pulled the boat along—and that is the assumption—*the force of the horse* must have been greater than *the force of the boat:* they did not *neutralize* one another ; therefore, the one was

stronger than the other. Either equal and opposite forces balanced one another, or they did not. If they did, then there was *no residual force* to cause the motion of the boat. As the boat moved the forces were *not equal* and opposite.

This is how I argued ; and the explanation of the difficulty given appeared to me exceedingly unsatisfactory.

Now, if the reader will turn to the *Principia* (Motte's translation), he will see that Newton *never mentions* the word " Force." What he said was that the finger *is also pressed* by the stone—" *tantundem*," *i.e.*, just as much. The expression, " with the same [an equal] *force*," is a gloss—unless, of course, it is supposed to be synonymous with " tantundem " ; in which case " Force " would be equivalent to " strength," or degree of intensity, and be a measure only.

No such explanation is possible in Thomson and Tait's " horse-boat " example, since the wording is " *by a force* equal to that which *he impresses* on the towing-rope." In this case there cannot be much doubt that the tension of the rope is referred to as a " force."

Let us see what Newton (according to Motte) actually said in reference to this. " If a horse draws a stone, tied to a rope, the horse (*if I may so say*) will be equally drawn back towards the stone ; for the *distended rope*, by the same endeavour *to relax or unbend itself*, will draw the horse as much towards the stone, as it draws the stone towards the horse."

The bracketed words, " if I may so say," are usually omitted by authors who quote from this paragraph of the *Principia*. They are, however, exceedingly important, since they would appear to indicate that Newton was here at a loss for a suitable phrase, and so made use of a metaphor. His meaning would appear to be that the horse is only " metaphorically " drawn back.

We see that Newton, here also, never mentions the word " Force." The horse is not *drawn back* by a force ; it is only " as it were " drawn back *by the rope*. In other words, the condition we now call " Stress," but which Newton calls " Action," impressed on the rope, is equal in both directions. There is no " momentum " generated in

the stone ; if the horse stops pulling, the stone will not move along " with uniform motion in a straight line " ; no, the stone *stops*. Since there is no momentum " changed " or generated, it is clear, by Law II, that there is *no Force*.

Glazebrook does not refer specially to this Law III in his *Dynamics ;* but Cox devotes several pages to it. He quotes the first half of the Law, and then continues : " Pressure and counter-pressure, action and counter-action are equal. All force is of the *nature of stress*, that is, a mutual action *between two bodies*, the same [exactly equal] from whichever side it is looked at . . . a cannon-ball can *exert no Force* till it meets with an obstacle, and then only *so great a Force* as that with which it is resisted. You cannot pull an object *harder than it pulls back.*"

This commentary I analyse as follows :—

(1) It does not say, explicitly, that any stress *is a Force*, though it strongly suggests it. Force is of " the nature " of a stress ; or, as some authors put it, it is " half a stress " —each end of the stress being a force.

(2) It is clear that " force," according to Cox, is not *a thing*, since you cannot *exert a thing*.

(3) You cannot *pull an object* harder than it pulls back.

This (3) is further emphasized later by the statement that, " The cart [Newton's stone is changed to a cart] pulls the horse backwards *as hard as the horse pulls the cart.*"

Two pages later, however, we read : " Now provided the tension T is greater than the resistance R, there will be a *balance of force* T − R forward, and the cart will begin to move, for it will be *subject to an acceleration forwards.*"

Here the tension is *explicitly* stated to be *an accelerating force ;* and apparently, for some unexplained reason, the horse *now pulls the cart harder than the cart pulls the horse !* This contradicts (3).

Later again Cox says the tension is " *no longer an external force*, but a mere internal reaction." Complications are multiplying !

Eventually the impression left on the reader's mind is that it must be the earth which *thrusts* both the horse and the cart forwards. In such case, why use a horse ?

Of course, this is not what the author intends to convey ;

but the explanation is far from clear, and that is *the sort* of impression which the description does convey.

The question is not really one of Dynamics at all; it is a question of Energetics. Even when the cart travels uniformly, there is no "momentum" or "free motion" of any kind. The Action and the Reaction always balance one another. There is no *Dynamical* problem in the case at all.

The explanation of this "motion" is very simple. The horse, transforming the potential energy of his food, *pushes* against his collar—thus causing a *tension* on the traces. If a dynamometer be fixed between the trace and the collar and another similar dynamometer between the trace and the stone, these two dynamometers will register *equal tensions*. This is, of course, the whole of the Third Law in a nut-shell.

Now, supposing that the horse increases the pressure on the collar—which, at first, was not sufficient to move the stone—to, say, 150 lbs, we may suppose the stone to now move along. If the horse travels three feet per second, he will be "*doing work*" on the stone, measured by $150 \times 3 = 450$ *foot-pounds per second*. (This is a fair amount for an ordinary horse to do, though the *theoretical* "horse-power" is 550 *foot-pounds per second*). During the whole of this time the dynamometers will *continue to register* tensions of 150 lbs.

The consideration of "Force" does not enter into the problem. Similarly, questions of heat, friction, etc., do not enter into the science of *Pure Dynamics*. In this problem, motion is *destroyed;* while pure Dynamics assumes that "momentum" is "indestructible."

Of course, the horse cannot do this "work" unless he can get a "purchase" on the earth. As Mr. Cox puts it, "No man can raise himself by pulling at his own boot-straps." Without some "fulcrum" on the earth, or some other body *outside himself*, the horse could not possibly move the cart at all; nor even himself.

It is well to note this, since "popular" so-called scientific books not uncommonly state that matter has inertia, *because matter cannot move itself*. Indeed, one of my text-books says: "A lump of dead matter at rest will not move

itself." This statement is only true under *very restricted conditions;* an example will be given later.

The horse, in the case just quoted, cannot, I repeat, move the " horse-cart " system, unless he can get a " purchase " on some body *outside the system.* Similarly, a body (a lump of iron, say) cannot move itself unless it, also, can get a " purchase " on some body *outside itself.* A body, however, which can get no purchase on another body, is a thing quite beyond our observation; and is, indeed, probably only imaginable by a metaphysician. Since the time of Newton, we say that *all bodies* attract one another—and so *can* get a " purchase " on one another.

INERTIA

Newton's First Law of Motion is: " Every body continues in its state of rest, or of uniform motion in a straight line, except in so far as it is compelled to change that state by impressed force."

This is commonly referred to as the Law of Inertia; and it is one of the fundamental assumptions underlying the science of *Dynamics.*

Referring to this Law, Cox says: " This is merely Galileo's Principle of Inertia, by which is meant that a body has no power in itself of altering its own state of motion. . . . It is distinctly contrary to the views held in his time, and *by unobservant people to this day.*"

This is another of those cases where a Law, as stated by Newton, appears clear and simple; but where the " explanation " of it, offered in a text-book, renders the Law extremely vague; whilst the commentary above offered is, certainly *generally,* untrue. " Bodies," in *Pure Dynamics,* are *assumed*—for their properties, or want of properties, are *arbitrarily assumed*—to have no power of moving themselves. In this science, motion can neither be *created nor destroyed:* hence it is purely superfluous to say that *a body cannot create motion.* To say, however, that what is true for bodies *in Dynamics* is equally trne for *real bodies* is rather " confusing "—to express myself mildly.

Newton's own commentary on his First Law might be

summed up briefly, as: "Bodies, in motion, *cannot retard or accelerate themselves.*" He suggests that planets and comets are "resisted"—and that consequently their motion is being retarded.

At the risk of being called an "unobservant person" I must point out that a vast number of *bodies* actually *do move themselves.* I suppose that one may call locomotives and motor-cars "bodies"; they certainly *move themselves.* Even very small children know that bodies *move themselves,* since they are well acquainted with clockwork mice, and even small wooden "jumping frogs." On any Guy Fawkes' day one can see very many rockets, and other fireworks *moving themselves.*

In short, if a body has got *energy* in it—and can we say that *any body* has *no energy in it?*—it certainly *can,* under suitable conditions, *move itself.*

"Inert" has two dictionary meanings: (1) destitute of the power of *moving itself,* and (2) "inactive," *i.e.,* incapable of acting, or "accelerating," say, any other body.

No body is "inert" in the second sense, since all bodies, as far as we know, *gravitate.*

As regards the first sense, a body may be said, generally, to be "destitute of the power of moving itself," but only on the condition that *there is no other body.* In other words, it cannot *alone*—and without the assistance of other bodies—move itself. In such a case, however, any "change in its position" would be unobservable; and, therefore, such an expression in Physics is meaningless. In *no case,* however, could a body *move itself unless there is energy in it!*

In the Third Law we have seen that the word "Force" is not used by Newton at all; but we have a clear idea of what he means by "action," which is the same as what we call "stress." This word (which we owe to Rankine) is the one invariably employed by engineers. It has a very clear meaning and it is *univocal,* whereas "force" is distinctly *equi*-vocal. If we have, therefore, two names, (1) "force" and (2) "stress," for this action—one a very good one, whilst the other is ambiguous—is it unreasonable to suggest that the word "force" (*in this sense*) be dropped once for all? I dislike violent changes; but this suggestion really involves *no*

change. Stress is retained, as a very good and intelligible word, whilst the use of "force" (*in this sense*) is simply avoided—as being confusing.

If this view is accepted, Newton's First Law of Motion would read: "Every body perseveres in its state of rest, or of uniform motion in a straight line, unless it is compelled to change that state by an impressed *stress*."

Only one word has been changed, and since "force" (in the sense of *vis impressa*) is synonymous with "stress," I may say that *no alteration has been made*—nor was any intended—*in the sense*. The word "impressed" must be taken to mean that *one end of the stress* must be acting on some body which is *outside* of "the system." The stress may be started either *inside* or *outside* of the system.

The adoption of this suggestion would add, very materially to the peace of mind of the young student, who certainly has all my sympathy in this connexion.

It is not proposed that the word "Force" should be dropped (as has, indeed, been suggested more than once), but that its use should be restricted to the very special and technical sense of *Time-rate of mass-acceleration*, in *Dynamics*; whilst its sense in *Energetics* would be *Space-rate of transformation of energy*. To, in fact, *the intensity* of an *action* or *stress*.

Mr. Bertrand Russell (*A.B.C. of Relativity*, page 12), says: "'Force' was known to be merely a *Mathematical fiction*." By this, I presume that he means that there is no such *thing* as force—that it is purely subjective—and he devotes the whole of Chapter XIII to *The Abolition of "Force."* Force is, of course, no more objective than the "bank rate," say; which may be familiarly defined as the measure of the *intensity* of the squeeze of the Bank of England on the Money Market. As I understand Mr. Bertrand Russell, his objection—like that advanced here—is to the use of "force" as a *cause of motion*.*

At this point, the cautious reader may object that a "stress" is not an "action" in the ordinary sense of the word; action usually connoting motion whilst stress does not.

* Notwithstanding all this, Mr. Russell uses the word "force" fairly freely—and in the sense that he objects to. For example, we find it four times on page 139, and twice on page 140—which, for a Purist, does not look well.

This is quite true ; but the question is not what we *now* mean by the word " action," but *what Newton meant by it.* About this there can be no reasonable doubt, since he gives two separate examples in illustration of the meaning he attaches to the word. In the first example this action is a *pressure,* whilst in the second it is a *tension*—both stresses.

It would appear that in Newton's time the word " action " had a wider meaning than it has at present. We find in Descartes' Works, Tom. II, page 204, 1897 Edition, the following statement : " Besides, it is to be remarked that the signification of the word *action* is general, and includes not only the power or inclination to move, but also the movement itself."*

Newton seems to have used it in the first sense ; whilst we now employ it rather in the last.

The view of the meaning of " action " put forward here gives one side of the question. There is another, however, advanced by Sir R. Glazebrook, which must be examined.

In his *Dynamics,* he gives a *résumé* of Newton's explanations and illustrations ; after which he makes the rather extraordinary statements : " These illustrations of Newton make it clear that *the action and reaction contemplated by him* was the *interchange of momentum* between the bodies . . . in a Scholium attached to the Law, he interprets the terms *in another sense.* . . .

" If then, we are to mean by *action, gain of momentum,* the experiments on impact . . . afford a verification of the Law.

" 102. *Conservation of Momentum.* In all these cases their *action is transference of momentum.*"

I consider these statements, *in explanation* of the Third Law, tend only to confuse what appears a very simple question ; and for the following reasons :—

It is certainly not " clear " (to me, at least) that Newton meant Action and Reaction to mean *the interchange of momentum.* I should say exactly the reverse. The word " momentum " does *not* occur in the Law ; and Newton

* Outre qu'il faut remarquer que la signification du mot *action* est générale, et comprend non seulement la puissance ou l'inclination à se mouvoir, mais aussi le mouvement même.

appears to have specially selected examples, in illustration, where there *was no momentum.*

In the first example, the " action " is caused by the finger, and the reaction by the stone. When the finger *presses* the stone, there is no " momentum " in the finger or the stone—either before, or after, the pressure. How, then, is the non-existent momentum *transferred ?*

In Newton's second example, the horse draws a stone. There is no " momentum " in the stone, which moves by a series of small jerks and so has no " velocity," in the ordinary sense of the term, *i.e.*, no " *free* motion."

The horse, likewise, has no " momentum," for he is pushing himself forward by a series of irregular and jerky movements. Newton's own " (if I may so say) "—omitted by Glazebrook—appears to show clearly what he meant.

If, however, Glazebrook relies on Newton's references to impact, let us turn to the *Principia,* where we read (Law III) :

" If a body impinge upon another, and by its force change the motion of the other, that body also (*because of the equality of the mutual pressure*)—[equality of action and reaction]—will *undergo an equal change in its motion* [momentum]. . . . The changes *made by these actions* [pressures] are equal, not in the velocities, but in the motions [momenta] of the bodies."

Also, Corollary III :

" For action and its opposite reaction are equal, by Law III, and therefore, by Law II, they *produce in the motions* [momenta] *equal changes* towards opposite parts."

We see no kind of evidence here that what Newton calls " action," is either " gain," " interchange," or " transference " of momentum. The action *changes the momentum,* but it is not itself *the change of momentum.*

The meaning of *action* in the *Principia* appears to correspond, as I have said before, with what Rankine calls " stress."

It would appear, therefore, that the statement that " action " (in the *Principia*) is *gain,* or change, *of momentum* is not justified. If we accepted this definition, and substituted the one for the other in Definition IV, it would read : " An impressed force is a *gain of momentum exerted upon a body* in order to change its state."

This does not read well. It is not clear how a "gain of momentum" could be *exerted* (Glazebrook's word is "exercised," or "acts") upon a body. If we substitute the word "stress," for action, the Definition is quite simple and comprehensible.

On the next page (p. 153) we read something *quite different: "Action* then, in the Third Law, may be measured by the *product of a force and the displacement of the point at which it is applied."*

It is very curious how loose this distinguished author is in his use of language. He gives most excellent definitions of "velocity" and of "speed"; he even carefully points out the essential *difference* between these words, and yet he employs them indiscriminately, as if they were synonymous expressions. He gives an excellent definition of "force," and then he uses the word in a variety of senses. For example: "We call *the rate* at which this transference takes place *force,* and when the transference is *going on* we say *force acts* between the two bodies."

Since a "three feet per second," say, could not *act* between bodies, it is clear that the second word "force" means something different to the first word "force."

In Chapter VIII he is continually equating momentum and force—which are *vectors*—with energy and work, which are *scalars.* Such equations as

$$\text{work} = \text{force} \times \text{space [? distance], or}$$
$$\text{force} \times \text{space} = \text{kinetic energy}$$

are *non-homogeneous* and, therefore, meaningless.

Glazebrook's "Definition of Work" (p. 154), is about as complicated as possible for the unfortunate young student. Its want of homogeneity is obvious, since he transforms "momentum" (a vector) into "work" (a scalar).

ACTION AND REACTION

It is a very common, but reprehensible, practice to refer to *all* mutual actions between two bodies as *action and reaction.* This reference is, of course, correct in the case of pressure and tension (as in Newton's illustrations); but it

must be observed that *Reaction*, in its proper sense, is a *counter-action*, which has been *provoked by another action*.

This is very clearly pointed out by Father Bayma, in his *Molecular Mechanics*, 1866, where he says:

"The words *action* and *reaction* cannot be used in the case of *mutual actions*, through which *the structure of the bodies is not shaken*, and of which the one is *not provoked by the other*. Thus the action of the sun upon the earth, and the action of the earth upon the sun, are *not action and reaction*; for the earth acts upon the sun not because the *sun provokes its action*, but because the earth possesses active powers of its own; and, in the same manner, the sun acts upon the earth, not because the *earth provokes such action*, but because the sun itself is endowed with activity and is determined to act. Therefore, the sun and the earth do not *act* and *react* on each other, but simply *act*. The idea of reaction implies in the reacting body an exertion of power which has been *awakened by violence*, viz., by another exertion *causing a disturbance in the body*, and putting it in an *unnatural state* from which it *strives to recover*. Now this occurs only when *traction* or *pressure* or any other analogous exertion of power *constrains the molecules* of a body to alter their *natural size* or their *distance* of relative equilibrium: this is the state of things by which reaction is *called into existence*, and kept up until by it the molecules reduce themselves again to their normal size and distance.

"Those physicists who, in speaking of the mutual action of the earth on a stone, and of the stone on the earth, use the words *action and reaction*, confound the *absolute principle* of the equality of action and reaction in the *impact of bodies* with another principle, *which is less absolute*, viz., that the *quantity of action* of a body A on another body B is equal to the quantity of action of the body B on the body A. These two principles, we say, *must not be confounded*. In the second, in fact, the question is not *action and reaction*, but mutual action only: and moreover, whilst the first principle is *absolute and general*, the second is true only *with a restriction*, viz., when the bodies A and B are *of the same nature*. Many a Physicist has overlooked *the necessity of this restriction*, from *assuming* (of course, without proof),

that the particles of matter, at least at non-molecular distances, were all *equally attractive.* This, I repeat, they assumed, for there is no fact and no argument in scientific reasoning that can prove such an assertion. . . .

"And consequently we cannot admit the Principle of *Equality of mutual actions* with regard to *substances of different species.* While, on the other hand, *we must admit* the *equality of action and reaction* for all ponderable bodies, however heterogeneous."

This question cannot be pursued further at present; but the reader who is interested in the subject will find it very interestingly developed, by Bayma, in *Molecular Mechanics.*

TRANSFERENCE OF MOMENTUM

There is an expression that one continually comes across in books on *Mechanics,* and that is the "*transference of momentum.*" It is used as if the momentum were like water, say, and could be "poured" from one jug into another—or "passed over the counter."

Newton always speaks of "*change* of momentum," "production of momentum," and such similar descriptions. Since "motion" is *a state,* and "momentum" is *a rate,* it would appear clear that these cannot be "transferred"—*i.e.,* in the ordinary sense of the word. You cannot "transfer" *three miles per hour.*

Bayma (*Molecular Mechanics*) in his Proposition IV, says:

"All increase or decrease of intensity in motion is always due to a *real production or extinction of velocity* . . . since motion cannot be communicated by means of *true and immediate contact* (Prop. III) the velocity of the body A will not pass into another body B, either totally or partially, unless it is possible for it to leave the body A, to which it belongs, and traverse an interval of space between A and B. Now this process is utterly impossible and absurd. Velocity [? motion] is *a mode of being,* and a mode of being *cannot leave the subject of which it is a mode* . . . consequently the velocity [? motion] of the body A cannot be *identically transmitted* to the body B. Therefore the velocity [? motion] acquired by the body B is not the velocity [? motion] *pre-*

existing in the body A, but a velocity [? motion] *really produced* by A acting on B."

For example, a billiard player can cause his own ball to strike the red ball, say, in such a manner that his own ball comes to rest, whilst the red ball travels at a speed nearly equal to that his own ball had before the impact. The motion of his own ball has been *extinguished*, whilst the motion of the other ball has been *actually* " generated "—or " produced," as Newton says.

The question of " impact," however, is really one of *Energetics*, and not of *Dynamics*, since the impact of *absolutely rigid bodies* is quite unthinkable. Such bodies could not *change their shape*, and so could generate *no reaction;* whilst *absolute contact*, combined with a change of motion, would involve *instantaneous acceleration!* This instantaneous acceleration would further involve, in my billiard ball example, that on the *instant of contact, both balls* would have speeds of, say, eight feet per second, as well as zero speed; *two speeds at the same time!*

I do not suppose, of course, that when Glazebrook uses the word " transferred," he really means *actual transference* in the Physical sense. The employment of words with " ragged edges," however, is confusing, and should be avoided. This practice is too common; for example, Tait, in *Newton's Laws of Motion*, says:

" Momentum is *transferred* (without change) from one body to another.

" Energy is *transferred* (without change of amount) from one body to another."

I do not understand what " momentum *without change* " means. The expression certainly suggests " Physical transfer "—*i.e.*, " passing over the counter."

Again, Stewart and Tait, in *The Unseen Universe*, 1876, say: " Momentum *cannot be produced* or destroyed *in any system as a whole.*" Yet Newton speaks of its being " produced."

The Principle of the Conservation of Momentum in Dynamics does not mean that momentum *cannot be extinguished*—that, in fact, *the same motion* is actually observed —but simply that the *amount* of the momentum *before the*

impact, say, is *exactly equal* to the amount of the momentum *after the impact*. In other words, that *on the balance*, no momentum *is deficient*.

When dealing, not with imaginary, but with *real bodies*, as we do in *Energetics*, the "Conservation of Momentum" is *never true*: the only cases where it *appears to be true* are when there is, in fact, *no momentum* to conserve. For example, if two billiard balls moving at equal speeds, but in opposite directions, strike one another centrally, there is *no momentum* either before or after impact. I should not use the word "conservation" for something which *does not exist!*

Energy also, like momentum, cannot be "transferred." It can only be "transformed"; and it does not appear to be recognized that it cannot be transformed into the *same kind of energy*. What I mean is, the energy of molar motion cannot be transformed *directly* into energy of molecular motion—or heat. It *must* be first transformed into some kind of *potential energy*, and then *re-transformed* into heat.

ACTION AT A DISTANCE

The reader may say that Bayma, in the statements I have quoted, refers to "action at a distance"; and that this, he has been taught, is impossible—some people even say that it is "quite inconceivable."

This was not the opinion of Clerk Maxwell, since at the Royal Institution, lecturing on *Action at a Distance*, February, 1873, he very sarcastically said: "If we are ever to discover the laws of nature, we must do so by obtaining *the most accurate acquaintance* with *the facts of nature*, and not by *dressing up in philosophical language* the loose opinions of men who had no knowledge of the facts which throw most light on these laws. And as for those who introduce etherial, or other media, *to account for these actions*, without any direct evidence of the existence of such media, or any clear understanding of *how the media do their work*, and who fill all space three or four times over with ethers of different sorts, why, *the less these men talk about their philosophical scruples about admitting action at a distance the better.*"

And as Bayma very forcibly put it: if action at a distance is *difficult to explain*, action by *real physical contact admits of no possible explanation!* It only, in fact, leads to all kinds of absurdities.

Bertrand Russell (*loc. cit.*, p. 12), says: "As a matter of fact, the two billiard balls *never touch at all.*" Also, page 197: "No two particles of matter *ever come in contact;* when they get too close, they both move off. If a man were had up for knocking another man down, he would be *scientifically correct* in pleading that he had *never touched* him. What happened was that there was a hill in space-time, in the region of the other man's nose, and *it fell down hill.*" The space "un-Euclidified" itself, in fact.

As a matter of fact, we have no sort of evidence that bodies *ever do touch one another;* whilst there is a good deal of evidence that *they do not*—in other words, that which we call "contact," is, in reality, only *virtual contact.*

Thus, as is very well known, if two curved plates of glass be strongly *pressed together* by suitable means, the distance that they are apart can *actually be measured.* Even if the pressure be so great as to produce a "black spot"—when the measurement fails—this is no evidence of *absolute contact,* but only that the distance that the plates are apart is *less than the smallest wave-length of light that we can see.*

There is still another way of looking at "Force." W. W. Rouse Ball (*Mathematical Recreations*), 1911, says:

"The First Law of Motion is often said to define *force,* but it is only in a qualified sense that this is true. Probably the meaning of the Law is best expressed in Clifford's phrase, that force is 'the description of a certain kind of motion'—in other words it is not an entity, but merely a convenient way of stating, without circumlocution, that a certain kind of motion is observed.

"It is not difficult to show that any other interpretation lands us in difficulties. Thus, some authors use the Law to justify a definition that force is that which *moves a body,* or changes its motion; yet the same writers speak of a steam-engine moving a train. It would seem then, that according to them, a steam-engine is a force. That such

statements are current may be fairly reckoned among Mechanical paradoxes."

I rather pity the poor student who tries to get a clear idea of force from this definition of Rouse Ball. I do not think that he will consider it all "Mathematical recreation." Rouse Ball, however, says that *any other interpretation* lands one in difficulties; but he mentions only *one* other interpretation—that which is so generally *taught*—viz., that force is the *cause* of motion. If "Mechanical paradoxes" occur, is it not rather the fault of the Professors who teach this *meaningless* definition? As Karl Pearson (*Grammar of Science*) puts it:

We know nothing about "effective causes"; what we call in Physics a "cause," means only a "Physical cause," which is a very different thing. "Cause is scientifically used to denote an antecedent stage in a routine of perception. *In this sense*, force, as *a cause*, is meaningless. 'First cause' is only a limit, permanent or temporary, to knowledge."

J. B. Stallo (*Concepts of Modern Physics*, 1882), says:

"Force is *not an individual thing* or entity that presents itself directly to observation or to thought; it is *purely an incident to the conception of the interdependence of moving masses*. . . . It is of the greatest moment, in all speculations concerning the interdependence of Physical phenomena, never to lose sight of the fact that force is a *purely conceptual term*, and that it is *not a distinct or tangible thing*.

"When, however, we speak of a 'force of nature,' we use the word force in a sense very different from that which it bears in Mechanics. A 'force of nature,' is a survival of ontological speculation; in common phraseology the term stands for *a distinct and real entity*."

We may, therefore, I trust, now agree that force (in Mechanics) is *never* to be conceived as being *a cause of anything*—such an expression being *meaningless*.

IMPRESSED FORCE

We have seen that what is called "impressed force" (*vis impressa*) was defined, by Newton, as being *an action*, or *in an action;* and that what he meant by "action" was what we should now call "stress."

A stress is, as we know, " two-ended." I define, therefore, the word " impressed " to mean that *one end of the stress* must be *outside of the " system "*—whether the stress is *generated inside or outside of the system.*

" Force " is, as I have said, more than once, *the measure of the intensity of this stress.* As Bayma puts it: " That which Physicists call Force is, strictly, nothing else than *the intensity of the action,* measured by the intensity or the quantity of motion which in the given circumstances *it is capable of communicating.*

The " Force is not the action, but *the intensity of the action; and is measured by its dynamical effect, etc.*"

Or, as Tait puts it, *Recent Advances, etc.:* [The] " Force is the *rate* at which *an agent* changes momentum per unit of time."

I would suggest that the word force should always be referred to, with the definite article, as THE force, rather than as A force; it would then be more easily conceived as being THE force (or THE intensity) of the action—just as we say " *The force* of an argument," or " *The force* of a blow."

I am afraid that the very young student—if he were perfectly candid—would define " forces " as:

> " Forces are little pointed arrows with which you poke rigid bodies about. They are also very convenient for making parallelograms with."

WEIGHT AS FORCE

Weight is generally recognized as force. How can we reconcile this with our definition; since, when a weight is at rest, *there is no mass-acceleration?*

The difficulty is not a serious one. In the same way that we divide " energy " into *kinetic* and potential, we must acknowledge that " force " can also be " potential mass-acceleration," " potential rate of change of momentum; " the measure of the intensity of the action *which is capable of* changing a certain quantity of momentum *in a given time.*

The idea of viewing weight as *potential force* is not exactly new, for Giuseppe Venturoli (Professor of Applied

Mechanics, in Bologna), said, in 1833, in his *Elementi di Meccanica e d'Idraulica*, that, " The motion which a balanced force *tends to produce* is called ' virtual.' "

As we should now say, the rate of change of momentum may then be called " virtual "—or, to use a better term, it is " potential."

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CHAPTER VIII

ENERGETICS

" WHEN we pass from *Abstract Dynamics* to Physics—from material systems whose only properties are those *expressed by their definitions*, to *real bodies*, whose properties we have to investigate—we find that there are many phenomena which *we are not able to explain* as changes in the configuration and motion of a material system." (Clerk Maxwell.)

This is a frank and straightforward statement, made by a great thinker, when he was at the summit of his intellectual career. It consequently gives us cause for deep reflection.

A. E. H. Love, *Theoretical Mechanics*, also says : " Some part of the system of postulates on which we base our Rational Mechanics [? Dynamics] though *valid in logic is not a true representation of facts*, and it is desirable to endeavour to reconcile the opposing hypotheses by giving up something *not really essential*, but actually *treated as fundamental*, when the general problems of Physics are approached from one side or the other."

As was pointed out previously, Mechanics divides naturally into two separate branches : pure *Dynamics* and *Energetics*. When we pass from Abstract Dynamics to Concrete Physics, we may be said to pass from the poetry to the prose of Mechanics. In Dynamics, we dealt with *rigid bodies ;* but no real (actual) bodies are rigid. In Dynamics, we postulate the Principle of the Conservation of Momentum ; but in dealing with real bodies, in consequence of their defect of rigidity, this Principle is *never true*, though there are cases where the error is inappreciable. These postulates are, as Love says, " valid in logic, but not a true representation of facts." These postulates must, therefore, be abandoned. We must, in fact, pass from a *deductive* to an *inductive* science.

Dynamics is the " science of momentum," as Energetics

is the " science of energy." Dynamics is a purely *theoretical science:* real bodies do not act, nor, indeed, *could they act,* as rigid bodies are assumed to do in this science. Dynamics can take no account of viscosity or elasticity, which are essential properties of all real bodies. When, however, bodies act upon one another *without altering their shapes*—as is, practically, the case with bodies acting at astronomical distances—Dynamics will give solutions, the accuracy of which is extraordinary. It must be remembered, however, that these solutions are, in reality, only *approximately correct.* We may not be able to measure the *very small* divergence from theory, yet some very small error exists *in every case.*

Although Dynamics and Energetics are essentially different, they are only separate branches of Mechanics. They bear to one another a relation analogous to that between Plane and Spherical Trigonometry. When one does not distinguish between these two branches of Mechanics, when one blindly transfers to Energetics the theorems demonstrated for Dynamics, one commits an error similar to that of a Geometer who should apply to spherical surfaces the theorems which are true for plane surfaces. Knowing, for example, that the three angles of any plane triangle are equal to two right angles—which the Geometer might perhaps call the " Principle of Triangularity "—he might rashly assume that this " Principle " was equally true for spherical triangles. If an experimenter measured up these triangles and found a constant discrepancy, our Geometer might hold that this was due to " experimental error " ; or, if this were not probable, he might refer to the result as a " paradox," and, as is commonly done, leave it at that.

Now, to quote Maxwell again: " Even when the phenomena we are studying have not been explained dynamically, we are still able to make use of the Principle of the Conservation of Energy as a guide to our researches."

The progress of Physical Science has led to the discovery and investigation of many different forms of energy, and thereby to the establishment of the doctrine that *all mundane systems* may be regarded as *conservative systems,* provided

that the different forms in which energy may exist in these systems be taken into account. This means that the *total energy* in any system remains a constant amount, provided always that the system be not acted upon by any other system or systems.

This doctrine, considered as a deduction from observation and experiment, can, of course, assert no more than that no instance of a *non-conservative system* has hitherto been discovered.

As Clerk Maxwell also says: "The doctrine of the Conservation of Energy is *the one generalized statement* which is found to be *consistent with fact*, and not in one Physical Science, *but in all.*

"When once apprehended it furnishes to the Physical enquirer a principle on which he may hang *every known law* relating to Physical actions, and by which he may be put in the way to discover the relations of such actions in new branches of science.

"For such reasons, the doctrine is commonly called the Principle of the Conservation of Energy."

Clerk Maxwell, however, having pointed out the direction of the road which will lead to such successes, does not carry us along it, as he undoubtedly could have done. The road is, however, fairly clear now. Possibly, Maxwell considered that since the field had been so ably surveyed by Rankine, in 1855, it was unnecessary for him to go over ground which that distinguished author had, as stated, already traversed.

On April 18th, 1855, Professor Rankine read his paper, *Outlines of the Science of Energetics*, published in the Proceedings of the Philosophical Society of Glasgow, for that year.

This paper, which was an extension and an improvement on another paper read to the same Society, on January 5th, 1853, appears to be very little known, for it is never quoted from in books on Mechanics. This is a great pity, as the paper is a very valuable one, and deserves to be studied *in extenso*. This neglect may possibly have been due to Rankine's notation being unusual, and his treatment of the subject very general. For example, it requires considerable mental effort to grasp the exact meaning of his numerous

terms, " active accident, passive accident, radical accident, complex accident, relative accident, absolute accident," etc., for though they were carefully defined, the terms were not taken up and are rarely found in books on Mechanics.

It will be appropriate to summarize the paper here.

Rankine commences by considering Energetics as an *inductive science,* and he points out that a Physical theory is built up by two stages. " The first stage consists in observing the relations of phenomena, whether of such as occur in the ordinary course of nature, or such as are artificially produced in experimental investigations, and in expressing the relations so observed by propositions called *formal laws.* The second stage consists in reducing the formal laws of an entire class of phenomena to the form of a science ; that is to say, in discovering the most simple system of principles, from which all the formal laws of the class of phenomena *can be deduced as consequences.*

" Such a system of principles and its consequences, methodically deduced, constitutes the *Physical theory* of a class of phenomena.

" A Physical theory, like an abstract science, consists of definitions and axioms as first principles, and of propositions, their consequences ; but with these differences :—first, That in an abstract science, a definition assigns a name to a class of notions derived originally from observation, but *not necessarily corresponding to any existing objects* of real phenomena, and an axiom states a mutual relation amongst such notions, or the names denoting them ; whilst in a Physical science a definition states properties common to a class of existing objects, or real phenomena, and a Physical axiom states a general law as to the relation of phenomena ; and, secondly, That in an abstract science, the propositions first discovered are the most simple ; whilst in a Physical theory, the propositions first discovered are, in general, numerous and complex, being formal laws, the immediate result of observation and experiment, from which the definitions and axioms are arrived at by a process of reasoning differing from that whereby one proposition is *deduced from another* in an abstract science, partly in being more complex and difficult, and partly in being to a certain

extent *tentative*, that is to say, involving the trial of conjectural principles, and their acceptance or rejection according as their consequences are found to agree or disagree with the formal laws deduced immediately from observation and experiment."

Rankine's fundamental equation appears to be:

$$W = \int_{x_0}^{x_1} X \, . \, dx$$

where x denotes the *passive accident*, say, for example, a distance; X an *effort*, or *active accident*, tending to vary x, and W stands for the *measure of the work* performed—or, what is the same thing, the amount of *energy transformed*—in increasing x from x_0 to x_1.

ENERGY AND WORK

Since the idea of Energy is the fundamental conception in this science, it may be well to pause here, and enquire what precisely do we mean by energy?

I have discussed this elsewhere* at some length, and have (I think) shown that we cannot give a complete and satisfactory definition of energy. I quoted from Prof. Soddy's *Matter and Energy*, that "this is the age of energy"; but I also pointed out that this author does not *define energy*. He says, "In Physics, work and energy are interchangeable terms"; but as, later on, he speaks of "the amount of work done, and the amount of energy spent in doing it," it is clear that we cannot interchange the position of the terms and speak of "the energy done" and the "amount of work spent on doing it."

M. Henri Poincaré, in *La Science et l'Hypothèse*, 1912, puts the matter thus: "Since we cannot give a general definition of 'energy,' the principle of the conservation of energy signifies simply that there is *something* which remains constant. Well, then, whatever may be the new ideas which future experiments may give us of the World, we are sure in advance, that there will be *something* which will remain constant, and which we shall be able to call Energy."

* *Motion of Liquids*, E. & F. N. Spon, 1914.

Probably the nearest approach to a satisfactory definition of energy—it is, certainly, a good *working definition*—is that given by Clerk Maxwell in his *Matter and Motion:*

" Work is the *act* of producing a change of configuration in a system *in opposition to a force which resists that change.*

" Energy is *the capacity of doing work.*

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" If, by the action of some agent *external to the system,* the configuration of the system is changed, while the forces of the system *resist this change of configuration,* the external agent is said to *do work on the system.* In this case *the energy of the system is increased* by the amount of work done on it by the external agent.

" If, on the contrary, the forces of the system produce a change of configuration which is *resisted by the external agent,* the system is said to do work on the external agent, and the energy of the system is diminished by the amount of work which it does."

Rankine's definition of energy is very similar to Maxwell's, while being, perhaps, a little more general. It is this:

" ENERGY, ACTUAL AND POTENTIAL

" The term ' energy ' comprehends every state of a substance which constitutes *a capacity for performing work.* Quantities of Energy are *measured* by the quantities of work which they constitute the means of performing.

" ' *Actual energy* ' comprehends those kinds of capacity of performing work which consist in particular states of each part of the substance, how small soever; that is, in an *absolute accident,* such as heat, light, electric current, *vis viva.* Actual energy is essentially positive. [That is to say, Energy is not a vector quantity. It is a " signless " quantity.]

" ' *Potential energy* ' comprehends those kinds of capacity for performing work which consist in relations between substances, or parts of substances; that is, *relative accidents.* To constitute potential energy, there must be a *passive accident capable of variation,* and an *Effort* tending *to produce such variation;* the integral of this Effort, with respect to the *possible variation* of the passive accident, is *potential energy,*

which differs from work in this—that in work the change *has been effected*, whilst in potential energy, it is *capable of being effected.*"

Referring again to the fundamental equation. Let x denote the accident, x_1 its actual value; X an Effort tending to vary it; x_0 the value to which the Effort tends to bring the accident; then

$\int_{x_1}^{x_0} X \, . \, dx = U$, expresses the potential energy.

To resume Rankine's argument:

"Two methods of framing a Physical theory may be distinguished, characterized chiefly by the manner in which the classes of phenomena are defined. They may be termed respectively the *abstractive* and the *hypothetical* methods.

"According to the *abstractive* method, a class of objects or phenomena is defined by describing, or otherwise making to be understood, and assigning a name or symbol to, that assemblage of properties which is common to all the objects or phenomena composing the class, as perceived by the senses, without introducing *anything hypothetical.*

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"The principles of the science of Mechanics, the only example yet existing of a complete Physical theory, are altogether formed from *the data of experience by the abstractive method.* The class of objects to which the science of Mechanics relates, viz., material bodies, are defined by means of those sensible properties which they all possess, viz., the property of occupying space, and that of resisting change of motion. The two classes of phenomena to which the science of mechanics relates are distinguished by two words, *motion* and *force;* motion being a word denoting that which is common to the fall of heavy bodies, the flow of streams, the tides, the winds, the vibrations of sonorous bodies, the revolutions of the stars, and generally to all phenomena involving change of the portions of space occupied by bodies; and *force*, a word denoting that which is common to the mutual attractions and repulsions of bodies, distant or near, and of the parts of bodies, the mutual pressure or stress of bodies in contact, and of the parts of bodies, the muscular

exertions of animals, and, generally, to all phenomena tending to produce or to prevent motion.

[In the above, "force" is employed by Rankine in the sense of Newton's "action"; it is the measure of *any action*, whether a *directed quantity*, or not.]

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"The fact that the theory of motions and motive forces is the only *complete physical theory*, has naturally led to the adoption of *mechanical hypotheses* in the theories of other branches of physics; that is to say, *hypothetical definitions*, in which the classes of phenomena are defined conjecturally, as being constituted of some kind of motion or motive force not obvious to the senses (called *molecular* motion or force), as when light and radiant heat are defined as consisting in molecular vibrations, thermometric heat in molecular vortices, and rigidity of solids in molecular attractions and repulsions.

* * * * * * *

"Of molecular hypotheses in particular, it is to be observed that their tendency is to combine all branches of physics into one system, by making the axioms of Mechanics the first principles of the laws of all phenomena; an object for the attainment of which an earnest wish was expressed by Newton (*Principia:* Preface).

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"Energy, or the capacity to effect changes, is the common characteristic of the various states of matter to which the several branches of physics relate; if, then, there be general laws respecting Energy, such laws must be applicable, *mutatis mutandis*, to *every branch of physics*, and must express a body of principles as to physical phenomena *in general*."

EFFORT OR "ACTIVE ACCIDENT"

The idea of "Effort" being a fundamental one in Rankine's Energetics, it is specially necessary to refer to his definition of it. He says:—

"The term 'Effort' will be applied to every cause which varies, or tends to vary, an accident. This term, therefore, comprehends not merely *forces* or *pressures*, to which it is

usually applied, but all causes of variation in the condition of substances.

" Efforts may be homogeneous or heterogeneous.

" Homogeneous Efforts are compared by balancing them against each other.

" An Effort, being a condition of the parts of a substance, or a relation between substances, is itself an accident, and may be distinguished as an *active accident.*"

PRESSURE

The word " pressure " is unfortunately used in Mechanics in two diverse senses, and this practice leads, at times, to considerable confusion. It is ordinarily used in Newton's sense of " Action," as previously referred to. It is in this sense that the student is mis-taught that pressure is *a kind of force*—which it is not, since it lacks the vectorial property. Employed in this sense, the dimensions of pressure are $[M]^1 [l]^1 [t]^{-2}$.

At the present day, however, the word pressure is beginning to be applied to what is, really, pressure *per unit area;* that is to denote not the stress, but the measure of *the intensity* of the stress. In this sense, the dimensions are no longer $[Mlt^{-2}]$, but $[Ml^{-1}t^{-2}]$, or $[M]^1 [l]^{-1} [t]^{-2}$.

To give an illustration of what I mean : an engineer might simply say that the *pressure* on a column is to be ten tons ; but he might so suitably design his column that the *intensity* of the pressure would not exceed 100 lbs. *per square inch.*

Carl Hering, *Conversion Tables*, 1914, gives *the derivation* of " Pressure, or intensity of Stress," as *Force ÷ surface.* Here again, since Force is a vector quantity, whilst Pressure is a scalar, one cannot equate them : and any equation expressing this is not homogeneous. Besides, as I have already pointed out in *Resistance of Air*, 1917, there are occasions when one would also be equating a *constant and a variable:* I have already quoted, in illustration, the two cases of the " Hydrostatic Paradox," and " Pascal's Paradox."

We see that when Rankine speaks of a " pressure " as being an " Effort," what he means is the *action* whose dimensions are Mlt^{-2}.

To quote Rankine, again :—" Work is *the variation of the accident by an Effort,* and is a term comprehending all phenomena in which *physical change* takes place. Quantity of work is measured by *the product of the variation of the passive accident by the magnitude of the Effort,* when this is constant ; or by the integral of the Effort, with respect to the passive accident, when the Effort is variable."

This is expressed in the general equation

$$W = \int_{x_0}^{x_1} X \, . \, dx \, ; \text{ or } W = X \int_{x_0}^{x_1} dx.$$

We see, therefore, that " Effort " may be measured by the length-rate, or distance-rate, of the transformation of energy. Or, put in Rankine's own words :—" *Each Effort is equal to the rate of variation of the potential energy with respect to the independent accident which that Effort tends to vary ;* or, symbolically,

$$\text{" (2).} \quad X = \frac{dU}{dx} \text{."}$$

In a similar manner—Effort being here defined in terms of Energy—we may conversely define Energy, in terms of Effort, as the *length-integral, or distance-integral, of the Effort.*

Rankine, having defined his terms, next states his axioms.

FIRST AXIOM

" *All kinds of Work and Energy are homogeneous.*

" This axiom means, that *any kind of Energy* may be made the means of *performing any kind of Work.* It is a fact arrived at by induction *from experiment and observation.*

" This axiom leads, in many respects, to the same consequences with the hypothesis that all those kinds of Energy which are not sensibly the results of motion and motive force are the results of *occult modifications* of motion and motive force.

" But the axiom differs from the hypothesis in this, that the axiom is simply the *generalized allegation of the facts proved by experience,* whilst the hypothesis involves conjectures as to objects and phenomena which *can never be subjected to observation.*

"It is the truth of this axiom which *renders a science of Energetics possible.*

". . . to *transform Energy* means to employ Energy *depending on accidents of one kind,* in putting a substance into a state of Energy *depending on accidents of another kind;* and to *transfer Energy* means to employ the Energy of one substance in putting another substance into a state of Energy, both of which are kinds of work, and may, according to the axiom, be performed by means of *any kind of Energy.*"

SECOND AXIOM

"The *total Energy of a system* cannot be altered by the *mutual action of its parts.*

"Of the truth of this axiom there can be no doubt; but some difference of opinion may exist as to the evidence on which it rests. There is ample experimental evidence from which it might be proved; but, independently of such evidence, there is the argument that the law expressed by this axiom is essential to the *stability of the universe,* such as it exists."

THIRD AXIOM

"The Effort to perform work of any given kind, caused by a given quantity of actual Energy, is the sum of the Efforts caused by the parts of that quantity.

"This axiom appears to be a consequence of the definition of Actual Energy, as a capacity for performing work possessed by each part of a substance *independently of its relations to other parts,* rather than an independent proposition.

"Its applicability to natural phenomena arises from the fact that there are states of substances corresponding to the definition of Actual Energy.

"The mode of applying this third axiom is as follows:—

"Let a homogeneous substance possess a quantity Q, of a particular kind of actual Energy, uniformly distributed, and let it be required to determine the amount of the Effort arising from the Actual Energy, which tends to perform a particular kind of Work, W, by the variation of a particular passive Accident x.

"The total effort to perform this kind of Work is represented by the passive Accident, viz. :—

$$X = \frac{dW}{dx}.$$

"Divide the quantity of Actual Energy Q into an indefinite number of indefinitely small parts δQ; the portion of the Effort X due to each of those parts will be

$$\delta Q \cdot \frac{dX}{dQ}$$

and adding these partial efforts together, the effort caused by the whole quantity of actual energy will be

$$Q \cdot \frac{dX}{dQ} = Q \cdot \frac{d^2W}{dQ \cdot dx}.$$

* * * * * * *

"The effort to augment a given accident x caused by actual energy of a given kind, Q, may also be called the '*Rate of Transformation*' of a given kind of actual energy with *increase of the given accident.*"

The foregoing gives a very general, and necessarily somewhat curtailed, view of Rankine's ideas on Energetics; his work should, however, be studied in the original paper.

The reader may have observed that in these quotations Rankine makes no reference to *time*. His *passive accident* is always a length, or a distance, and in no case *a time*. It is true, however, that at the end of the paper Rankine says: "The general relations between energy and time must form *an important branch of the science of energetics*"; but he does not pursue the subject further, which seems unfortunate. Let us consider it :—

POWER

Just as, in Dynamics, *Force* is the *time-rate of change of momentum*, so, in Energetics, *Power* is the *time-rate of the transformation of energy*. The parallel is a somewhat close one; though the dimensions of *Force* are $[MST^{-2}]$, whilst those of *power* are $[ML^2T^{-3}]$.

Rankine does not mention the word " Power "—which has a very clean-cut meaning—but had he done so, he would have classed it as an *active accident*, which, combined with the *passive accident*, time, measures *the quantity of work*. In Rankine's own language—but changing one word—" The quantity of work is measured by the product of the passive accident [time] by the magnitude of the *Power*, when this is constant ; or by the integral of the *Power*, with respect to the passive accident [time] when the *Power is variable*."

Or, put into mathematical language,

$$W = \int_{t_0}^{t_1} P \, . \, dt.$$

where t is the *passive accident*, and P the *Power*, or *active accident*, tending to vary t.

I may say there is a long and interesting discussion of " Power " in M. Gandillot's pamphlet, *Note sur une illusion de Relativité*, Gauthier-Villars, 1913, to which the reader is referred.

As some of the foregoing ideas may be unfamiliar to the reader, I have abstracted them, and put them into a tabular form (p. 110) ; Dynamics and Energetics are there contrasted in parallel columns.

HISTORICAL NOTE

Although Rankine, as I have already said, distinctly formulated the science of Energetics as far back as 1855, this branch of Mechanics does not appear to have made much headway in England : one might indeed say that it is practically unknown. In France, however, it was taken up vigorously by the late Prof. Pierre Duhem, who published valuable work on the subject in his *Traité d'Energétique*, 1911. In this he shows that the idea of the " Indestructibility of Energy " is by no means as modern as most people think. He points out that as long ago as in the thirteenth century there lived a certain Jordanus de Nemore, who says he was born at Nemi in Italy. Jordanus and his followers definitely stated that " that which can raise a certain weight

to a certain height can also raise a weight n times greater to a height n times less."

Duhem also points out that an Italian historian, M. Vailati, " has also found the first origin of the method of virtual displacements in Hero of Alexandria, who lived in the time of the astronomer Ptolemy," and further, " In the last years of the Greek philosophy, an Alexandrian Christian, John Philipon, disputed the validity of the peripatetic doctrine of the movement of projectiles ; the arrow he maintained continued its motion, without any moving power being applied to it, because the string of the bow had engendered in it a *kinetic energy* (that is the equivalent of the term that Philipon employed), which played the part of the moving power [*vertu motrice*].

" About the middle of the fourteenth century, a master of genius, Jean Buridon, took up the idea of Philipon. The energy communicated to the projectile he called the impetus, and with the theory of impetus *he forms the foundation of a new mechanics.*"

It appears certain that Lionardo da Vinci was cognizant of and in agreement with the views of these schools of the thirteenth and fourteenth centuries.

MECHANICS

VECTORIAL

Pure Dynamics

The Science of *Momentum* (MV)

A Deductive science

All dimensions are in space, mass and time. All equations are *Vectorial*.

Calling F the measure of the "force-stress" and dQ an element of the momentum changed—*i.e.*, generated or destroyed—in a differential of time, then,

$$F dt = dQ = d(MV) = M dV,$$

Vectorial equation, where V = *velocity*.

$$\therefore F = dQ/dT,$$

and "dimensionally,"

$$[M]^1[S]^1[T]^{-2} \times [T]^1 = [M]^1[S]^1[T]^{-1}.$$

Calling Θ the measures of the *vis viva*, in the direction of the motion, then

$$2\ F.dS = d\Theta = d(MV^2) = 2\ MV.dV.$$

All "momenta" are added *geometrically*.

Second Law of Momentum.

The impressed force is proportional to the momentum *altered*, in a given time; and it has the same "currency."

Force is the *time*-rate of *change of momentum*, or, conversely, *momentum* is the *time*-integral of *force*.

2 × force is the *space*-rate of change of *vis viva*, or, conversely, *vis viva* is the *space*-integral of 2 × *force*.

Fundamental Principles.

Conservation of *momentum* and of *vis viva*. This follows logically from the assumption of all bodies being *rigid*; consequently there can be no room for light, heat, or electricity, which depend on *vibration*.

Dynamics is, really, only idealization of one aspect of "real bodies"; all the properties—or want of properties—of the bodies with which it deals being *arbitrarily assumed*, by definition.

SCALAR

Energetics

The Science of *Energy* ($\frac{1}{2}$ Mv^2).

An Inductive science.

All dimensions are in length, mass and time. All equations are *Scalar*.

Calling P the measure of the *power*, and dE an element of the energy *transformed* —*kinetic* to potential, or *vice versa*—in a differential of time, then

$$P dt = dE = d(\tfrac{1}{2}Mv^2) = Mv.dv,$$

Scalar equation, where v = *speed*.

$$\therefore P = dE/dt,$$

and, "dimensionally,"

$$[M]^1[l]^2[t]^{-3} \times [t]^1 = [M]^1[l]^2[t]^{-2}.$$

Calling Φ the measure of the "effort," or "stress," in the direction of the motion, then

$$\Phi\ dl = dE = d(\tfrac{1}{2}\ Mv^2) = Mv.dv.$$

All "energy" is added *arithmetically*.

Second Law of Energetics.

The impressed power is proportional to the energy *transformed* in a given time (no reference to direction).

Power is the *time*-rate of *transformation of energy*, or, conversely, *energy* is the *time*-integral of *power*. *Effort*, or *stress*, is the *length*-rate of *transformation of energy*, or, conversely, *Energy* is the *length*-integral of *effort*.

Fundamental Principle.

Conservation of energy. This is only tentatively true: we *assume* that all systems are conservative, since we know of no exception. Motion (molar motion) is *not conserved*: all motion being perpetually destroyed: the kinetic energy being transformed into the "shriller" varieties of light, heat and electricity.

Energetics is based on observed phenomena by abstraction, classification and inference, the properties of the "real bodies," with which it deals, being derived from observation or experiment.

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CHAPTER IX

VISCOSITY AND RIGIDITY

IN the study of Hydromechanics—and Aerodynamics—frequent use is made of the word " viscosity." Very many books and papers have been written about the resistance which is encountered by bodies moving in " viscous fluids " ; books and papers charged with very forbidding equations, which are anything but attractive to either the engineer or the young student. Some of the writers of these books admit—perhaps rather apologetically—that Hydromechanics would be a comparatively simple study *if it were not for viscosity*.

In spite of this, I think I may say that the authors themselves have but a hazy notion of what they really mean by the " resistance *due to viscosity*." They generally use the word " viscosity " in two separate and very distinct senses, viz., (1) as a property of fluids, and (2) as a " resistance to motion." For example, Lord Rayleigh (Royal Institution), March 20th, 1914, said : " In dealing with a large and interesting class of fluid motions we cannot go far without including fluid friction, or viscosity, as it is called, in order to distinguish it from the very different sort of friction encountered by solids, unless well lubricated."

It is true that when lecturing he added (in an aside, which is not printed), that we did not know what fluid friction was, but that it was " a very convenient expression."

We see that Lord Rayleigh here uses the word " viscosity " as synonymous with " fluid *friction*." Friction is an " action "—" the act of rubbing "—though Lord Rayleigh allows it is very different in solids and in liquids.

To treat of viscosity at all thoroughly would require devoting a whole book to it. Since I must be brief here, I will commence by giving my definition of viscosity, and then giving my reasons for having adopted it. Since, also, I

consider—foolishly, some may perhaps think—that one does not understand a subject unless one can explain it to " the man in the street," an endeavour will be made to treat this subject from an everyday point of view. By " man in the street " I mean an educated man, but not a specialist. If, for example, a well-educated engineer does not understand what I mean, I shall have failed in my task: whether he *agrees* with my views or not, is a separate question.

Definition: Viscosity is that property of matter in virtue of which it resists " shear."

If I left this definition, as thus stated, and without explanation, I should convey an *entirely erroneous impression,* since " shear," as ordinarily understood, suggests *cutting;* and cutting is by no means the idea which I wish to convey. In fact it is *exactly the opposite* of my idea ; which is that there is *no cutting*—no physical discontinuity. I must, therefore, rigidly define " shear."

Let me quote from that excellent work of P. G. Tait, *Properties of Matter:*

" When a liquid partially fills a vessel, and has come to rest, it assumes a horizontal upper surface. If the vessel be tilted, and held for a time in its new position, the liquid will again ultimately settle into a definite position, with its surface again horizontal. Practically it occupies the same *bulk* in each of these positions. Hence *the only change* it has suffered is a *change of form.*

" . . . let us consider . . . what is involved in *shear: i.e., change of form of a body without change of bulk.*"

This is the correct *mechanical* meaning of " shear " : this is the exact sense in which I employ the word, and not as in any way suggesting " cutting." It is the sense in which a sailor says, " Now ! sheer off." It suggests *pushing.*

To continue from this quotation : " There has thus been change of form only . . . and *we see* [?] that it has been produced by the *sliding* of every horizontal layer . . . over that immediately beneath it. . . . Now when there is resistance to sliding of one solid on another we call it *friction.* Thus the viscosity of a fluid *is due to its internal friction.*"

Prof. Tait says here, " *We see* [emphasis mine] that the change of form has been produced *by the sliding,* etc." As a

matter of fact, *we see nothing of the sort.* What we do see is that the higher layers move faster than the lower ones—that is indisputable. Between any two *contiguous layers,* however, there is no *finite* difference of motion : the difference is only *a differential.* There is *no sliding !**

I regret to say that even Prof. Tait is, in this quotation, arguing over-hastily from solids to liquids, which is a most dangerous practice. Having said that " the resistance to sliding of one solid on another we call friction," he *assumes,* like most authors, that the particles of fluid *slide* over one another ; after which he concludes, " Thus the viscosity of a fluid *is due to its internal friction.*"

It is in the *interpretation* of this observation that I beg to differ from Prof. Tait.

In the first place, the molecules of fluids do not *touch* one another, and contiguous molecules have no *finite* difference of motion past one another ; it is not easy therefore to understand what is meant by the word " friction " here. Besides, in another place, Tait says that " *the property of viscosity* is possessed by *even the most elastic of solids* " ; in which finite slipping of layers is not even suggested.

We see here that Tait considered that viscosity is the " property " of a liquid—" *due* to its internal friction " [whatever that expression may mean]. Lord Rayleigh, on the other hand, used the word " viscosity " as being synonymous with the " fluid friction " of the liquid.

Supposing, however, that we granted this " friction "—which I do not—there is another insuperable difficulty in connection with it. Stokes, who was, more or less, a believer in the " fluid friction theory," was nevertheless not blind to its defects, since he wrote : (Camb. Phil. Soc. Trans., Vol. VIII) : " Even the supposition of a certain friction . . . would be of no avail, for such friction *could not be transmitted through the mass.*"

This ought to be sufficient to demolish this " friction

* I see that in my *Motion of Liquids,* p. 159, I said that " The top layer of molecules will *slide* over the next layer." This is a slip : there is no *finite slide.* What was intended was that there was a *differential motion.* This is clear from the calculations that follow. It is also in accordance with the views of Unwin, whose article *Hydromechanics,* Ency. Brit., IX edition, I am there quoting from.

theory," but there is still another extremely important question, which does not appear to have been referred to. All the energy communicated to a liquid is, eventually, changed into heat. This is indisputable. Now, *how* is the energy transformed? What is the machinery for carrying out this transformation? How is it *done*, in short?

It is a fact—not generally stated, but not, I fancy, disputed—that you cannot *directly* transform molar motion into heat. The kinetic energy must *first* be transformed into *potential energy*. If anyone disputes this, it is for him to *show how it can be done*. It is not for anyone to prove a negative!

It is, of course, vastly easier to raise objections to any theory than it is to propose a better and a simpler one. Finding fault, also, leads to no purpose unless one can explain "facts" by a suggested theory which is, in reality, simpler than the one it is being substituted for. I propose, therefore, to now give the reader a rough idea of how the particles of a body actually move when under *shear;* such motion being preliminary to that of *flow*. This will assist him in following my criticism of the "fluid friction theory."

Some seven or eight years ago I formulated a theory of liquid flow, which I thought was new and original. Original it was; but "new" it certainly was not; for a series of accidents have since led me to the discovery that it was formulated by Poisson as far back as 1831.

I cannot enter into all the details of Poisson's theory, but Clerk Maxwell, *On Double Refraction in a Viscous Fluid in Motion* (Proc. Roy. Soc., 1873–4), when referring approvingly to it, says: "According to Poisson's theory of internal friction of fluids, a viscous fluid *behaves as an elastic solid would do* if it were periodically *liquefied* for an instant and *solidified again*, so that at each fresh start it becomes *for the moment* like an elastic solid *free from strain*."

This very crisp and clear summing-up means, in other words, that a viscous fluid (liquid or gaseous) behaves, *first*, like an *elastic solid; i.e.*, when subjected to "shear," it is distorted, or *strained*. Poisson really only refers to a *viscous* fluid; I say *every fluid*—in fact, *every form of matter*, since *all* bodies (even the hardest steel) can be made to "*flow*": and this is the first step in "flow." What a "mathematical

fluid "—which, apparently, has only negative properties—would do, I am not prepared to say.

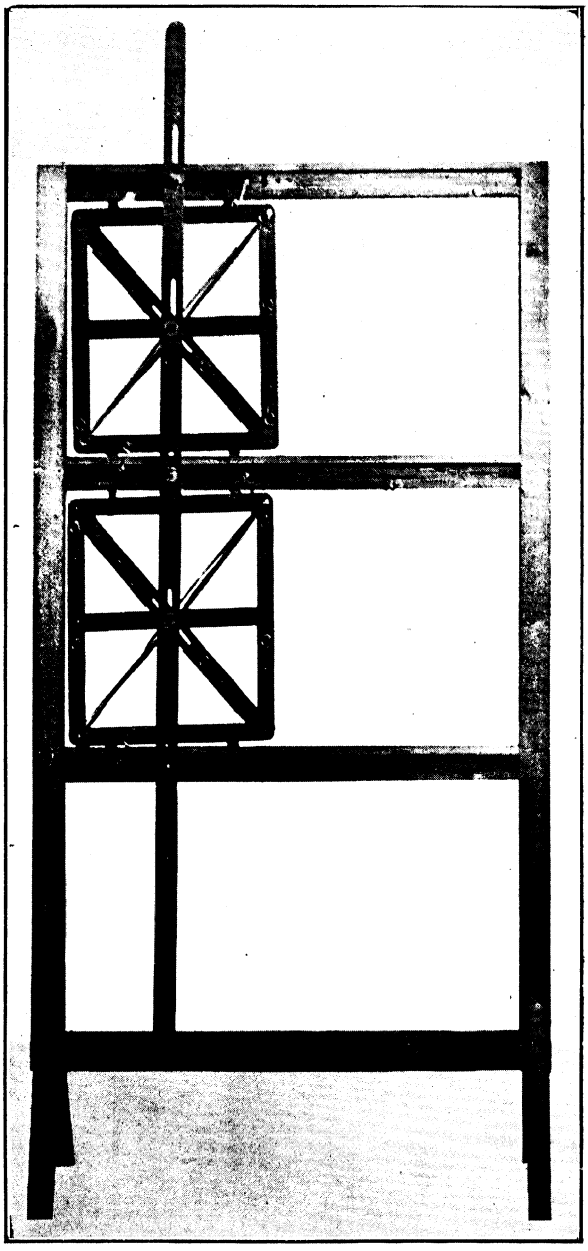
This strain lasts only for an *exceedingly short time*, after which it breaks down, and the potential energy of " strain " is *transformed into heat*. The still smaller period during which this last transformation takes place, Maxwell calls " the time of relaxation." In his own words, " the ' time of relaxation ' of a substance strained in a given manner is the time required for the complete *relaxation of the strain*, supposing the rate of relaxation to remain the same as at the beginning of the time."

From this argument a very curious fact, which Poisson and Maxwell appear to have overlooked, follows logically; viz., that the generation of heat *takes place in quanta*. (I use the word " quanta " because it is the fashionable and learned expression which is generally employed.) What I mean is that the heat is generated " periodically," or " in jerks " —not *continuously*. The molecules of the fluid being only capable of sustaining a *certain* strain, it follows that the heat is not only generated at periodic times but also in fixed *quantities*.*

I have said that the energy of strain is transformed *into heat*. This, like very nearly every statement one makes in Mechanics—Hydromechanics especially—is only *conditionally true*. The condition is that the " time of relaxation " must be of *the correct order of minuteness*. If, for example, this duration were of the order of 10^{-15} of a second—a thousand million millionths of a second—the relaxation would cause *a bright flash of light*.

This idea is not a fantastic one, since flashes of light are known to occur in liquids; the detonation of nitro-glycerine is most probably caused in this manner. When certain crystals are pounded in a mortar, flashes of light are also frequently seen. The flash of the well-known " flint and steel " is also produced in this manner.

* Mr. Bertrand Russell (*A.B.C. of Relativity*), says that " at present there is no bridge connecting the *quantum* with the theory of relativity." Also (*A.B.C. of Atoms*), " Planck's quantum, for the present, is a *brute fact*. It is involved in all very small periodic processes; but *why this should be the case we do not know*." Although it does not fit in with " relativity," it appears to be very simply explainable by Newtonian mechanics.



Ordinarily, however, heat only is produced. We can imagine, however, the " time of relaxation " not being *small enough* to generate heat. In such a case *electricity* would be generated ; and we can imagine the relaxation taking place so slowly that the waves of the electric current produced might have a length of several hundreds of kilometres.

Lord Kelvin used to say that one did not understand any motion properly until one had made a model of it. Bearing this very excellent advice in mind, I had a model made which will, I trust, show simply and clearly what I suggest takes place when a body is put " in shear."

There are two jointed square frames, shown in Plate I, which represent the " molecules " of the body to be put in shear. These " molecules " have all the necessary degrees of freedom for " shear distortion " ; and the " molecular forces," which are brought into play, are represented by india-rubber bands or strings, as shown in the model. The molecules do not touch one another, but are held together by " forces of cohesion "—represented by brass hooks.

In Plate II the molecules are shown as in *pure shear*. They are distorted, as shown, but there has been *no sliding action* between them ; though the centre of gravity of the upper one has moved further than that of the lower one.

The molecules are now full of " energy of strain " At this period, I cause the strain to " break down," by means of suitable " triggers."

It will be seen in Plate III that the " molecules " have resumed their original shape, but there has been no *real sliding*. The motion, as will be seen, is irrotational ; and all the energy which I put into the model has been transformed into heat.

I wish specially to draw attention to the motion of the " molecules " being *irrotational* (they turn through a small angle, and then back again), since it is commonly taught that all rotational motion in liquids is *caused by viscosity*. This, however, is the reverse of the truth : vortices in fluids are *not caused by viscosity*. Viscosity indeed *prevents*, and *damps down* all vortices and rotational motion.

In the re-arrangement of the molecules, here shown, the " break down " of the strain takes place *very rapidly*. This,

however, is not at all a *necessary* condition of "shear"; the molecules (especially those of solids) frequently re-arrange themselves *very gradually*, and even *very slowly*.

Take lead, for example; it is very easily "distorted," but it takes a fairly long time for its molecules to *re-arrange themselves; i.e.*, the "time of relaxation" is rather large. Therefore, if the "shearing action" is applied too rapidly, the material will "tear"; *i.e.*, there will be a distinct *physical discontinuity*.

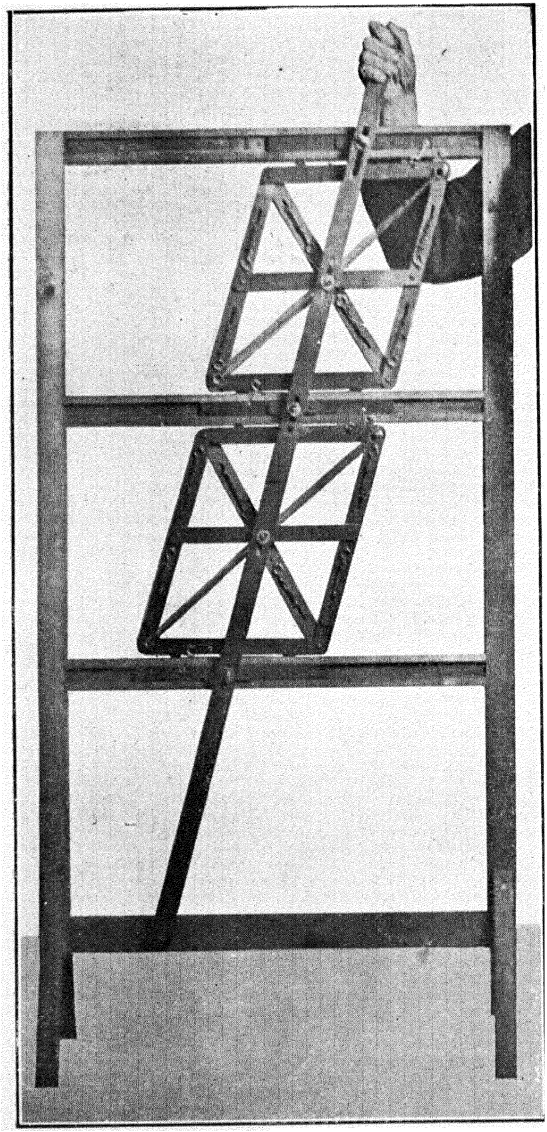
Copper is very fairly "malleable." When a workman is doing "repoussé work," he hammers the copper, and so, changing its shape, generates "energy of strain" in it. The "time of relaxation" of copper is considerable; therefore the "distortion" cannot be continued beyond a certain amount, without fear of *physical rupture* of the material. In order to get over this difficulty, and inconvenience, the workman heats the copper, and thus *hastens* the relaxation of the strain in the metal; after which he can again proceed with the "distortion" by hammering.

When a metal (say steel) has been distorted and its molecules have subsequently re-arranged themselves, the material no longer strives to *return to its original shape*: as the engineers say, it has "taken a *permanent set.*"

We see that, as Tresca pointed out, "The flow of liquids is only *a particular case* of the flow of *all bodies.*" "Heraclitus the Obscure," more than 2,000 years ago, observed that "All things flow." He was laughed at for this; but we are finding out that he was quite right. There is no *essential* difference between solids and liquids. "There seems no line of demarcation between a solid and a viscous fluid" (Stokes).

This theory of flow is clearly strictly applicable to the flow of glaciers, which, as is now well known, flow in an exactly similar manner to rivers—and for the same reasons.

I have said that, if the "time of relaxation" is not sufficiently minute to generate heat, an electric current is generated. This statement is confirmed by Sir Jagadis Chunder Bose's experiments, *The Response of Inorganic Matter to Mechanical and Electric Stimulus* (Roy. Inst., May, 1901). He showed that simply striking a piece of metal would cause the production of an electric current. It is hardly necessary



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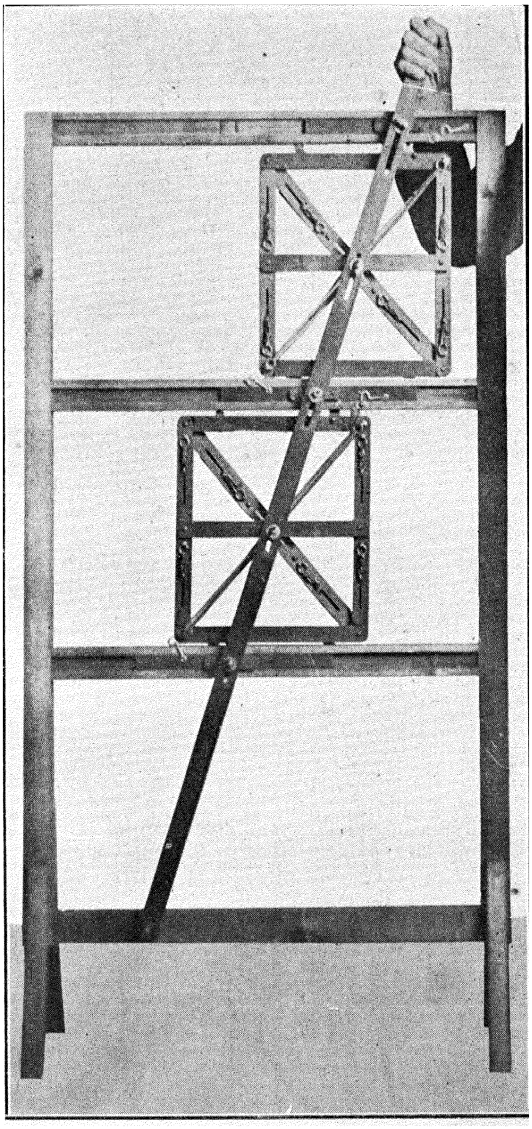

III

to point out that the metal is *strained* by the blow. Being elastic, the deformed part of the metal will spring back into, *very nearly*, its original shape. Since, however, it is not possible to deform any material without *some part* of the energy being transformed into what Lord Kelvin called a " shriller variety " of energy, a part is here transformed into " electricity."

I have spoken of a metal " tearing " if the shearing action is applied *too suddenly*, always provided that the elasticity* [using this word in its popular sense of " capability of rapid extension and recovery "] of the metal is not sufficient.

It will be found that the material " tears " (as would be expected) along lines more or less parallel to AD (Fig. 3), in consequence of the tension B to C. It will, at the same time, show a tendency to crumple, or " crinkle " along lines parallel to BC—in consequence of the pressure A to D.

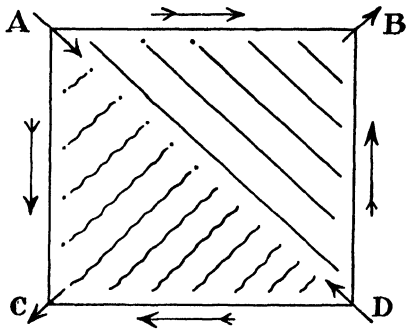

FIG. 3.

The " dimensions " of μ, the coefficient of viscosity, are those of a stress, divided by an area, and by a speed ; and multiplied by a length, or distance. . That is, the " dimensions " are

$$\frac{\mathrm{M}lt^{-2}}{l^2 \times \frac{l}{t}} \times l, \text{ or } [\mathrm{M}]^1[l]^{-1}[t]^{-1}.$$

RIGIDITY

What do we mean in Mechanics by " rigidity "? Probably most people have an idea that they know ; but it would not, I fancy, be very difficult to show them that they do not know as much as they think. They would probably say that

* This is not a good word, but I can think of no better (? stretchability, extensibility).

" Rigidity is the capacity for resisting change of form " ; and, as examples, they would say that steel was rigid, whilst water was not. Suppose, however, that I asked, " At what temperature ? " It would then be apparent to them that *temperature* had to be taken into account, and the answer to the question is now seen to be not quite so simple as it looks at first sight. As a matter of fact, molten steel is, I believe, *more fluid than water* (at ordinary temperature).

I know of no text-book on *Dynamics* which defines rigidity. The definition which I formulate is: " Rigidity is the measure of resistance to change of shape *in a given time.*"

Tait in his *Properties of Matter* refers to rigidity in two places :

" 178. We now define as follows :

" *The Rigidity of an isotropic solid* (*i.e.*, the resistance to change of form under a stress . . . . .), [tangential stress] *is directly proportional to the tangential force per unit area, and inversely as the change of one of the angles of the figure.*"

Prof. Tait is here nodding. His wording, between his brackets, does not agree with his wording *in italics*. He has omitted the *time-condition* between the brackets ; the " resistance to change of form, etc.," is, practically, his definition of "viscosity."

The " Dimensions " of Rigidity (according to Tait) would be, in my terminology, " a stress divided by an area " (since an angle has no " Dimensions ") or :

$$\frac{\mathrm{M}lt^{-2}}{l^2} = [\mathrm{M}]^1[l]^{-1}[t]^{-2}.$$

That this is what Tait *really meant* is clear, since, in another place, he says : " The Dimensions of Viscosity, therefore, *differ from those of Rigidity*, simply by the *time-unit ;* as the dimensions of velocity differ from those of acceleration."

It is well to carefully note the meaning I attach to the word "stress." Tait says: " *Every action between two bodies is a stress.*" With this language I most cordially agree ; but Tait says, in another place, " Stress is *force per unit of surface,*" which is *quite a different thing.* Here stress is used in the sense of " *the intensity* of a Pressure," or, as I would say, *the intensity of a Stress.*

The correct relationship between Viscosity and Rigidity is therefore as stated by Tait :

$$\text{Rigidity} \times \text{Time} = \text{Viscosity},$$

or, mathematically expressed, " Rigidity is the *Time-Rate* of the resistance to change of shape." Rigidity is thus the measure of *the Intensity* of a body's resistance to change of shape : and the relationship between Viscosity and Rigidity can be expressed by the Equation to a Rectangular Hyperbola, as shown in the accompanying Fig. 4. It the time be small enough, the "Rigidity"—*the intensity* of the resistance to change of shape—may be as great as we please : the amount of the viscosity is then comparatively unimportant.

Authors very commonly say that, " Rigidity is *not possessed* by Fluids." This is one of those half-truths which are frequently so misleading. It is true that if you deform a fluid, in a leisurely manner, the resistance which it will offer to this deformation is " very small." If, however, you are in a hurry you will find that the resistance it offers is far from negligible. Any bather who has dived badly into the water from a moderate height will be satisfied that water can be *very hard*, and that its resistance to change of shape is sometimes very considerable. In a heavy gale, also, great waves sometimes smite a ship with the force of a steam-hammer.

It appears that in the plant of the Consolidated Virginia and Col. Mining Company, at Virginia, Nevada, water issues from a nozzle under a pressure of 911 lbs. per square inch. The Rigidity of this jet of water is such that it is said to be impossible to cleave the jet with an axe, as the axe will rebound just as it would from a hard steel rod.

A circular sheet of brown paper made to spin rapidly about its centre becomes as rigid as a board of wood.

A closed chain put on a rapidly-rotating mandril assumes a circular shape. If it be then pushed off the mandril it will run along the floor like an iron hoop.

All this is well known, but the facts are commonly referred to as cases of " Pseudo-Rigidity "—whatever that expression may mean.

Similarly with gases, which, as Clerk Maxwell points out

(*The Dynamical Theory of Gases*), also "*offer a resistance to change of form.*" The force of the wind in a heavy gale is known to everybody. The "Rigidity" of the air may become

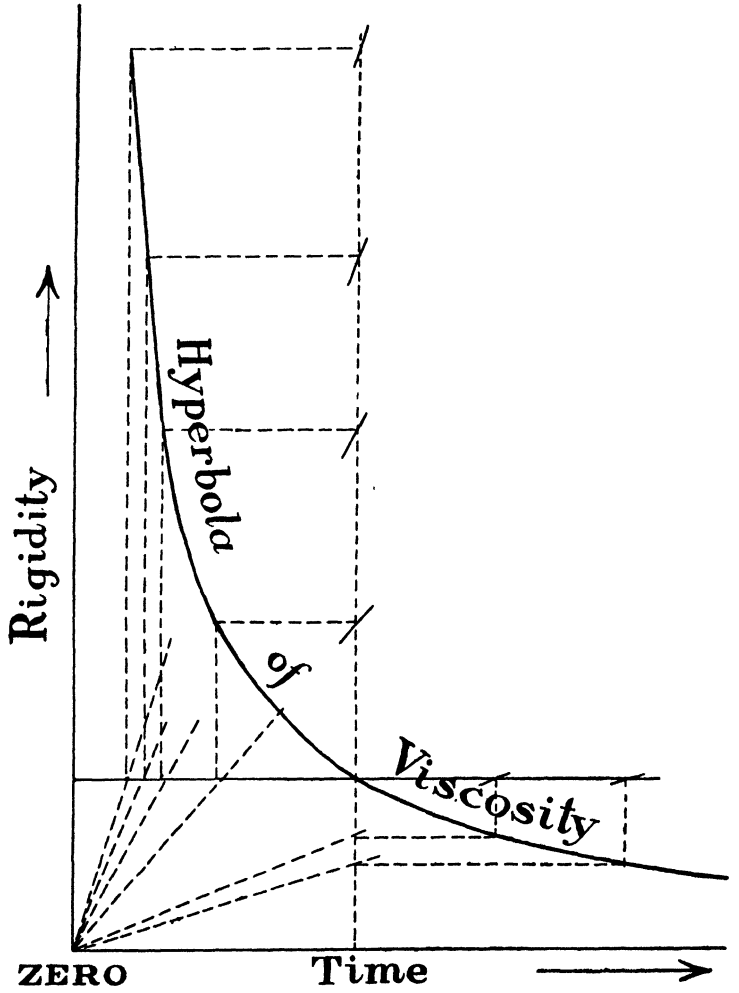

FIG. 4.

enormous. Anyone who has witnessed, as I have, the effects of a hurricane or tornado, who has seen how it can cut a straight avenue through a dense forest; anyone who has seen this will never deny that the rigidity of air may become colossal.

Let us go a step further, and refer to the ether. This medium is still usually spoken of as a " jelly." It is assumed to have convenient properties, since when it is required that it shall be rigid, it is said to be *one million times* more rigid than steel (Sir Oliver Lodge). Try and imagine going into a room filled with steel " jelly " : but ether is *a million times* more rigid than that !

At the same time, this " jelly " is so evanescent that it offers, practically, *no resistance* to the motion of the Planets through it—at least none has been observed by astronomers, except in the case of Mercury, which, in consequence of its very elliptical orbit, moves with exceptionally accelerated motion round the sun.

Newton's views on the ether are certainly very interesting, if only historically. He says, in his *Opticks*, Query 21 :

" An attraction is stronger in small magnets than in great ones, in proportion to their bulk, and Gravity is greater in the surfaces of small planets than in those of great ones in proportion to their bulk, and small bodies are agitated much more by electric attraction than great ones ; . . . and so if anyone should suppose that ether (like our air) may contain particles which *endeavour to recede from one another* (for I do not know what this ether is) and that its particles are exceedingly smaller than those of air, or even than those of Light ; the exceeding smallness of its particles may contribute to the greatness of the force by which those particles may recede from one another, and thereby make that medium exceedingly more rare and elastic than air, and by consequence exceedingly *less able to resist* the motions of Projectiles, and exceedingly more able to *press upon gross bodies*,* by endeavouring to expand itself.

" Query 22. May not planets and comets, and all gross bodies, perform their motions more freely, and with less resistance in this etherial medium than in any fluid, which fills all Space adequately without leaving any pores, and *by consequence* is much denser than quicksilver or gold ? and may not this resistance be so small as to be inconsiderable ? For

* [? in the sense used by Descartes in his letter to Father Mersenne (quoted by me at p. 128), when referring to a Fluid which would offer no Resistance to a body moving in it.—R. de V.]

instance, if this ether (for so I will call it) should be supposed 700,000 times more elastick than our air, and *above* 700,000 times *more rare;* its resistance would be above 600,000,000 times less than that of water, and so small a resistance would scarce make any sensible alteration in the motions of the planets *in ten thousand years.*"

This passage could be read several times with advantage, for it is most pregnant and suggestive. We see that Newton imagines an Ether which is *more than* 700,000 times *less dense* than our air.

Mendeléef, in, I believe, the last pamphlet he wrote, on *A Chemical Conception of the Ether*, 1904, estimates that the density of the ether is somewhere about *one million times less than hydrogen*. He has even a place for it in his famous Table.

Now, if we take the speed of Light as 186,000 miles per second, and the length of a medium wave of Light as 2×10^{-5} inch (the lengths vary from $2{\cdot}71 \times 10^{-5}$ to $1{\cdot}55 \times 10^{-5}$ in.), if my arithmetic is correct, there will be about 600 *millions of millions of waves* (6×10^{14}) per second. But for each wave of Light the ether has its shape changed twice. The time of each change will consequently be one 1,200 *billionth of a second!!* The "Rigidity" of the ether, therefore—the *intensity of its resistance* to change of form—in this extraordinarily small interval of time, will be almost inconceivably great.

At the same time, I am not prepared to approve the bald statement that "the ether is a million times *more rigid than steel*"—without any reference to the Time-condition of Rigidity.

I beg also to differ from Clerk Maxwell (*On the double Refraction in a viscous Fluid in Motion*) when he says that "a given liquid differs from a solid in having a *very small 'rigidity,'* or in having a small 'time of relaxation,' or in both ways."

We must not omit the "Time-condition" of Rigidity; but taking this into account, I venture to suggest that the Rigidity of either Newton's, or Mendeléef's Ether, as shown above, should be sufficient to satisfy the requirements of even the most exacting mathematician.

The reader may, not unreasonably, ask how the view I

have advanced will fit in with what is called " solid friction." Is not this, at least, caused by a " rubbing action " ?

I do not think so. Take, for example, the case of a " shaft " working in a " journal." The action is, I fancy, as follows :—

1. The shaft and journal " seize."

2. The material of both is then " strained "—Kinetic Energy being converted into Potential Energy of strain.

3. The strain breaks down, and the Energy of strain is very rapidly transformed into the " shrill form " of Energy —Heat. The cycle being " seize, strain, heat," repeated with extreme rapidity.

How the lubricant affects this action is not as clear to me as I could wish. That it reduces the " seizing " is, I fancy, certain ; but the action of " lubrication " is not yet properly understood. The value of a lubricant appears to depend on a specific property which may be called " Greasiness " ; but why two oils, which appear very similar, should possess different " Greasiness " is not quite evident.

In recapitulation :

Viscosity is the measure of the Resistance of a body to *change of shape.*

Rigidity is the measure of the Resistance of a body to change of shape *in a given time ;* or, otherwise expressed,

Rigidity is the measure of *the Intensity* of a body's resistance to change of shape.

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CHAPTER X

RESISTANCE

IF one considers the enormous amount of theoretical work which has been done on this subject, it may appear a very extraordinary statement to make, that the resistance which bodies experience when moving through liquids and gaseous fluids is very imperfectly understood, even at the present day. Nevertheless, Lord Rayleigh, during a Friday evening discourse at the Royal Institution in March, 1914, on *Fluid Motions*, made the following remarks: "The general equations of fluid motions, when friction or viscosity is neglected, were laid down in quite early days by Euler and Lagrange, and *in a sense* they should contain the whole theory. But as Whewell remarked, it soon appeared that these equations by themselves take us a surprisingly little way, and much mathematical and physical talent had to be expended before the truths hidden in them could be brought to light and exhibited in a practical shape. What was still more disconcerting, some of the general propositions so arrived at were found to be in flagrant contradiction with observation, even in cases where at first sight it would not seem that viscosity was likely to be important. Thus a solid body, submerged to a sufficient depth, should experience no resistance to its motion through water. On this principle the screw of a submerged boat would be useless, but on the other hand, its services would not be needed. It is little wonder that practical men should declare that theoretical hydrodynamics has *nothing at all to do with real fluids.*"

The latter part of this statement alludes to what is commonly known as "d'Alembert's Paradox." Stated briefly, d'Alembert said that a body moving uniformly, in a perfect, compressible fluid, indefinite in all directions, will experience *no resistance whatever.* He sums up the case thus: "a perfect fluid opposes no resistance to the uniform translation of a solid."

M. Léon Lecornu, *Cours de Mécanique*, Tome II, 1915, referring to this, says: "This very surprising result constitutes the Paradox of d'Alembert."

In 1781 Lagrange published his *Théorie du Mouvement des Fluides* (Acad. Roy. des Sciences de Berlin), in which he says: "Since d'Alembert reduced to analytical equations the true laws of the movement of fluids, this matter has become the object of a great number of researches which are to be found scattered in the smaller works of d'Alembert. The general theory has been much improved in these different researches; but it is otherwise for that part of the theory which concerns the manner of applying it *to particular questions*. M. d'Alembert even appears to be inclined to believe that *this application is impossible in most cases*, especially when dealing with the motion of fluids which flow in vessels [*vases*]."

Lagrange's equations are very complete, and—*granted his assumptions*—appear to supply a very excellent basis for the solution of problems on this subject. It can be shown from these that a body moving uniformly in a "perfect fluid" does not meet with any resistance to its motion.

If the word "perfect" be taken to mean "inviscid," it is a logical deduction from Lagrange's equations that, since bodies moving uniformly in water, or air, *do* actually experience resistance to their motion, this resistance *must be due*, directly or indirectly, to the fact that the liquids are viscous.

Common sense rebels against such an idea; but if we are satisfied with Lagrange's equations, we are *irresistibly driven* to accept this *logical conclusion*.

There is one point about which there can be, I think, no dispute, and that is that *real fluids* do, sometimes, flow as Lagrange said they ought to; but that in the great majority of cases they do nothing of the sort—but *behave quite differently*. The reason for this I discussed at considerable length in *A.B.C. of Hydrodynamics*,* 1912, to which the reader is referred.

Thinking, however, at that time of incompressible fluids only, it is clear to me now that the explanation which I then offered was not broad enough to cover the cases of compress-

* Published by E. & F. N. Spon, 1912.

ible fluids, like air. A broader, and more general, explanation covering both compressible and incompressible fluids is required, if it is to be considered at all satisfactory.

In 1911 I showed that, by assuming the fluid to be incompressible, and of *infinite dimensions*, " discontinuity " in the fluid becomes impossible, since the dimensions of the fluid *could not be increased;* and, further, that *without discontinuity*, any resistance due to inertia would be impossible. This, clearly, would not cover the cases of compressible fluids.

In my search for conditions which would *necessarily* produce continuity, I had quite overlooked the fact that Lagrange, without hesitation, had simply *assumed continuity*.

Similarly, it is very curious that the large majority of people who study Lamb's *Hydrodynamics* do not appear to realize the conditions contained in the first three lines of this book: " 1. The following investigations proceed *on the assumption* that the matter with which we deal may be treated as *practically continuous* and homogeneous in structure."

Now, Lagrange in the paper previously quoted from, says: " Now, *in consequence of the continuity of the fluid*, we may imagine, etc." He considers this continuity as an almost self-evident truth.

When, however, one assumes continuity, one at the same time implicitly *assumes* unresisted flow. This implication Lagrange appears to have overlooked.

Assuming continuity, therefore, d'Alembert was quite justified in saying that " a perfect fluid opposes no resistance to the uniform translation of a solid ": the word " perfect " implying *perfect continuity*.

A hypercritical reader might even say that this is tautological, as the statement is implicitly contained in the explicit assumption of continuity.

If there should be any doubt in the reader's mind about the correctness of my foregoing statement, let him refer to the letter of Descartes to Father Mersenne dated 9th January, 1639—three years before Newton was born. He says: " When I conceive that a body moves in a medium *which does not hinder it at all*, I suppose that all the particles of the

liquid body which surround it are disposed to move *exactly as fast as it*, and no faster, whether in ceding to it their place, or in occupying that which it has quitted; and thus *there are no liquids* which are such as in no way to hinder certain movements."*

Some years later (1647) he also said, in the *Principes de Philosophie*, "that a body ought not to be considered as *absolutely fluid* [perfectly fluid] in regard to an inelastic body [*corps dur*] which it surrounds, when some of its particles *move less quickly than the inelastic body does.*"

This is as much as to say that a fluid which is *perfectly fluid*—a "perfect fluid," in fact—is one where *the continuity is perfect.*

That Newton was well aware of all this seems almost certain: a careful perusal of Bk. II of the *Principia* more than suggests it, though it is not stated definitely.

What Newton actually says (Motte's trans.), Prop. 37, Book II, is this: "A fluid must be compressed to become continued; it must be continued and non-elastic [incompressible] in order that all the pressure arising from its compression may be propagated in an instant; and so acting equally upon all the parts of the body moved, may produce no change of resistance. The pressure arising from the motion of the body is spent in generating a motion in the parts of the fluid, and this creates the resistance. But the pressure arising from the compression of the fluid, be it never so forcible, if it be propagated in an instant, generates no motion in the parts of a continued fluid, produces no change at all of motion therein; and therefore neither augments nor lessens the resistance. This is certain, that the action of the fluid arising from the compression cannot be stronger on the hinder parts of the body moved than on its fore parts, and therefore cannot lessen the resistance described in this proposition. And if its propagation be infinitely swifter than the motion of the body pressed, it will not be stronger

* Quand je conçois qu'un corps se meut dans un milieu qui ne l'empêche point du tout, c'est que je suppose que toutes les parties du corps liquide qui l'environnent sont disposées à se mouvoir justement aussi vite que lui et non plus, tant en lui cédant leur place qu'en rentrant en celle qu'il quitte; et ainsi il n'y a point de liqueurs qui ne soient telles, qu'elles n'empêchent point certains mouvements.

on the fore parts than on the hinder parts. But that action will be infinitely swifter and *propagated in an instant*, if the fluid be *continued and non-elastic.*"

It is very curious that all this should have been forgotten, and that no one should (as far as I am aware) have noticed that d'Alembert, Lagrange and the modern mathematicians are arguing in a vicious circle, when they state that a body moving uniformly in an inviscid, incompressible fluid, meets with *no resistance*—they having previously *assumed continuity*: which implies zero resistance.

Is it wonderful, therefore, that, as Lord Rayleigh put it, " practical men should declare that theoretical hydrodynamics has *nothing at all to do with real fluids*"—*which are not continuous?*

Although my sympathy is all with the practical men, nevertheless I feel that such a declaration rather overstates the facts, since it is undeniable that when the motion of the fluid is *practically continuous*, real fluids do behave as the mathematicians say perfect fluids do. For example, in the well-known experiments made with Dr. Hele Shaw's apparatus, the results obtained agree *most accurately* with Theory. I have, at different times, by kind permission of Dr. Hele Shaw, reproduced a good many of his photographs to show this remarkable agreement.

If we wish to understand the practical subject of " Resistance," it is necessary that we should turn back again to Newton's teaching, which is applicable to *real fluids*, in contradistinction to perfectly continuous fluids, which do not exist.

Newton stated, in the *Principia* (Bk. II, Prop. 4, etc.), that neglecting the resistance due to viscosity, the resistance experienced by bodies moving uniformly in incompressible, or more correctly, *nearly incompressible*, fluids, varies as the density of the medium, and as the square of the speed of the moving body. For the resistance experienced in compressible fluids, " as our air," Newton further stated that we must " *augment this resistance a little.*"

A few years ago I had occasion to consult Newton's *Opticks*, when I made what was, to me, a very curious and interesting discovery. A book on optics is hardly one to which one

would turn for a discussion of the resistance of bodies moving in fluids, but in the 28th Query one may read: "For the resisting power of fluid mediums arises partly from the attrition of the parts of the medium, and partly from the *vis inertiæ* of the matter. That part of the Resistance of a spherical body which arises from the attrition of the parts of the medium is very nearly as *the diameter*, or at the most, as the *factum* [product] of the diameter and the Velocity of the spherical body together. And that part of the Resistance which arises from the *vis inertiæ* of the matter, is *as the square of the factum*. And *by this difference* the two sorts of Resistance *may be distinguished from one another in any medium;* and these being distinguished, it will be found that *almost all the Resistance* of Bodies of a competent magnitude moving in air, water, quicksilver, and suchlike fluids, with a competent velocity, arises *from the vis inertiæ of the parts of the fluid.*"

This passage cannot be read too frequently. We see here that, amongst other things, Newton forestalled Sir George Stokes by more than one hundred and fifty years, in pointing out what is commonly known as "Stokes' Law." He does not appear to have considered it any great discovery; he simply states it as what he apparently considered an almost *self-evident truth.*

These Queries are of a much later date than the original *Principia*, and so may be taken as giving Newton's more matured views. It would thus rather appear as if he was not aware of "Stokes' Law" when he wrote the first *Principia* in 1685. This Query 28 is not to be found in the First Edition of the *Opticks*; it was only added in the Second Edition, dated 1718.

Before leaving this Query 28 I should like to make further quotations: "The density of fluid mediums is very nearly proportional to their resistance. Liquors which differ not much in density, as water, spirit of wine, spirit of turpentine, hot oil, differ not much in resistance. Water is thirteen or fourteen times lighter than quicksilver, and by consequence thirteen or fourteen times rarer, and its Resistance is less than that of quicksilver in the same proportion, or thereabouts, as I have found by experiments made with pendulums.

The open air which we breathe is eight or nine hundred times lighter than water, and by consequence eight or nine hundred times rarer, and accordingly its Resistance is less than that of water in *the same proportion*, or thereabouts ; as I have also found by experiments made with pendulums."

Also : " Heat promotes fluidity very much, by diminishing the tenacity of bodies. It makes many bodies fluid which are not fluid in cold, and increases the fluidity of tenacious liquids, as oil, balsam and honey, and thereby decreases their resistance. But it *decreases not* the Resistance of water considerably, *as it would do if any considerable part of the Resistance arose from the attrition or tenacity* of its parts. And therefore the Resistance of water arises *principally and almost entirely* from the *vis inertiæ* of its matter ; and by consequence, if the Heavens were as dense as water, they would not have much less resistance than water ; and if as dense as quicksilver, they would not have much less resistance than quicksilver."

Roger Cotes edited the Second Edition of the *Principia*. He died young, and Newton is reported to have said, when he heard of his death, " If Cotes had lived we might have known something." In his Preface to the Second Edition, Cotes says : " [The Resistance] which arises from the want of lubricity *is very small*, and is *scarce observable* in the fluids commonly known, unless they are very tenacious, like oil and honey . . . as is most evidently demonstrated by our author in his *noble theory of Resistance* in the second book."

Such was the view of the great Newton, tersely and precisely expressed. We must not, however, rest satisfied with this, unless it will stand the very severest cross-examination by all the methods available to us. Let us, therefore, examine it from the point of view of the Principle of Similitude.

We know that, when a body is moving at a uniform speed in a fluid which has no viscosity—or, what is practically the same thing, when its viscosity is supposed to be so small as to be quite negligible—Resistance being an *effort*, or pressure, whose dimensions are $[M]^1[l]^1[t]^{-2}$. The viscosity being negligible, $\mu=0$; and this Resistance may be expressed in terms of ρ, l and v.

But by Dynamical Similarity, the Dimensions in length,

mass and time (added together) must be *exactly equal* on both sides of any dynamical equation.

(I) Consequently, we must put

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\rho]^x[l]^y[v]^z.$$

Here x, y and z are to be found; ρ being the coefficient of density, and v the coefficient of speed.

Expanding, dimensionally,

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\mathrm{M}l^{-3}]^x[l]^y[lt^{-1}]^z.$$

Equating for M, $x = 1$.

Equating for l and t,

$$1 = -3x + y + z \text{.....for } l.$$
$$-2 = -z \text{.....for } t.$$

Hence $x = 1$, $y = 2$, and $z = 2$, and the equation becomes Resistance $= \mathrm{K}\rho l^2v^2 = \mathrm{K}\rho(lv)^2$, where K is a constant.

This is in exact agreement with Newton's statement that the Resistance varies as the *square of the product* of the diameter and the speed.

(II) Let us next examine the Resistance due to the viscosity of the fluid. We know, from experiment, that when the Resistance is due *solely to the viscosity*—as is the case at *very low speeds*—the density of the fluid does not affect the result.* The Resistance may, consequently, be expressed in terms of μ, l and v; where μ is the coefficient of the viscosity. Therefore,

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\mu]^x[l]^y[v]^x,$$

and, expanded dimensionally,

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\mathrm{M}l^{-1}t^{-1}]^x[l]^y[lt^{-1}]^z,$$

the dimensions of μ being $[\mathrm{M}l^{-1}t^{-1}]$.

Equating for M, $x = 1$.

Equating for l and t,

$$1 = -x + y + z, \text{ or } y + z = 2.$$
$$-2 = -x - z, \text{ or } z = 1, \text{ also } y = 1.$$

and the equation becomes,

$$\text{Resistance} = \mathrm{K}\mu\,(lv), \text{ or,}$$

* Viscosity is what Duhem would call "a variable without inertia."

as Newton stated, the Resistance varies *as the product of the diameter and the speed.*

This is the well-known " Stokes' Law," which, as I have pointed out, was defined by Newton *more than* 200 *years ago !*

In working out (I) and (II) we assumed that the two forms of Resistance (that due to the viscosity and that due to the density) were *quite independent of one another.* This is in accordance with Newton's theory, and it has been thoroughly confirmed by experiment—notably the experiments of Coulomb, referred to by me in *A.B.C. of Hydrodynamics*, 1912. In other words, the Resistance encountered by a body moving uniformly in a viscous fluid (water, say) may be expressed as a binomial ; that is, it is composed of *two terms*—quite independent of one another—one of which is due to the viscosity alone (*i.e.*, due to the *change of shape*) of the liquid, and the other *solely to the density of the fluid medium* (*i.e.*, due to the generation of *kinetic energy* in the fluid).

As Duhem has pointed out, the differential equations of the former *are of the first order*, whilst those of the latter *are of the second order.* In fact, I have quoted Newton as having said (p. 131) " and *by this difference* the two sorts of Resistance *may be distinguished* from one another in any medium."

That this is so can hardly be doubted by an unbiassed student of Hydrodynamics. Nevertheless, the very opposite is at present taught ; and even that great leader of thought, Lord Rayleigh, said in 1914 (*loc. cit.*) : " In a well-designed ship *the whole Resistance* (apart from wave-making), may be ascribed to *skin friction** of the same nature as that which is encountered when a ship is *replaced by a thin plane moving edgeways.*"

It is difficult to understand this statement, more especially as Lord Rayleigh said himself that he did not know what " skin friction " and " fluid friction " are. Neither do we know definitely what a " well-designed " ship is. The work of this distinguished author on Hydromechanics is, one feels, perhaps his least satisfying contribution to Physics.

If we push this statement to its logical conclusion, such an expression as *a shape of least resistance* becomes an absurdity.

There is a vast amount of experimental work which is

* Whatever " skin friction " may be.—R. DE V.

diametrically opposed to this view; whilst I do not know of any experiments which favour it. It has been well known, since the time of Colonel Beaufoy, that lengthening "almost any body" tends to reduce the body's resistance to motion in a liquid.

The mathematically disposed naval architect may, in a learned discussion on Ship Resistance, bow to "authority"; but at the back of his mind he knows perfectly well that *shape* is everything, whilst the "skin friction theory" is chiefly useful for "mathematical window dressing" only.

We may, however, examine this question of Resistance "dimensionally," without making the assumption that the Density and the Viscosity of the medium are absolutely independent of one another, in the following manner:

(III) It is more convenient, by this method, not to employ simply μ (the measure of the coefficient of viscosity), but to employ the expression μ/ρ, which is called the *kinematic* viscosity, the dimensions of which are $[l^2t^{-1}]$.

Expressing the Resistance in terms of ρ, l, v and $\nu = \mu/\rho$, we get

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\rho]^x[l]^y[v]^z[\nu]^n,$$

and, expanding dimensionally,

$$[\mathrm{M}]^1[l]^1[t]^{-2} = [\mathrm{M}l^{-3}]^x[l]^y[lt^{-1}]^z[l^2t^{-1}]^n,$$

whence we get

$$\text{Resistance} = \mathrm{K}\rho\,(lv)^{2-n}\,\nu^n$$

$$= \mathrm{K}\rho\,(lv)^2.\left(\frac{\nu}{lv}\right).$$

Or (since we do not know the value of n), as it is commonly written,

$$\mathrm{R} = \mathrm{K}\rho.\,(lv)^2\,f\left(\frac{\nu}{lv}\right)$$

This "function" is generally referred to as "Lord Rayleigh's function," he having brought it to the notice of the Advisory Committee for Aeronautics in 1909–10. He gave the formula as

$$\text{“}\rho = \rho v^2 f\left(\frac{\nu}{vl}\right)\text{”} \qquad \text{.....(A).}$$

" Where f is an arbitrary function of *one* variable ν/vl, it is for experiment to determine the form of this function, or in the alternative to show that the facts cannot be represented at all by an equation of form (A)."

For some unexplained reason, the argument of this function is generally inverted at the National Physical Laboratory (Sir Thomas Stanton being an exception), so that the function reads

$$f\left(\frac{vl}{\nu}\right).$$

It is possibly for this reason that " the form of this function " has not been determined. I venture, also, to suggest that Lord Rayleigh's formula (A) is not quite as complete as it might be, from the omission of l^2—the Resistance " varying as the *factum* " of l and v. With this addition, it does not appear to be very difficult to find the " form of the function."

Up to the present, we do not know the value of n in the equation

$$R = \rho\,(lv)^2\left(\frac{\nu}{vl}\right)^n.$$

A little reflection, however, will show that there are only two possible values for this n. If there is no viscosity, $n = 0$. If, however, there is viscosity, then $n = 1$.

When $n=0$, $(\nu/vl)^n=1$; and when $n=1$, $(\nu/vl)^n=(\nu/vl)$.

Therefore, knowing from experiment that the Resistances due to the viscosity and to the inertia of the fluid are independent of one another, also that these Resistances have the same " Dimensions," and that they can, consequently, be added algebraically, the Resistance experienced by a body, moving uniformly in a viscous fluid, would be expressed as :

$$\text{(A). Resistance} = \rho\,(lv)^2\left\{1+\frac{\nu}{vl}\right\}.$$

Or, the " form of the function " $f\left(\frac{\nu}{vl}\right)$ becomes

$$\left(1+\frac{\nu}{vl}\right);$$

and, since $$\nu = \mu/\rho,$$

$$\text{Resistance} = K\{\rho(lv)^2 + \mu(lv)\},$$

which is as stated by Newton (*Opticks, loc. cit.*).

(IV) In equation (A), ν is a *very small* number. For air, at 15°c, $\nu = 0{\cdot}000159$; whilst, for water, it is less than one-twelfth of this, being only equal to $0{\cdot}0000123$.

Now, when $vl = 1$, which implies an *exceedingly moderate* "competence" of size and speed in the moving body;

$$\left\{1 + \frac{\nu}{vl}\right\} = \{1 + 0{\cdot}000159\};$$

and the Resistance *due to tne viscosity* of the fluid is only about 1/6oooth of the total Resistance; all the remainder being due to the inertia, or density of the fluid. Now one sixtieth of one per cent. is within the limits of most ordinary experimental error.

If the body be fairly large, and the speed be somewhat greater—so that $vl = 100$; a very moderate "*competency*"—the error caused by neglecting the Resistance due to the viscosity, would be reduced to *one six-thousandth of one per cent.*

We see, therefore, that in calculating the Resistances of bodies moving in air, or water, even at low speeds, we are quite justified in neglecting that part due to the viscosity of the fluid, and in treating the fluid as if it were non-viscous; the error being generally quite inappreciable. Therefore, *practically*,

$$(1 + \nu/vl), \text{ or } f\left(\frac{\nu}{vl}\right) = 1.$$

This is what has been found at the National Physical Laboratory to be the case. It does not appear to be recognized, however, that when one says that

$$f\left(\frac{\nu}{vl}\right) = 1,$$

one is *implicitly* stating that the Resistance due to Viscosity is so small as to be quite negligible.

If, however, the body be *very small*—say, for example, a

very fine wire—ν/vl may then become quite an appreciable quantity, quite a large fraction of unity, and its neglect will be perfectly unjustifiable.

This statement is not to be construed as being out of accord with Newton's teaching. Newton was quite well aware of all this, for he most carefully stipulates that the moving bodies must be of "*competent magnitude*," and that they must be moving "*with a competent velocity*." The range of this law is a fairly wide one ; but we must not extrapolate !

That there should be no mistake, Newton, in the Scholium to Prop. XXXI, tells us that "the Resistance of the globe, *when it moves very swift*, is in the duplicate ratio of the velocity, nearly, and when it moves slowly, *somewhat greater than that ratio*." That is to say, that, at slow speeds, the Resistance due to the viscosity becomes appreciable.

Clearly, what is applied here to velocity is equally true when applied to size ; the whole question depending on the value of vl, which is the product of the measures of the two.

(V) All the foregoing only applies strictly to non-compressible, that is, *practically incompressible*, fluids.

With reference to compressible fluids, Newton carefully adds : "and when the Resistance of bodies in non-elastic [incompressible] fluids is once known, we may then *augment this Resistance* a little, in elastic fluids, as our air." Newton does not say *how much* the Resistance is to be augmented, but simply that it *must be augmented*.

I have gone into this question at some length in *Resistance of Air*, and have shown how, and to what extent, this augmentation can be arrived at. To save repetition, the reader is referred to that small book.

I refer to this point, specially, because it is commonly taught that any Resistance due to the compression of air is insensible until one approaches the velocity of sound : a doctrine which only retards progress.

In conclusion, we have seen that when Newton's Theory of Resistance—including the Resistance due to compression—is examined from the point of view of the Principle of Similitude, it stands the test most satisfactorily, being, in fact, strengthened rather than weakened by the examination.

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CHAPTER XI

RESISTANCE (*continued*)

IN the last Chapter we examined Newton's Theory of Resistance by the application of the Principle of Similitude. It may be said, however, that all this is theoretical: how far will this Theory enable us to *deduce*, logically, the "Formal Laws" derived from experiment? It will be our object now to show how far the Theory can stand this very severe test also, since, if it cannot do that, it must, as a *Physical Theory*, be considered defective.

That great genius, Lionardo da Vinci (1452–1519), said, more than four hundred years ago: "Theory is the general: Practice the soldiers. . . .

"In the study of the sciences which are connected with Mathematics, those who do not consult Nature, but only the authorities, are not the children of Nature; I would say that they are only her grandchildren. She alone is, in fact, the master of real geniuses. But look at the absurdity! We sneer at the man who prefers to learn from Nature herself, rather than from the authorities who are only her pupils."*

Certainly, experiment without *some kind of theory*, however rough, will not lead us very far. The thinker must *arrange*. A science is built with facts, as a house is built with stones; but an accumulation of facts is no more a science, than a heap of stones is a house. When one makes an experiment one is really putting a question to Nature, and should always know, beforehand, *what sort of reply* one wishes to get. The most perfect experiments are those so arranged as to get a simple "yes" or "no" from Nature.

* La Théorie c'est le général: la Pratique ce sont les soldats. . . . Dans l'étude des sciences qui tiennent aux Mathématiques, ceux qui ne consultent pas la Nature, mais les auteurs, ne sont pas les enfants de la Nature. Je dirais qu'ils n'en sont que les petits enfants; elle seule en effet est le maître des vrais génies. Mais voyez la sottise! On se moque d'un homme qui aimera mieux apprendre de la Nature elle-même que des auteurs qui n'en sont que les élèves. (Venturi.)

As Pasteur put it : " All scientific research, in order to be undertaken and followed up with success, should have, as point of departure, a *preconceived idea*, an hypothesis, which we must seek to verify by experiment."

Now, *can* we deduce the " Formal Laws " derived from experiment from Newton's Theory of Resistance ? In a very large number of cases we, undoubtedly, *can*. There are, nevertheless, cases where the attempt has been anything but satisfactory ; but I attribute these failures largely to the want of proper interpretation of the replies given by Nature ; that is to say, to *imperfect framing of the Formal Laws*.

This may appear to be a very strong and very bold statement to make, but I consider it well justified by the facts. In all the cases where experiments are quoted as showing a " Dimensional Effect "—as when the Resistance of plates, say, is measured in a wind-tunnel and the Resistance measured, *per unit of area*, is concluded to be greater on larger plates than on small ones—this is an improper interpretation of the answer given by Nature. Properly corrected, anything like " Dimensional Effect " disappears.

In *Resistance of Air* I have shown that the Resistance of bodies, moving both in air and in water, *can be deduced* from Newton's Theory ; and that the calculated results agree, to an extraordinary degree of accuracy, with the results obtained from experiment.

The fundamental assumptions which I made when calculating these resistances were : (1) That the resistance *due to the viscosity of the medium* was so small as to be negligible. (2) The Principle of Similitude. (3) That the coefficient K, for a square or circular plate, or a cylinder, *obtained from experiment*, is really a *constant* (I did not calculate it ; indeed, in some cases I do not know how it could be calculated). (4) It was also *assumed* that the resistance of the sphere is two-fifths of that of the circumscribing cylinder, all other things being exactly equal ; this also was *assumed*, though the calculation offers no difficulty.

The calculations were made for the resistances of bodies moving in a variety of ways—including amongst others the movement of bodies by means of a whirling machine. The bodies were, of course, not very large, but the speeds varied

from a few feet per second up to over 2,000 ft. per second, in the cases of spherical shot. The latter calculations involved multiplying small constants by *millions*, and in some cases by *thousands of millions*. The shapes of the bodies were simple and regular, being limited to square and circular plates, cylinders of different lengths, and spheres.

These results show, conclusively I think, that Newton's Theory will stand the test of experiment very satisfactorily. Even some "Paradoxes," as they are called, were found to "fall into line" by a proper application of this very fine—and very simple—Theory. This work cannot, of course, be repeated here, but I will refer to certain points, and so extend further what has been said in *Resistance of Air*.

As I have stated previously, I had to be satisfied to take the value of the constant K from experiment, and I *assumed* that a sphere moving in a fluid would experience only two-fifths of the resistance experienced by its circumscribing cylinder, all other things being exactly equal. This latter is not difficult to calculate ; and a reference to Charles Hutton's experiments, recorded in his *Mathematical Tracts*, 1812 (experiments made in 1786 and 1787), shows that this ratio is *strictly accurate!*

Now, if Newton's theory is correct, the value of K (in the Equation, Resistance $= K\rho Sv^2$, where ρ = measure of density, S = area of cross-section, taken perpendicular to the line of motion, and v = the speed), *should be deducible* from it ; or, in other words, the numerical value of K *should be calculable*—and, further, K *should be a constant.*

That K actually is a constant, is, in my opinion, as certain as we can be of anything. In *Resistance of Air* I made this assumption ; and the calculated results, based on this assumption, were found to agree with the experimental results very accurately. Further than this, the constancy of K follows logically from the Principle of Similitude. K is, in fact, a "*coefficient of shape.*"

As regards the calculation of the value of the constant K, one cannot speak so cheerfully. M. Alexandre Sée, in his *Lois expérimentales de l'Aviation*, 1911, devotes several pages to the question of the possibility of making this calculation,

and he sums up: "We will therefore conclude that one *cannot calculate theoretically* the coefficient K."

I am afraid that M. Sée is inclined to be a little too positive in some of his statements. To my mind, this impossibility is by no means self-evident, though I think that there are further experimental data still required to base the calculation. In order to succeed, it would be necessary to know *exactly* how real liquids *move past a circular plate.* A great deal of information on this subject is to be found in my *Motion of Liquids*, based, very largely, on Colonel Duchemin's experiments. We have not, however, all the information we require; we want to know very accurately the *distance in front of the plate* at which *the liquid divides;* this would give

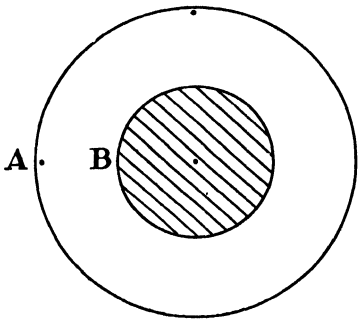

FIG. 5.

the measure of the *rate of transformation of the energy.* This distance is undoubtedly *about* half the breadth of the circular plate, *i.e.*, about a radius distant. Assuming this to be correct, I have calculated the value of K, and obtained a *very good result.* It would be well, however, to have Duchemin's experiments repeated, checked and extended.

The first part of my calculation was to find out the speed of the liquid filaments past the edge of the plate. This can easily be done as follows: Starting from the simplest assumptions, let us suppose the shaded circle (Fig. 5) to represent a circular plate, moving at a uniform speed v, normally to itself, in water *at rest.* It is a matter of the very commonest observation that the plate will disturb the liquid beyond its edge. It is also well known that this disturbance will only

take place for a certain distance from B. Let this distance be represented by the circle A : beyond this, the fluid remains " at rest," and consequently anything outside of A will not enter into our present investigation.

Since the whole of the body of the liquid whose cross-section is represented by the large circle A has to pass through the annular section between the circles A and B ; and since the area of this annular section is certainly smaller than the area of the circle A, it is clear that the fluid must pass through this annular section at a *greater speed* (relatively to the plate) than v, the speed of the plate through the liquid. In other words, the water, originally at rest, will be *accelerated past the plate*, some of its potential energy being transformed into *kinetic* energy.

If we knew the speed of all the filaments of the liquid whose energy has been transformed we should know the *total amount* of energy transformed—which is what we are seeking. Now, experiment has shown (see Duchemin's experiments, in my *Motion of Liquids*) that the radius of the larger circle A is, very approximately, *twice* that of the radius of the smaller circle, representing the plate. Several observers have noted this, but Duchemin is the only one (as far as I know), who ever *measured* it.

Hence, postulating continuity (which is true *in front of the plate*), and incompressibility ; and noting that the area of the annulus is three-quarters of that of the large circle A, it is clear that the *average relative speed* of this part of the fluid past the plate will be *four-thirds* of that of the plate, relatively to the undisturbed fluid. If, therefore, the speed of the plate be v, the average relative speed of the fluid filaments moving past it will be $4v/3$; and the *average actual acceleration* of these filaments will be $v/3$.

Knowing the *average* speed of the filaments will not suffice for measuring the amount of the energy transformed. We know, of course, from the most casual observation, that the speed of the fluid filaments is greatest near the edge of the plate, and that, at A, this speed is zero. It is necessary, however, to know the *law* of this change, or, as it is called, the " speed gradient " from B to A.

Duchemin (see my *Motion of Liquids*) measured this and

found that the gradient was approximately a straight line. In other words, the speed is greatest at B, and diminishes *inversely as the distance* from B to A.

There are, perhaps, theoretical reasons for supposing that this speed gradient is a hyperbola; Colonel Duchemin's experiments (which were perhaps a little rough) showed that the curve did not differ *very materially* from a straight line.

Let us represent this, diagrammatically, in a new figure.

In Fig. 6 let BD represent a section of the circular plate; AB and A'D being sections of the " annular space " round the plate.

Let BC represent the measure of the *maximum increase* of speed of the fluid filaments, which increase becomes *zero* at

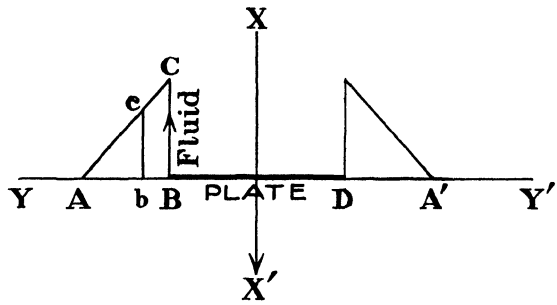

FIG. 6.

A and A', *cb* being the measure of the *average increase* of this relative speed, which we have found to be $v/3$. We do not know yet the relation that *b*B bears to AB.

Now, the total acceleration of all the liquid, per unit of time, may be represented by a solid of revolution whose cross-section is ABC, revolving round the axis XX', and it is not difficult to show that *b*B is 5/12 of AB.*

Knowing now the values of *cb* and *b*B, we find that the value of BC $= 0{\cdot}57\ v$; and, since this is the *increased speed* past the edge of the plate, it follows that the *relative speed* of the fluid filaments past the edge of the plate is $1{\cdot}57\ v$.

Let us now " call a halt " for a moment, in order to take stock of our results. We have *deduced* from Newton's

* In *Motion of Liquids*, I see that this was, rather carelessly, taken as one third of AB.

Theory that the flow of the fluid past the edge of the plate may be measured by 1·57 v; what does experiment say to this?

Duchemin measured this flow for water (see *Motion of Liquids*), and found that the negative pressure at 2 mm. from the edge of the plate was 2·578, the pressure of the fluid at a distance (*i.e.*, the "velocity head" of the fluid) being taken as unity. This shows that the *relative speed* of the water past the edge of the (12 in. square) plate was 1·6 v.

Experimenting with air, Mr. Mallock stated (Inst. Civ. Eng., 1903) that "he had found a negative pressure equal to $2\frac{1}{2}$ times the dynamic head in front of the plate, with a 10-inch disk and the hole of the anemometer less than 1/100 inch from the edge."

This shows that the relative speed of the filaments was $v\sqrt{2\cdot5}$, or 1·58 v. Such close agreement between theory and experiment cannot but be considered as very satisfactory.* It shows that the assumptions made are justifiable.

It may be well to note, however, that in the data derived from experiment, referred to here, the plates were at rest (relatively to the earth, of course), the water, or air, flowing past them; whilst in the assumptions the plate was supposed to be moving in stagnant water. There seems no possible reason why the relative motion should not be the same in both cases. Duchemin made experiments, with water, and found no difference in the relative motion; and there is no apparent reason why what is true for water should not be equally true for air.

It must not be hastily assumed, however, that the *Resistances encountered* would be the same in both cases. This question involves "DuBuat's Paradox," which will be considered in the next chapter.

I have referred to what I call the *imperfect framing of the Formal Laws;* and it is necessary that I should justify this critical language.

(1). The Theory of Fluid Motion which holds the field to-day is purely mathematical, and it is based on the assumption of the *continuity of the fluid.* The equations are commonly

* Had the speed-gradient been taken as a *slightly-concave* curve, instead of a straight line, the agreement would have been even closer.

referred to as " The Equations of Motion," but I suggest that it might, in this case, be more correct to speak of them as the " Equations of Continuity."

(2). From these equations it has been *deduced*—improperly, I maintain—that a " *non-viscous fluid* " would offer no resistance to any body—of any shape—moving in it at uniform speed and in a straight line.

Now, without wishing to make any reflection on the equations, I do maintain that this deduction from them is unsound. The question of Viscosity does not appear to me to enter into the argument. The only really sound deduction that one can draw is that " a *perfectly continuous fluid* would offer no resistance to any body, etc." This, as I have pointed out previously, is really an argument in a vicious circle, the deduction being contained in the premisses.

(3). Assuming (2) to be true, it followed that all the resistance in real fluids was due, *directly* or *indirectly*, to viscosity. This, however, violates the Principle of Similitude, and it also most certainly does not agree with facts. The " perfect " fluid which " offers no resistance " is not *necessarily* only a non-viscous one. It is, however, necessarily a perfectly continuous fluid.

It is hardly necessary to point out that this (2) is diametrically opposed to the teaching of Newton, who said (as the reader may think I am too fond of pointing out), that in an *inviscid* liquid the Resistance that would be experienced by a body moving uniformly in it, would vary as the density of the liquid and as the square of the velocity of the body through the liquid.

Having cut adrift from Newton's teaching, a complicated Theory of " Fluid Friction " has been erected on certain unsound arguments. I confess that I do not understand this Theory, but Lord Rayleigh (March 20th, 1914) explained it, as I have previously quoted: " In a well-designed ship the *whole resistance* (apart from wave-making) may be ascribed to *skin friction*, of the same nature as that which is encountered when the ship is replaced by a thin plate moving edgeways."

" Friction," I suppose, implies " rubbing," but how molecules of a fluid *which do not touch one another*, and which move at the same *finite speed* as that of the molecules which

are adjacent to them, how, I repeat, these molecules *rub against one another*, passes my comprehension. Lord Rayleigh himself admitted that we did not know the meaning of that " very convenient expression," " fluid friction."

This " skin-friction " theory does not appear (as far as I am aware) to be capable of explaining any of the Formal Laws, as well as conflicting with the Principle of Similitude. It is not surprising, therefore, to find authors frankly admitting that they cannot calculate the Resistance of *any body* moving in a liquid or a gas. Therefore, since this Theory does not appear to be competent for *deducing the Formal Laws* derived from experiment, I do not think I am speaking too strongly when I say that it must be pronounced defective.

If we wish to make real progress in Hydromechanics, we must return to Newton's teaching ; there must be what I may call a " Pre-d'Alembert movement." At the present day a good deal of what is called hydrodynamical calculation is little better than what schoolboys call " fudging." For example, if one " calculates " the discharge of water through a 1 in. circular hole in the thin flat wall of a tank with given " heads," one makes the calculation by well-known formulæ, and obtains a result of, say, 100. One *knows, a priori*, that this is 50 or 60 per cent. *too great.* One therefore multiplies the result by what is called a " coefficient of discharge," which we happen to know is about 0·625. We then get a result, 62·5, which is a very fair approximation to the result given by experiment.

I do not call this calculation. Calculation gives 100 ; we know that the *correct answer* is 62·5 ; we therefore multiply $100 \times \cdot 625 = 62 \cdot 5$. That is not calculation.

As regards experiment, probably the chief defect in the interpretation of the results—and in framing " Formal Laws " —is due to the absence of careful discrimination between *uniform motion* and *accelerated motion.* For example, in whirling-machine experiments, the motion is generally assumed to be *uniform*, if the speed of rotation is uniform, and if the length of the whirling-arm is " fairly long."*

The motion, however, is *not uniform*, and the Formal Laws

* See S. P. Langley's classical experiments, and the tangled results produced from them.

deduced from these and similar experiments are defective, in that

(1) The value of K is *always too great*, and
(2) K is not a constant, but increases with the size of the moving plate.

This "Dimensional Effect" (as it is called) was first observed by the Chevalier De Borda. It has been repeatedly noted since, but, up to quite lately, it has been treated rather as a "Paradox." It violates the Principle of Similitude. It is really an increased resistance due to the *accelerated motion*.

If proper corrections are made for the acceleration (see *Resistance of Air*, where this is explained), it will be found that the value of K is *really constant*, and all "Dimensional Effect" disappears. The experimental results then "fall into line," and can then be *deduced from Newton's theory*.

Another example.—In the wind-tunnel experiments the motion of the air is, obviously, an *accelerated motion*. A very small portion of this is taken, however, and treated as if it were uniform motion, as if this motion changed its character when "chopped up small"!

Still another example. In M. Eiffel's famous experiments made on the resistance of plates falling from the Eiffel Tower, the motion of the plates was *an accelerated one*—as M. Eiffel very distinctly tells us. He, however, took the *average speed* for a very small interval of Time, and *treated it as if it were uniform*. Is that logical?

In the results which he obtained there was a very marked "Dimensional Effect"; K *was not constant*. This Dimensional Effect was due to the acceleration of the motion of the plates.

But a very similar set of experiments was carried out, at about the same time, and at the same place, by MM. Cailletet and Colardeau. The apparatus was designed by M. Eiffel, who also superintended the work. In these experiments the plates were accelerated by *small weights*, so that they acquired "terminal velocities"—which were, of course, uniform. The Resistances were only taken after the plates had assumed these "terminal velocities."

No "Dimensional Effect" was observed in these experi-

ments; K *was a constant.* This absence of Dimensional Effect was specially noted and referred to by M. Eiffel himself.

As previously stated, in all wind-tunnel experiments, the motion of the air is an *accelerated one;* but it is always referred to as a " uniform motion." It is clear that the speed of the air is either *increasing* or *decreasing*, from one end of the tunnel to the other, according to whether the wind is being *drawn through* the tunnel or *driven through it.* " Dimensional Effects " are, as one would expect, observable in nearly all the experiments carried out in the tunnels.

It follows, therefore, that all the " Formal Laws " based on such experiments, are vitiated by this assumption of uniform motion. The Principle of Similitude shows that for *uniform motion* there should be no " Dimensional Effect "; and that K *should be a constant.*

In all experiments made with pendula swinging in resisting media, it is always found necessary to take account of what is called the " added mass." This is not to be interpreted as meaning that the *actual mass* of the pendulum-bob is increased in any particular ratio. The " added mass " bears no necessary relation to the mass of the bob itself : it is a certain fraction of *the mass of the fluid displaced by the bob.*

Sir George Stokes found, theoretically, that this " added mass " was *half of the mass of the fluid displaced by the bob,* and was consequently a *constant* coefficient. On the other hand, DuBuat (who was the first to draw attention to this " added mass "), working experimentally, found that it was about 0·6 of the mass of the fluid displaced, and that this fraction was *not constant.* The extra resistance is, here again, due to the accelerated motion of the pendulum-bob. I have discussed these questions at considerable length in *Resistance of Air,* to which the reader is referred for fuller details.

Another defect in the deductions made from wind-tunnel experiments is due to the air being treated as if it were incompressible : or rather as if the error caused by assuming incompressibility were negligible—*except at speeds approaching that of sound.*

This assumption is based (I believe) on a formula first enunciated by Daniel Bernoulli, where the increased Resist-

ance caused by the compression of the Fluid was assumed to depend on the velocity of sound; and is given as of the order of v^2/V^2, where V is the speed of sound, and v is the relative speed of the moving body. The increased Resistance, however, as was pointed out by Duchemin, is much greater than this, being of the order of v/V—where V = speed of the flow of the gas into what we may consider the relative vacuum behind the moving body.

Duchemin pointed out a very curious fact (also referred to in my *Resistance of Air*), which is that the fraction v/V can attain, but can *never exceed*, unity. That is to say that however much v may exceed V, the increased Resistance can *never exceed* 100 *per cent.* The effect of this is *as if* the density of the fluid medium was increased in the ratio of $(V + v)/V$.

For example, if the density of the medium was ρ, the quantity

$$\rho\left(\frac{V+v}{V}\right)$$

would be inserted into the equation, instead of ρ only. At a speed of 2,000 fs., where v exceeds V, the density of the fluid would be taken as 2ρ. I hope this is clear.

Hutton's experiments with air, recorded in his *Mathematical Tracts*, support this view very strongly. Hutton's very valuable work on the Resistances of Bodies moving in still air (whirling-machine experiments) has, I may say, been sadly neglected. I do not think that any finer or more careful work has ever been done by anyone.

I would finally refer, with deep respect and with profound regret, to an unfortunate slip on Newton's part, in his immortal *Principia*. I do not imply any defect in the *Theory of Resistance*. It is nothing more than a slip made in the *deductions* from this Theory. The slip is, indeed, a serious one, as it confuses Momentum and Resistance. I am not aware that anyone has drawn attention to it before, though it is quite evident when pointed out.

In Lemmas V, VI and VII, Book II, Newton says:

"Lemma V

"If a cylinder, a sphere and a spheroid of equal breadths be placed successively in the middle of a cylindric canal, so

that their axes may coincide with the axis of the canal; these bodies will *equally hinder* the flow of water thro' the canal."

This is quite evident, since the water flows in all the cases "thro' equal spaces"—*i.e.*, through *equal apertures*. One may say, therefore, that the *momentum generated* is equal in all the three cases.

"Lemma VI

"The same supposition remaining, the forementioned bodies are *equally acted on* by the water flowing thro' the canal."

The slip is involved in this Lemma, since, although the

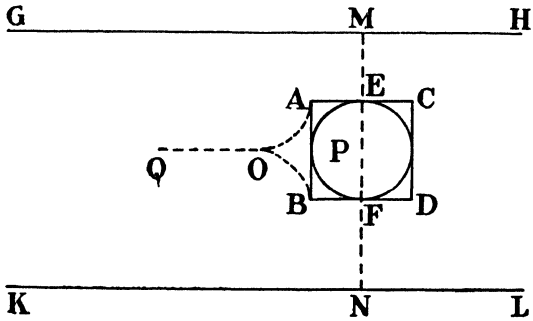

FIG. 7.

momentum generated (and Newton, of course, deals only with momentum) is equal in all the three cases, it is not a logical sequence that the *Resistance* (or "Force," as some might call it) is *also equal* in all three cases. Everything depends (as I have pointed out in Chap. VI) on the condition of *Time*; in other words, the Resistance depends on the amount of Momentum generated *in a given Time*. It depends, therefore, on the Time *during which the total Momentum* is generated.

At the risk of repetition, I may say that (as we should put it now) the Resistance varies inversely as *the distance through which the energy* is transformed. Let me give an example, in order to put my meaning beyond doubt.

In Fig. 7 let GH and KL represent the inner surfaces of the "cylindric canal"; ABDC the cylinder, with the inscribed

sphere; ME and FN sections of the "aperture," which is the same for both of the bodies.

In the case of the cylinder we know from experiment that the fluid "divides" at a point O, which is *about* the radius of the cylinder from the face AB. That is to say that the generation of the momentum *commences at O*. The momentum is therefore generated during the time that the Fluid takes to travel a length OP.

In the case of the sphere, however, the Fluid "divides" at a point Q, such that QP is about 2·5 times OP. Taking OP as unity, therefore, the *Resistance* of the sphere

$$= \frac{\textit{Resistance of the cylinder}}{2{\cdot}5}$$
$$= 0{\cdot}4 \text{ of that of the cylinder.}$$

This, I hope, the reader will find a simple and satisfactory illustration of my argument.

"Lemma VII

"If the water be at rest in the canal, and the bodies move *with equal velocity* and in contrary ways [*in partes contrarias*] thro' the canal, *their resistances will be equal* among themselves."

It is not easy to understand how Newton could have made this slip; since we know from many other parts of the *Principia* that he was perfectly well aware that the resistances *would not be equal*. For example, in Prop. 34—only 3 *propositions earlier!*—he treats of the resistance of a globe, which he shows is only *one half* of that of the circumscribing cylinder. It is true that this is in his imaginary "rare medium," which does not resemble ordinary liquids. But in the Scholium following this same Prop. 34, he says: "By the same method *other figures* may be compared together *as to their resistance;* and those *may be found* which are *most apt to continue their motions in resisting mediums.*"

That Newton was here thinking of *real media* is further evident from the concluding words of this Scholium: "which proposition I conceive *may be of use in ship-building.*" These and other remarks would be meaningless if the statements in the Lemmas, just quoted, were correct.

These Propositions 36 and 37 have always been considered the least convincing of any in the *Principia*. In the cases enunciated in these Lemmas it is, of course, true that (dynamically) the *total alteration of momentum* is the same in all three cases: and therefore according to the second Law of Motion—*as Newton worded it*—the "Impressed Force" (or, in this case, Resistance) is the same. Resistance, however, is not measured by the *amount of the momentum* altered—but by the *Rate at which it is altered, i.e.*, the alteration *in a given time.*

Again, thinking now "energetically," the amount of *energy transformed* is the same in all three cases. Resistance, however, is measured by the *Rate* at which the Energy is transformed, *i.e.* (paraphrasing Newton's own language), *in a given distance*, and this rate is *by no means the same* in each case.

We have, in brief,

$$\text{Resistance} = \frac{\text{Momentum altered}}{\text{Time Interval}}, \quad \textit{Dynamically,}$$

$$\text{Resistance} = \frac{\text{Energy transformed}}{\text{Distance}}. \quad \textit{Energetically,}$$

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CHAPTER XII

DUBUAT'S PARADOX

HYDROMECHANICS is full of "Paradoxes." Possibly the oldest, as well as one of the most famous of these, is "The Hydrostatic Paradox"; of which Pascal said: "and thus it appears that a vessel full of water is a new principle of Mechanics, and a new machine which will multiply Force to any degree we choose." "Pascal's Paradox" was referred to in my *Resistance of Air;* and "d'Alembert's Paradox" has been referred to here in Chapter X.

There is one Paradox, however, which I must now specially discuss, since the question involved being a *fundamental* one it becomes of the very highest importance; this is the well-known "DuBuat's Paradox."

But first, what do we understand by a "Paradox"? The dictionary definition is: "A proposition contrary to received opinion: one seemingly absurd, yet really true."

A witty writer has described a Paradox as a truth standing on its head, in order to attract attention. This is, in reality, a very good definition, which accords with the dictionary meaning whilst accentuating it. Paradoxes are certainly amongst the most interesting things in Science, since they point to the fact that there is some flaw in "received opinion." Paradoxes are, in fact, the salt of Science. Hydromechanics is full of them; and that is what makes it so fascinating a subject of study.

The late Philip E. B. Jourdain, in his amusing and sarcastic little book, *The Philosophy of B*rtr*nd R*ss*ll*, 1918, says:

"A 'paradoxical attitude' is, of course, the assertion of one, or more, propositions of which the truth cannot be perceived by a philosopher—and particularly an idealist—and can be perceived by a logician, and occasionally, but not always, by a man of common sense. . . .

" Nearly all Mathematicians agreed that the way to solve these paradoxes was simply not to mention them ; but there was some divergence of opinion as to how they were to be unmentioned. It was clearly unsatisfactory, merely not to mention them. Thus Poincaré was apparently of opinion that the best way of avoiding such awkward subjects was to mention that they were not to be mentioned."

The essence of DuBuat's Paradox is that the Resistance encountered by a square plate, say, in motion of rectilinear and uniform translation through a fluid, is *less* than the Resistance which the same plate would experience when fixed, and the fluid was flowing uniformly past it *at the same relative speed.* In other words, if in the first case the speed of the plate in the stagnant fluid is v, and the Resistance to its motion is R ; whilst in the second case the speed of the fluid is also v, whilst the Resistance is R^1 : then $R^1 > R$.

The Chevalier DuBuat discovered this, experimentally, towards the end of the eighteenth century. In the earlier part of the nineteenth century, Colonel N. V. Duchemin (French Royal Artillery) repeated DuBuat's experiments and obtained exactly similar results. Neither of these distinguished men offered any explanation of these curious results, and the discovery has ever since been referred to as " DuBuat's Paradox."

This difference in the resistances has indeed been noticed by other observers, but it has then usually been explained as due to " experimental error " ; while, as a possible fact, it has been dismissed as *absurd.*

In my *Motion of Liquids,* I gave Duchemin's experiments in some detail and discussed the question at some length, offering an explanation of the Paradox. I have seen no reason to be dissatisfied with that explanation, though I think it might be stated in a somewhat different, and perhaps more convincing manner.

Blaise Pascal says, in his *Pensées:* " When one wishes to correct usefully, and to show another person that he is mistaken, one must observe from which side he looks at the matter, for it is commonly true from that point of view, and admit this truth to him, but also explain the point of view from which it is false. He is satisfied with that, for he sees

that he was not mistaken but that he only failed to see all sides. Now, one does not get annoyed at not seeing everything, but one resents having been mistaken."*

This advice may appear a little "Jesuitical" but it is good, nevertheless. Let us, therefore, examine the side from which the objectors look at the Paradox, since (as Pascal says), their objection or denial may well be true from that point of view. The objectors consider the question from the point of view of "Relativity." They argue that all motion is Relative, and that consequently *the motion* must be *exactly similar in both cases.* I grant this as true; though perhaps not *exactly* true, as I will explain a little later.

"This is the Principle of Relative Motion, which imposes itself upon us for two reasons: firstly, the very commonest experience confirms it, and further, the contrary hypothesis would be repugnant to the intellect." (H. Poincaré, *La Science et l'Hypothèse,* 1912.) This is undisputed and indisputable, but the same author also says, in his *Dernières Pensées,* 1913:

"The *differential equations* are not altered when one changes the axes, but *it is not the same* for the *finite equations;* a change of axes would oblige us, in fact, to *change the constants of integration.* The Principle of Relativity *does not apply* to the *finite equations* directly *observed,* but only to the *differential equations.*

"We do not observe, directly, the differential equations; what we do observe are the finite equations, which are the immediate translation of the observable phenomena, and from these the differential are deduced by differentiation."†

* Quand on veut reprendre avec utilité, et montrer à un autre qu'il se trompe, il faut observer par quel côté il envisage la chose, car elle est vraie ordinairement de ce côté-là, et lui avouer cette vérité, mais lui découvrir le côté par où elle est fausse. Il se contente de cela, car il voit qu'il ne se trompait pas, et qu'il manquait seulement à voir tous les côtés. Or, on ne se fâche pas de ne pas tout voir, mais on ne veut pas s'être trompé.

† Les équations différentielles ne sont pas altérées quand on fait un changement d'axes, mais il n'en est pas de même des équations finies; le changement d'axes nous obligerait en effet à changer les constantes d'intégration. Le principe de relativité ne s'applique donc pas aux équations finies directement observées, mais aux équations différentielles.

Nous n'observons pas directement les équations différentielles; ce que nous observons, ce sont les équations finies, qui sont la traduction immédiate des phénomènes observables, et d'où les équations différentielles se déduisent par différentiation.

We see, therefore, that those Physicists who are opposed to DuBuat's Paradox are right when they say that *the motion*, being relative, must be exactly similar, whether the body is fixed and the fluid flows past it, or whether it move in a stagnant fluid *at the same relative speed.* In other words, if the relative speed be v, then dv/dt is unchanged when the axes to which it is referred are changed.

Even if they go further and, arguing purely "dynamically," say that the *momentum changed* is exactly equal in both cases, this, also, *cannot be disputed.* But, as I have said more than once (I hope the reader will not think *ad nauseam*), the Conservation of Momentum, though " true," is true only in the Mathematical fairyland of Pure Rigid Dynamics, and not outside it. When we are dealing with *real bodies*, when we start experimenting, when we have to argue " Energetically " and not " Dynamically," then the Conservation of Momentum is *never true.*

A French writer, M. Alexandre Sée, however (*Les Lois Expérimentales de l'Aviation*, 1911), goes a good deal further than this, for he says :

" It is a question of determining the *action* [*i.e.*, the Pressure—Newton's " Action "] of the air at rest on a body animated by a rectilinear and uniform motion of translation or, *what is the same thing*, the action of a constant current of air on a motionless solid.

" In virtue of the Principle of Relative Motion *these two cases are identical* and the results *should be the same ;* mere facilities for experimenting should induce us to select either the one or the other of these dispositions."

He adds, in a note, " Nevertheless, there have been experimenters [obviously referring to the Chevalier DuBuat and Colonel Duchemin] who have not recognized this Principle, who have thought it necessary to make experiments in both senses, and who, wonderful to relate (*chose admirable*) *have obtained entirely different results.*"

I am afraid that this very positive and dogmatic statement is thoroughly illogical. It calls up memories of some of the logic of our old friends Humpty-Dumpty and Tweedle-dee. Although we all accept the Principle of Relative Motion, we cannot deduce from it that *these two cases are identical.* As

a matter of fact they are *not*. If the two cases were "identical," identical results would be expected, and would in all probability be obtained. But:

(1) In the first case the fluid is a *static* fluid, and the relative motion of the plate is *uniform*—that is to say, it is not an *accelerated* one.

(2) In the second case, the fluid is *not a static fluid*, and its relative motion is *an accelerated one;* that is, it is not uniform.

In order to make the two cases "identical," it would be necessary to imagine the fluid enclosed in a large box, which should move uniformly past the plate at the same relative speed as the body in the stagnant fluid. In such a case, we should expect, and would probably get, exactly similar results.

In the cases (1) and (2) we ought *not* to expect to get similar results; and experiment has shown that as a matter of fact we *do not*.

Up to the time of the Chevalier DuBuat—may I call this the "Pre-Rowan Hamilton" age?—indeed one might almost say up to the present day, the view expressed by M. Sée was considered an *axiom*. Referring to this, DuBuat himself said: "This Principle, *without being proved*, seemed, at least, *so probable* that no one doubted it, and no one had ever tried *to verify it* by any experiment; but one risks being mistaken when one applies to fluids the Laws of Motion which are appropriate to [Rigid] solids."

He tried the experiment and found very different results in the two cases.

Colonel Duchemin, in conducting his experiments, was guided by the programme proposed by the *Académie des Sciences*, which stated that experiments should be made "when the body is exposed to the shock of the fluid in motion, and when it moves in the same fluid at rest." Thus the *Académie* apparently did not consider it obvious that the two cases are *necessarily* "identical." Besides, as Poincaré remarked: "We should never disdain to make a verification when the opportunity occurs."

Both DuBuat and Duchemin were fine experimenters; but before them, at the latter part of the eighteenth century, that able observer, the Abbé Bossut, also knew perfectly

well that the resistances in these two cases *were not the same.* He did not theorize on the subject; he simply tried the experiment and observed the results!

Duchemin in any case did well to repeat the experiment, *even though he may have been pretty sure that he would get a negative result.* M. Sée's sneer is, indeed, one feels, both unjust and ungenerous, as well as being inconsistent with the true scientific spirit.

DuBuat and Duchemin observed the "finite equations," and, as previously stated, the Principle of Relativity *does not apply to these:* the results are not *necessarily* the same for any change of axes. Let me explain my meaning a little more. If the question of Resistance be examined "Dynamically," *i.e.*, as a question of "Kinematics," we should have:

$$\text{Force} \times \text{Time} = \text{M}\int dv.$$

Integrating this between $v = v$, and $v = 0$,

$$\text{Force} \times \text{Time} = \text{M}v.$$

Or, if we integrate between $u + v$ and u,

we get $$\text{M}\int_{u}^{u+v} dv = \text{M}\{(u+v)-u\}$$

or, again, $\text{M}v$.

Suppose, however, that we employ "Energetics" (which is the correct and only rational way of solving the problem) We now have:

$$\text{Force} \times \text{Distance} = \text{M}\int v\,.\,dv = \text{M}\frac{v^2}{2} + \text{C}$$

Integrating this between $v = v$, and $v = 0$,

$$\text{Force} \times \text{Distance} = \text{M}\frac{v^2}{2}$$

But, between the limits $u + v$ and u,

$$\text{Force} \times \text{Distance} = \text{M}\left[\frac{(u+v)^2}{2} - \frac{u^2}{2}\right] = \text{M}\left(\frac{v^2}{2} + uv\right),$$

which is, obviously, *greater* than $\text{M}\frac{v^2}{2}$.

The " Action," or " Effort," or " Resistance," *per unit of distance,* which is the measure of the work done, will clearly be *greater* in the latter case than in the former, although the *Relative Motion*—the acceleration, the dv/dt—is *the same in both cases!*

We thus see that M. Sée, and those who agree with him, are correct in affirming that the *Relative Motion* is the same and also that the *amount of momentum* changed is the same. The point of view, however, from which they have *not* looked at the question is that of *experiment;* they have not examined it from the point of view of " Energetics."

The problem, in short, is in no way a problem in " Pure Rigid Dynamics," but a problem in " Energetics " where " Conservation of Momentum " is *never true.*

Nearly two and one half centuries ago, Leibnitz, in his *Short Demonstration of a Notable Error of Cartesius and others touching a Law of Nature according to which they hold that the Quantity of Motion is ever conserved the same by the Deity; which they also misuse in Mechanics* (*Acta Eruditorum,* 1686), very elegantly proved that when dealing with *real bodies* the Conservation of Momentum was not true ; but what *was* conserved was the *Vis viva.*

In view of this fact, which seems sufficiently obvious, it is extraordinary how often in Physical problems we meet with arguments which only tend to confuse the subject. For example, one of our most eminent Physicists, Sir J. J. Thomson, in his *Adamson Lecture,* in Manchester, on the 4th November, 1907, said : " If the Principle of the Equality of Action and Reaction is true the Conservation of Energy holds, whatever axes we use to measure our velocities, but if Action and Reaction are not equal and opposite the Principle [? Conservation of Energy] will only hold when the velocities are measured with reference to a particular set of axes.

" The Principle of Action and Reaction is thus one of the *foundations* of Mechanics, and a system in which this Principle did not hold would be one whose behaviour could not be imitated by any Mechanical model."

But how can this be questioned? If Action were not equal to Reaction the universe would not be stable. The

" system " would move of itself. Sir Joseph, however, does question it, since he continues : " The study of Electricity, however, makes us acquainted with cases where the Action is apparently *not equal to the Reaction.* Take, for example, the case of two electrified bodies A and B in rapid motion, we can, from the laws of electricity, calculate the forces which they exert on each other, and we find that, except in the case when they are moving with the same speed and in the same direction, the force which A exerts on B is *not equal and opposite* to that which B exerts on A, *so that the momentum* of the system formed by B and A *does not remain constant.* Are we to conclude from this result that bodies when electrified are not subject to the Third Law, and that therefore any Mechanical explanation of the forces due to such bodies is impossible ; this would mean giving up the hope of regarding electrical phenomena as arising from the properties of *Matter and Motion.*" [Emphasis, in all cases, mine.—R. de V.]

Sir Joseph here states definitely that there are cases where, *apparently,* (thus safe-guarding himself) the Third Law of Motion—which he has previously said is " one of the *foundations* of Mechanics "—is *not true ;* and, in explanation, he adduces the fact of two independent electrified bodies, and says that (except under very restricted conditions) the Action *is not equal to the Reaction.*

With all due respect to his great Authority, I would submit that the deduction he makes from his experiments is not justified. All that he has a right to assert is that the Third Law *and* the Conservation of Momentum are *not both true.* Sir Joseph is here, of course, speaking as a Physicist, and in reference to *real bodies*—not the imaginary bodies of Pure Dynamics. He *assumes* that the Conservation of Momentum is fundamentally true ; it is for him, apparently, almost sacrosanct ! Consequently, one of his " foundations of Mechanics " has (if necessary) to be thrown overboard, since the Momentum, etc., *does not remain constant !*

Arguing in the same manner, we may equally say that, since " Action and Reaction " is one of the " *foundations* of Mechanics," *the Conservation of Momentum is not true* (as I have pointed out almost *ad nauseam*).

Sir Joseph having admittedly got into a very unpleasant situation proposes to get out of it as follows:

"Fortunately, however, it is not necessary. We can follow a famous precedent and *call into existence a new world* to supply the deficiencies of the old. We may *suppose* that connected with A and B there is another system, which though invisible possesses Mass and is *therefore able to store up Momentum*, so that when the Momentum of the system A, B, alters, the Momentum which has been lost by A and has not gone to B has been *stored up* in the invisible system with which they are in connection, and that A and B, *plus the invisible system*, together form a system which obeys the ordinary laws of Mechanics and whose *Momentum is constant* . . . and we may infer from it that when we have a system whose Momentum *does not remain constant*, the conclusion we should draw is not that Newton's Third Law fails, but that our system is connected with another system which *can store up the Momentum* lost by the primary, and that the motion of the complete system is in accordance with the ordinary laws of Dynamics.

"Returning to the case of the electrified bodies *we see* that these *must be connected* with some invisible universe, which we may call Ether, and that this Ether must possess Mass and be *set in motion* when the electrified bodies are moved."

The logic of all this is not quite self-evident. It is clear, however, that Sir Joseph treats the Conservation of Momentum (in Physics) as an "Article of Faith"; consequently, he finds it necessary to create "a new world," to supply the deficiencies of the old, which experiment has pointed out. But even then how "Momentum" can be *stored* is anything but clear. Momentum—*i.e.*, matter in motion—is *a state*, a condition; and how "a condition" *can be stored up* is not explained. All this appears to be sheer Metaphysics, which Physicists should avoid.

We see clearly, however, the difference between this teaching and that of Newton, who says that Motion is *not conserved;* his exact words are, "'tis *very certain* that there is not always *the same quantity of motion in the world.** Sir

* *Opticks*, 3rd edition, p. 373.

Joseph, on the contrary, says that *there is.* He explains this by telling us that: "The electrified body has thus associated with it *an ethereal or astral body*, which it has to *carry along with it* as it moves and which *increases its apparent mass.* Now this piece of the unseen universe which the charged body carries along with it may be expected to have *very different properties* from ordinary matter: it would, *of course,* defy chemical analysis, and probably would not be subject to gravitational attraction; it is thus a very interesting problem, etc." For further details, I must refer the reader to the original lecture.

One thing certainly appears to me to be established: we may believe in either (1) The Conservation of Momentum (as in Rigid Dynamics), or (2) The Conservation of Energy (as in Energetics). It is impossible to believe in *both at the same time!*

Although on this question of relative actions nearly all modern experimenters, both in France and England, take the same view as M. Sée, there are a few exceptions. As far back as 1860, General Morin, *Notions fondamentales de Mécanique,* though opposed to DuBuat's Paradox, admitted that Duchemin's experiments *ought to be verified;* and, also, that they were in agreement *with the experiments of Thibault, on Air.**

Again, M. Armand de Gramont, Duc de Guiche, *Essai d'Aérodynamique du Plan* (Vol. I., 1911) referring to this question, says: "This identification has always appeared to me doubtful: for, *a priori,* summary reflections on the resistance of air had led one to think that *there should be differences* between the two cases, which one could not neglect." All M. de Gramont's experiments were carried out on plates moving in still air: the results were *not* identical with those obtained by M. Eiffel, whose plates were at rest, with the air moving past them.

Later M. Maurice Gandillot (ancien élève de l'École Polytechnique), in his *Note sur une Illusion de Relativité,* Gauthier Villars, 1913, goes a good deal farther than any of

* Thibault's book is very scarce. It took me over twelve years to obtain a copy. The only public copy that I know of this talented young experimenter's extremely interesting book is in the *Bibliothèque Nationale* of Paris.

these, for he says: "It is generally considered that the experiments of Duchemin are *paradoxical, because they contradict the postulate of Kinematic substitutions,* which is considered above *all discussion.*"

He further points out that: "The postulate that you admit [Kinematic substitutions] is perfectly correct *so long as one does not leave the domain of Pure Kinematics.*" M. Gandillot might, even, have added "or Pure Rigid Dynamics" (the science of "Momentum"), where Momentum *is* conserved.

Still quoting from this interesting pamphlet: "Kinematic substitution is perfectly correct if, in the case you are considering, the relative motion of the disc and the fluid is *exactly similar* [*demeure en entier le même*] *in the different circumstances.*

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"Unfortunately, so long as scientists retain this sort of faith which the postulate of Kinematic substitution inspires in them, they will have great difficulty in advancing [*à faire progresser*] the Dynamics of fluids, as well as in understanding the causes of certain natural facts, amongst which we must probably reckon the ease with which birds (and perhaps also fishes) turn themselves in their fluid element.

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"The convenient hypothesis of rigidity, the consideration of the invariable solid, occupy a large place in theoretical Mechanics.

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"It is to be feared that progress [in the Dynamics of Fluids] will be slow, since often, in order to study fluidity, we employ methods of reasoning which *amount to denying it* at the outset."

To prevent there being any mistake, M. Gandillot further definitely says: "The resistance which a disc animated by an *orthogonal* speed v experiences in passing through a fluid medium at rest, *is not necessarily equal* to the resistance which it offers to the stream when, being fixed, it is surrounded by the fluid flowing at the speed v."

The whole of this pamphlet is most interesting. It should be studied in the original. It is refreshing to read the work

of a man who has really rational ideas on Mechanics as applied to ordinary experience. M. Gandillot, I may add, is the first author that I have come across who divides Mechanics into " Vector " and " Scalar." He even distinguishes between " Vector Energy " (*vis viva*) and " Scalar " or " Algebraic," Energy, and views questions both " Kinematically " and " Energetically." He is thoroughly acquainted with Sir William Rowan Hamilton's work on this subject, and recognizes that, in Mechanics, all " quantities " can be divided into " Vector "—or " directed," or, shall I say, " Trigonometrical " quantities—and "Scalar," or " non-directed," or " Algebraical " quantities ; the two being *essentially different*, so that it is impossible *to change one into the other*—and hence, also, it is *impossible to equate them.*

In this pamphlet there is also a reference to another important " Paradox," which, I do not think, has ever received a name, as it is not referred to by writers on Mechanics. It is, however, another *mis*application of the Principle of Relative Motion—another "*Illusion de Relativité.*" Thus, it is well known that " The Motion of the centre of Mass of a system is not affected by the mutual action of the parts of the system " [Maxwell, *Matter and Motion*]. For, as Maxwell continues : " The Momentum generated in B by the action of A, during an interval, is equal and opposite to that generated in A by the reaction of B during the same interval, and *the motion of the centre of Mass* of A and B is, therefore, *not affected* by their mutual action " ; and from this it is deduced that the same " Force " would be required to move the system, whether A and B were in relative motion or not.

This is Pure Dynamics, and the truth of the abstract statement cannot be questioned. Where the " Illusion of Relativity " comes in is when it is *assumed*, as it generally is, that this is equally true for " real bodies " ; that, in other words, it would require an equal *expenditure of Energy* to move a system whether the different parts of the system were in relative motion to one another or not—always presuming that this relative motion *did not alter the position of the centre of Mass.*

This is very commonly believed ; but M. Gandillot shows that ordinarily it requires more power to move the system when the parts of the system are in motion than when they are at rest relatively to one another.

When I say this is " commonly believed," I mean that it is believed *theoretically*, and by those who study Pure Dynamics, as taught ; it is the sort of statement that would be made in an examination paper. *Practically*, the same student would know that such is *not the case*. Any man who is racing, in a carriage, a rowing, or sailing boat, or in a motor car, knows the great importance of keeping still in the vehicle.

In the carriage, he would probably say that moving about " puts the horse out of his stride." In the case of the boat, that the movement " spoiled the trim of the boat." What he would say in the case of the motor car, I do not know ; but, in any case, the man knows *practically* that *more power is required* to propel the vehicle when the passengers are moving than when they keep quiet ; and he expresses this fact in different ways.

Very many other examples of " Illusions of Relativity " might be given, but these must suffice. They are nearly all variations of these two tunes ; sometimes in one key, and sometimes in another.

I have said that the " Relative Motion " of the plate and the fluid is not *exactly* similar in both cases. If the plate moves in fluid at rest the vortices are as shown—equally balanced (Fig. 9). If the fluid flows past the fixed plate the vortices are produced, as I might say, *alternately—i.e.*, Right, Left, Right, Left, etc. (Fig. 8).

I have said " alternately, Right, Left, etc.," but this is when viewed in two dimensions only. If viewed in three dimensions the vortices are shown as " spirals " as Mr. A. Mallock has shown.* This is a necessary consequence of the fluid " spiralling " before it reaches the plate.

In nearly all the published photographs the vortices are shown as in Fig. 8 ; but in these cases the fluid was always flowing past a *fixed plate*. Lord Rayleigh, on the other

* Mr. Mallock's figure was reproduced, by kind permission, in my *Resistance of Air*.

hand, who was fond of showing these vortices at the lecture table, had his fluid at rest, with some sulphur powder sprinkled over its surface. The vortices he showed were strictly in accord with Fig. 9.

I cannot pursue this subject further here, especially as I have dealt already with it at some length in *Resistance of Air*, where Mr. Mallock's fine work on this subject is fully reported, and his diagrams are also reproduced. There is also some good work on this subject to be found in Riabouchinsky's publications (*Aéronautique de Koutchino*).

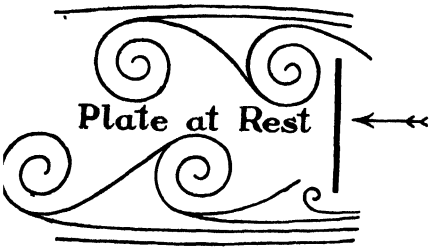

FIG. 8.

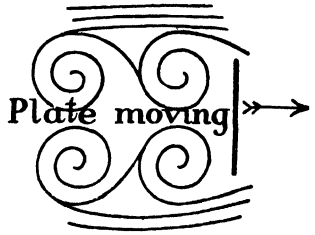

FIG. 9.

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CHAPTER XIII

THE PRINCIPLE OF LEAST ACTION

THE " Principle of Least Action," although by no means new, is still very imperfectly understood; notwithstanding that it is, after the " Principle of the Conservation of Energy," the *most important Principle* in Physical Mechanics. The doctrine underlying the Principle is that all motion, in Nature, takes place along the line of least Resistance, least Work, or least Transformation of Energy. This is indicated (shall we say " postulated " ?) in Newton's first of his *Rules of Reasoning in Philosophy.*

" *We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearance.*

" To this purpose the philosophers say that Nature *do's nothing in vain,* and more is in vain, when *less will serve;* for Nature is pleas'd with simplicity, and affects not the pomp of superfluous causes." (Book III.)

This axiom is perhaps Metaphysical; but our common observation of natural phenomena, as well as our experiments, show that Nature *does* in fact produce motion, or transformation of Energy, along that path where the least Work is done, or the least Energy is transformed, *in a given time.*

Such being the fact, it follows that if we know the relation between the amount of Energy transformed and the various paths taken by a body, we can, in any given case, work backwards and, by reducing the Energy transformed to the *minimum,* arrive at the *actual path* which the body will take.

Further, since the Work done, or Energy transformed during motion along this line, is a *minimum,* it follows that the motion along *any other line* (however near to it) would necessitate the transformation of a *somewhat greater* quantity of Energy; so that the body would tend to move back to

the "minimum path." Consequently, motion along the line of "Least Work," so to call it, will be the only form of motion which can be *stable*.

Attentive reading of the *Principia* will show that Newton recognized this Principle, though he, unfortunately, never got nearer formulating it than his above First Rule and comment.

Having thus roughly sketched the general idea, it will be useful to trace this famous Principle historically. We commence with Maupertuis, whose name it commonly bears, since he appears to have been the first to point it out definitely.

Pierre Louis Moreau de Maupertuis was born at Saint Malo in 1698, and died at Bâle in 1759. He was one of the first French Newtonians. His teleological tendencies appear to have shown themselves early in his career, when he speculated as to what ground the Creator could have had for preferring the law of the inverse square to all other possible laws of Attraction. Some of his writings were ruthlessly and rather unjustly turned to ridicule by Voltaire. This may, perhaps, account for Maupertuis' name ranking lower among scientists than he appears to deserve.

On the 15th April, 1744, he enunciated a Principle, which he called the *Principle of Least Action*, in a memoir read in the French Academy of Sciences,* entitled, *Accord de différentes Loix de la Nature qui avoient jusqu'ici paru incompatibles*. His Principle had, undoubtedly, a theological foundation, for Maupertuis declared it to be one which eminently accorded with the wisdom of the Creator: he calls it "*Principe si sage, si digne de l'Etre Suprême.*"

In this first paper he took for the measure of his "Action" the product of the velocity and the space described, or *vs*. It is not clear why he adopted this measure, since *vs* is not what is usually accepted as the meaning of "Action." There can, however, be no doubt as to what he meant by "Action," since he says: "I must now explain what I mean by the quantity of Action. A certain Action is necessary for the carrying of a body from one point to another; this Action depends on the velocity which the

* Histoire de l'Académie Royale des Sciences: Année 1744 (Paris 1748).

body has and the space which it describes ; but it is neither the velocity nor the space taken separately. The quantity of Action varies directly as the velocity and the length of the path described ; it is proportional to the sum of the spaces, each being multiplied by the velocity with which the body describes it. It is this quantity of Action which is here the true expenditure [*dépense*] of Nature."

Here, in this general description, there is no special mention of Mass. But in a later memoir, in the *Histoire de l'Académie de Berlin*, of 1746, Maupertuis says :

" When some change happens in Nature, the quantity of action necessary for this change is the *smallest possible*. . . . The *quantity of Action* is the product of the Mass of the bodies by their velocity and the space which they describe. When a body is transported from one place to another, the Action is greater in proportion as the Mass is greater, as the velocity is greater and as the path by which it is transported is longer." Here he very distinctly includes Mass.

It has been questioned whether Maupertuis really understood his own Principle, for at times he appears to use his word " Action " in different senses. In fact, d'Arcy hinted that " Maupertuis merely chose his formula for the ' Action ' so that, when minimized, it gave the results he wanted. In fact, he did dishonestly, pretentiously, and unskilfully, what Euler did honestly, humbly, and skilfully." [I quote from the very valuable monograph of Philip E. B. Jourdain, *Principle of Least Action*.]

In the *Histoire de l'Académie de Berlin*, 1752, in replying to d'Arcy—who maintained (it would appear correctly) that *mvs* is *not action*—Maupertuis said that he had only *adopted Leibnitz's definition*. His words are : " Mais pour trancher court avec M. d'Arcy, je puis dire que ce n'est pas mon affaire. Leibnitz, et ceux qui l'ont suivi, ont appelé ainsi le produit du corps par l'espace et par la vitesse ; j'ai adopté une définition établie, contre laquelle on n'avait point disputé, et que je n'avais aucune raison de changer."

Leibnitz's definition of " Action " is given in one of his reputed letters, thus : " L'action n'est point ce que vous pensés, *la consideration du tems y entre ;* elle est comme le produit de la masse par le tems, *ou* du tems par la force vive."

This Leibnitzian sense is, unfortunately, that in which modern Relativists use the word "Action." Thus Eddington (*Space, Time and Gravitation*, p. 47), says: "After Mass and Energy there is one Physical quantity which plays a *very fundamental part in Modern Physics*, known as *Action*. Action here is a very technical term, and is not to be confused with Newton's 'Action and Reaction.' . . . Action is thus Mass multiplied by Time, or Energy multiplied by Time, and is *more fundamental than either*."

It does seem unfortunate to take a word which was apparently established in Mechanics in Newton's sense, and to give it this *totally different meaning*. It tends to create still more confusion. Henri Poincaré said (*Science et Méthode*, p. 29): "La Mathématique est l'art de donner le même nom *à des choses différentes*."

Leibnitz (as we should now say) is, in his definition, confusing two Principles, viz., Fermat's Principle of Least Time $[M\int dt]$, and Maupertuis' Principle of Least Action $[M\int v.ds]$. I will confine my attention to the latter, and I draw special attention to the words I have above emphasized, "the consideration of Time enters into it"; this is most important. This so-called "Action" is "as Time into *vis viva*." We should express this now "as Time into Energy."

Johann Samuel König (1751) said that, since Action is *vis viva* into Time, the Principle is that *vis viva is a minimum;* or, as we should now say, that *Energy is a minimum*. This appears much clearer and more "rational" than the way Maupertuis put it. In fact, Maupertuis was unpardonably obscure and, perhaps, not always unwilling to suggest that he saw truths when he did not, but would have liked to. We must remember, however, that often the whole of a difficulty lies in our not being able to set it down in words, because our ideas are vague and confused, though often unconsciously so; or conversely, our ideas are vague and confused, because we cannot express them clearly in words. It was Rousseau, I fancy, who somewhere says definitions might be good if words had not to be used in making them.

Maupertuis himself gave no valid reasons in support or

proof of his thesis, nor did he develop it. However, in the hands of Euler and Lagrange the Principle of Least Action became " a *condensed form of the equations of Motion.*" (Jourdain, as before.)

In modern times Poincaré (*La Science et l'Hypothèse*, 1912) describes it thus :

" What does the Principle of Least Action teach us ? It teaches us that in order to pass from the initial situation which it occupied at the instant t_0 to the final situation which it occupies at the instant t_1, the system must take such a path that, in the interval of Time between the instants t_0 and t_1, the mean value of the ' Action ' (that is to say, of the *difference* between the two energies T and U) shall be as small as possible.

" If one knows these two functions T and U, this Principle suffices for determining the equations of Motion.

" Amongst all the paths which allow the passage [which are open, which are available] there is evidently *one* for which the mean value of the action is smaller than for any other. Moreover, *there is only one*, and it results from this that the Principle of Least Action *suffices for determining the path followed*, and consequently, the equations of Motion."

There are several nearly related Principles in Mechanics which bear a great family resemblance to one another, viz., (1) d'Alembert's Principle, (2) Gauss' Principle of Least Constraint, (3) Hamilton's Principle, and (4) The Principle of Maupertuis which we are discussing.

Gauss observed that no *essentially new Principle* can now be established in Mechanics ; but he added, this does not exclude the discovery of new *points of view* from which Mechanical phenomena may be fruitfully contemplated. These several Principles quoted appear to give different views of the same general idea. I shall only consider the last two, since they are the most closely connected and, in a way, help to explain each other. We may even go further and say that Hamilton's Principle is one form of the Principle of Least Action.

We have already considered that of Maupertuis. Regarding Hamilton's Principle, Poincaré (*La Science et l'Hypothèse*), says :

" All the changes which bodies in Nature can go through are governed by two experimental laws.

" (1) The sum of the Kinetic Energy and of the Potential Energy is a constant. This is the Principle of the Conservation of Energy.

" (2) If a system of bodies is in the situation A at the epoch t_0 and in the position B at the epoch t_1, it always moves from the first to the second situation by such a path that the *mean* value of the difference between the two kinds of Energy, *in the time interval* which separates the two epochs t_0 and t_1, shall be as small as possible.

" This is Hamilton's Principle, which is one of the forms of the Principle of Least Action.

* * * * * * *

" Whenever the Principles of Energy and of Least Action are satisfied, we shall see that not only is there always a Mechanical explanation possible, but there are always innumerable such [*une infinité*]."

It will be seen how very closely these two Principles resemble one another ; they may, in fact, be considered one Principle explained in slightly different terms. In both of these descriptions, however, it must be admitted that the use of the word " Action," in this non-Newtonian sense, is anything but satisfactory, since, as I said before, it undoubtedly tends to confusion. M. Poincaré speaks of the " mean value of the ' Action ' (that is to say, *the difference* between the two Energies T and U), etc." Newton's " Action " has a clear and well-accepted meaning, and I see no reason why this Principle of Least Action could not be expressed less obscurely and with the use of the word " Action " in its recognised Newtonian sense. Besides, to say, as one here does, in effect, that the " Action is least when it is reduced to a *minimum*," is not a very helpful Principle.

Lagrange is certainly clearer. In his *Mécanique Analytique*, Second Edition, he says : " The Principle in question reduces to this : the sum of the instantaneous *vires vivæ* of all the bodies, from the moment when they start from given points to that when they arrive at other given points, is a maximum or a minimum. We might then call it, on better grounds, the principle of the greatest

or smallest *vis viva*, and this way of regarding it would have the advantage of being general, both for motion and for equilibrium, since we have seen that the *vis viva* of a system is always a maximum or a minimum in a position of equilibrium."

E. T. Whittaker (*History of Theories of Ether and Electricity*), says: "Maupertuis' memoir is of great interest from the point of view of Dynamics; for his suggestion was subsequently developed by himself and Euler and Lagrange into *a general principle which covers the whole range of Nature, as far as Nature is a Dynamical system.*"

As Euler himself (1762) said: "What satisfaction would M. de Maupertuis not have, if he were still alive, to see his Principle of Least Action carried to the highest degree of dignity of which it is susceptible."

Euler had already shown, in the Supplement to his *Methodus Inveniendi, etc.*, 1744, that $\int v.ds$ is a minimum for planetary orbits. Jourdain (*loc. cit.*) also points out that Daniel Bernoulli, in a letter to Euler, dated April 23rd, 1743, comments on the latter's discovery that $\int v.ds$ is a minimum for central orbits, which Euler had obviously communicated to him without proof. Bernoulli considered the observation "very beautiful and important."

Maupertuis refers to Euler's discovery, in the Introduction to his paper to the Académie de Berlin, 1752, "rather patronizingly"—as Jourdain puts it—saying, "it is a beautiful application of my Principle to the Motion of the Planets, of which this Principle is, in fact, the rule."

Referring to his own discovery of the minimum of the action-integral for central orbits, Euler remarked: "Besides, I had not discovered this beautiful property *a priori*, but (using logical terms) *a posteriori*, deducing after many trials the formula which must become a minimum in these movements; and, not daring to give it more force than in the case which I had treated, I did not believe that I had discovered a wider Principle; I was content with having found this beautiful property in the movements which take place around centres of forces." [Jourdain, *P. L. Action.*] Euler thus appears to have *assumed* this "Least Action"

and to have specially sought for an action-integral which must become a minimum.

It may be well now to endeavour to restate this Principle of Least Action in a slightly different manner. In doing this, we shall be assisted by Lagrange's proof that the Integral in the Principle of Least Action transforms into $\int 2T.dt$.

In this re-statement nothing will, in reality, be *added* to what has been said here previously; nevertheless, putting the matter in a different light often assists in clarifying a subject; especially as I personally attach the greatest importance to the use of the word "Action" in its Newtonian sense. I would venture, therefore, to restate the Principle thus:

> "There is the Least Action [Newton's "Action," Stress, Resistance] *when* the sum of the quantities of Energy transformed during each of the very small, successive intervals of Time, t_0, t_1, t_2, etc., is a minimum."

This may be expressed in Mathematical language as "Action is least, when $M\int v.\frac{ds}{dt}.dt$, or $M\int v.ds$, is a minimum." The Principle might, in fact, be called the "Principle of Least Resistance," and is a form of the Principle of Parsimony.

A corollary to this would be that there is always "Least Action" when Energy is *uniformly transformed* during successive intervals of Time, t_0, t_1, t_2, etc.; that, in other words, the rate of the change of speed must be a *uniform rate*. I am here always using "Action" in its old Newtonian sense, as synonymous with "Stress," or "Resistance," and not for the expression $M\int v.ds$—which is certainly not "Action"; not having its *dimensions*.

In "Relativity," Mr. Bertrand Russell tells us: "The word 'Action' is used to denote Energy multiplied by Time. That is to say, if there is one unit of Energy in a system, it can *exert* one unit of Action in a second, 100 units of Action in 100 seconds, and so on." (*A.B.C. of Relativity*, 1926.)

This is the use of the word " Action " in the sense to which, as previously stated, d'Arcy objected nearly two hundred years ago. " One unit of Energy," also, would appear to be capable of " exerting " an *unlimited* amount of " Action " !

Mr. Russell further says : " ' Action ' is thus, in a loose sense [! !] a measure of *how much has been accomplished:* it is increased by *displaying* more Energy and by working for a longer time"—whatever Mechanical meaning this may have.

If we consider Leibnitz's first definition of " Action " (previously referred to), viz., " the product of the Mass by the Time," we are led, not to the Principle of Least Action, but to Fermat's Principle of *Least Time.* In this Principle the Integral is not $\int v.ds$, but

$$\int \frac{ds}{v} = \int dt,$$

where, as in the other integral, ds is an Element of the Path. Fermat's Principle of Least Time is certainly a very important one, but it is not very relevant here.

Since we know from this discussion that a body will take such a path that, in the intervals between the instants t_0 and t_1, the mean value of $M\int v.ds$ becomes as small as possible, we may, in popular language, paraphrase this as " the body *must take the line of Least Resistance* "—which is almost a truism. The value of this truism will, however, appear later.*

Let us now take a case in illustration by applying this Principle to the path followed by a projectile, which we will suppose to be moving in a vacuum and accelerated by Gravity. I will quote it from Mach's *Science of Mechanics* (p. 368) :

" For a *single* body moving under the action of Forces, Euler finds the requisite expression in the formula $\int v.ds$, where ds denotes the Element of *the path* and v the corresponding Velocity. This expression is smaller for the path *actually* taken than for any other infinitely adjacent neighbour-

* This paraphrase must not be pushed too far, since in the case I will now adduce *there is no resistance.* Energy is transformed, though no *work* is done.

ing path between the same initial and terminal points, which the body *may be constrained to take.* Conversely, therefore, by *seeking* the path that makes $\int v.ds$ a minimum, we can also *determine the path.* The problem of minimizing $\int ds$ is, of course, as Euler assumed, a permissible one, only when v depends on the position of the Elements ds, that is to say, when the Principle of *vis viva* holds for the forces, or a force function exists, or, what is the same thing, when v is a simple

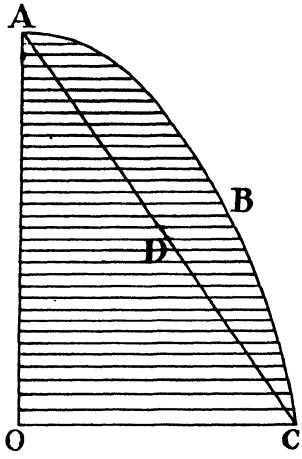

FIG. 10.

function of co-ordinates. For a Motion in a plane the expression would accordingly assume the form

$$\int \phi\,(x, y)\,\sqrt{1+\left(\frac{dy}{dx}\right)^2}\,.\,dx.$$

" In the simplest cases Euler's Principle is easily verified. If no forces act, v is constant, and the curve of Motion becomes a straight line, for which $\int vds = v\int ds$ is unquestionably *shorter* than for any other curve between the same terminal points. Also, a body moving on a curved surface *without the action of forces* or friction, *preserves its Velocity,* and describes on the surface a *shortest* line.

" The consideration of the motion of a projectile in a parabola ABC (Fig. 10) will also show that the quantity

$\int v.ds$ is smaller for the parabola than for any other neighbouring curve ; smaller even than for the straight line ADC between the same terminal points. The Velocity, here, depends solely on the *vertical space described by the body*, and is, therefore, *the same for all curves* whose altitude above OC is the same. If we divide the curves by a system of horizontal straight lines into Elements which severally correspond, the Elements to be multiplied by the same v's, though in the upper portions smaller for the straight line AD than for AB, are in the lowest portions just the reverse ; and as it is here that the large v's come into play, the sum upon the whole is smaller for ABC than for the straight line.

" Putting the origin of the co-ordinates at A, reckoning the abscissas x vertically downwards as positive, and calling the ordinates perpendicular thereunto y, we obtain for the expression to be minimized

$$\int_0^x \sqrt{2g\,(a+x)}\,\sqrt{1+\left(\frac{dy}{dx}\right)^2}\,.\,dx$$

where g denotes the acceleration of gravity and a the distance of descent corresponding to the initial Velocity. As the condition of minimum the Calculus of Variations gives

$$\frac{\sqrt{2g\,(a+x)}\cdot\frac{dy}{dx}}{\sqrt{1+\left(\frac{dy}{dx}\right)^2}}=C$$

$$\left(\frac{dy}{dx}\right)^2\{2g\,(a+x)-C^2\}=C^2$$

$$\frac{dy}{dx}=\frac{C}{\sqrt{2g\,(a+x)-C^2}},$$

or

$$y=\int\frac{C\,dx}{\sqrt{2g\,(a+x)-C^2}},$$

and, ultimately,

$$y=\frac{C}{g}\sqrt{2g\,(a+x)-C^2}+C_1,$$

where C and C^1 denote constants of integration that pass into $C=\sqrt{2ga}$ and $C^1=0$, if for $x=0$, $dx/dy=0$, and $y=0$ be taken. Therefore $y=2\sqrt{ax}$. By this method, accordingly, the path of a projectile is shown to be of parabolic form."

As was said previously, Euler simply *assumed* that the minimizing of $\int v.ds$ was permissible *only* when the Principle of *vis viva* held for the forces. It was left for Lagrange to *prove* this limitation. If, however, we refer to my statement of the Principle, as above (page 176) we shall see that this limitation follows as a necessary condition. If the Action is least *when* the Energy transformed is a *minimum*, it seems to follow that if the Principle of Energy does not hold, neither does the Principle of Least Action.

Maupertuis, as a follower of Newton, held a Corpuscular Theory of Light, and endeavoured to apply his Principle of Least Action to the path of Light; but with indifferent success, for Light moves according to Fermat's *Principle of Least Time*.

Considering the enormous importance of the Principle of Least Action, or, I would say, *Least Resistance*, it is very curious how it is ignored by writers of Text-books on Mechanics; Mach appears to be one of the very few exceptions; even in Karl Pearson's *Grammar of Science*, I can find no reference to it.

I will conclude this Chapter by illustrating it by a pretty little problem involving "Least Action," which was, I believe, propounded by Dodgson, and is referred to by W. W. Rouse Ball in his *Mathematical Recreations*. The problem is certainly not a very new one, but I am not aware that anyone has ever discussed the correct solution.

Rouse Ball prefaces the problem by stating that: "The idea of Force is difficult to grasp." This, as we have seen, is unfortunately too true; the idea of Force has been made as *difficult as possible;* indeed, at times, it appears almost impossible to grasp it at all. But to continue the quotation: "How many people, for instance, could predict correctly what would happen in a question as simple as the following? A rope (whose weight may be neglected) hangs over a smooth pulley; it has one end fastened to a weight of ten stone, and the other end to a sailor of weight ten stone,

the sailor and the weight hanging in the air. The sailor begins steadily to climb up the rope ; will the weight move at all ; and if so, will it rise or fall ? "

Dodgson himself tells us that he submitted this problem to certain Mathematicians, of whom some said that the weight would not move ; some that it would go down ; whilst some said that it would go up ! Rouse Ball, however, laconically says : " In fact, it will rise." This is no solution of the problem, nor does it completely answer the question. That the stone will *rise* is a matter of common observation ; the problem, however, involves :

(1) *Why* will the stone rise ? ; and
(2) *How fast* will it rise ?

I trust that my reader will have no difficulty in solving this problem for himself. Since, however, there may be some who do not trust their own powers, I will give the solution here.

In the first place, it is obviously not a problem in Pure Dynamics, but one in Energetics—a vital difference. The Principle of the Conservation of Energy must be satisfied ; hence the only way in which the sailor can climb the rope is by transforming some of his Potential Energy into Kinetic Energy.

But the Principle of Least Action, or Least Resistance, must also be satisfied ; hence the " sailor-stone system " in moving from a position A to a position B, must do so in such a manner that the amount of Energy transformed is a *minimum*. Now it is not difficult to show that in order to do this the sailor and the stone must ascend *at equal speeds*.

For example, let us suppose that the sailor climbs up the rope at a speed of four feet per second. If the stone did not move—as would be the case if the friction of the pulley were very great—but here assumed as " smooth "—the sailor would transform Energy which may be measured by $4^2=16$.

Suppose, however, that whilst the sailor is climbing the rope, the stone goes up at the rate of one foot per second. The sailor would now only transform Energy measured by $(4-1)^2+1^2 = 10$.

But when stone and sailor move at equal speeds, the

Energy transformed is only *one half* of that required if the stone were immovable ; *i.e.*, $2^2+2^2 = 8$. This is the simple proof of the solution.

Latterly, in 1921, this problem of " The monkey and the stone," was revived in the *American Mathematical Monthly*, Vol. XXVIII. The solutions which were offered of it again varied considerably. Eventually, however, it appears to have been agreed that, *provided friction were neglected*, the monkey and the stone would go up equally ; but that, if the friction of the pulley were taken into account, the monkey would move up faster than the stone. A very fearsome formula was given which expressed the relationship of this difference. It is needless to say, however, that if this were the case, the Principle of Least Action would not be satisfied, since the Energy transformed *would not be a minimum.*

The correct solution, clearly, is that, whatever amount of Energy is required to turn the pulley, that amount becomes a *first charge* on the monkey. Whatever further Energy the monkey can transform must then be *equally divided* between the stone and himself ; *i.e.*, they must *move up equally.*

Résumé.

The line of Least Resistance is the path along which $M\int v \,.\, ds$ is a *minimum.*

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CHAPTER XIV

SHAPES OF LEAST RESISTANCE

THE Problem of the Shape of Least Resistance of bodies moving in Fluid Media was first studied seriously by Newton in the *Principia;* but since his time very little that is useful has been done on this subject. In fact, by the present method of study of the Resistances of bodies moving in fluids, I do not see how it would be possible to *calculate* the shape which would offer the Least Resistance; since it does not appear that the Resistance of *any* body can be calculated by this method. Physicists are now satisfied by making small-scale models of the bodies whose Resistance they require to determine, and then *measuring* what the Resistance of these models *actually is*, in a wind-tunnel, water-channel or tank. This is not calculation!

Historically now, in the *Principia*, Book II, Prop. 34, it is shown that if "a globe and a cylinder described on equal diameters move in a rare medium with equal Velocities in the direction of the axis of the cylinder, the Resistance of the globe will be but half so great as that of the cylinder."

This problem, though highly interesting mathematically, is, from a practical point of view, of very little use, since all the particles of Newton's "rare medium" are supposed to move in straight lines parallel to the axis and to "impinge" with a given Velocity upon the bodies; after which they appear to vanish down a kind of mathematical "sink." The Resistance of a sphere, in a *real fluid*, such as water or air, is actually only two-fifths of that of its circumscribing cylinder; as can be determined either by calculation or by experiment. Real fluids, moreover, divide at some distance from the body and then flow round and past it with accelerated Motion; the particles of the fluid do not actually "impinge" on the solid but *flow round it.*

In the Scholium to this Proposition, Newton states that

adapting a kind of flat cap to the front end of a solid "generated by the convolution of an elliptic or oval figure," reduces the Resistance of the original solid—*i.e.*, the "capped solid," as I phrase it, will be less resisted than the uncapped solid. In this case Newton was obviously thinking of real fluids, for he concludes: "which proposition, I think, will not be without use in ship-building."

Newton then goes a step further, for he defines a figure such that "the solid described by the revolution of this figure about its axis AB, moving in the before-mentioned rare medium from A towards B, will be *less resisted than any other circular solid whatsoever, described of the same length and breadth.*" He gives no proof, but the demonstration of

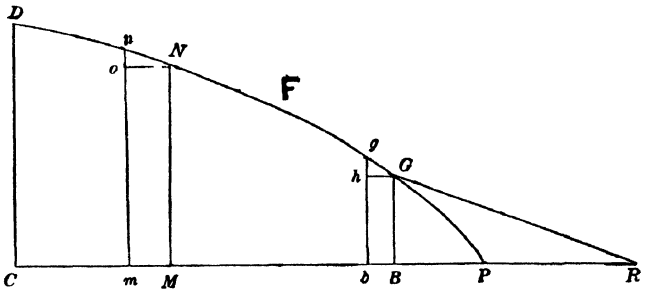


FIG. 11.

this curious Theorem will be found in the Appendix to Motte's Translation of the *Principia* (1729), where we have the "explication (given by a friend)." I would draw special attention to this proof, since it is not, apparently, generally known; and it was long considered a mystery how Newton arrived at his solution.

A copy of a letter in Newton's own hand is extant ("Portsmouth Collection") which also contains the same proof, but I would like to point out that in the reproduction of this letter ("Catalogue of the Portsmouth Collection, p. xxii") the accompanying figure (copied photographically here, as Fig. 11) is *not* correctly drawn, and for the following reasons:

(1) *hg* is drawn shorter than *h*G.—They should be equal.

Newton's letter says : " DNFG is *an uniform curve* meeting with the right line GB in G, in an angle of 135 degrees."

(2) The line GP, in the rough draught of Newton's actual letter (reproduced here photographically, as Fig. 12), is *not a curve,* but simply a constructional line representing the tangent to the curve at G. The letter P is not found in

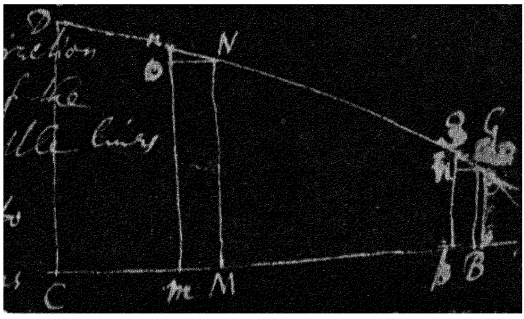


FIG. 12.

Newton's figure at all, and the line GP is never referred to by him.

The figure in the Catalogue has been carelessly drawn, and is very misleading, for it shows DNFGP as *one continuous curve.* It therefore suggests that the solid of Least Resistance is that generated by the revolution of

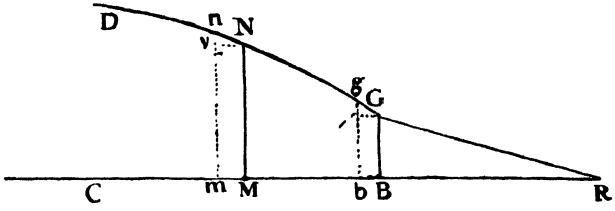


FIG. 13.

DNFGP about the axis BC ; whereas it should be the body formed by the revolution of DNFGB *; i.e.,* as Newton says : " the solid with the same top and bottom BG and CD." The figure is correctly drawn in the Appendix to Motte's Translation (reproduced here as Fig. 13).

Whether Newton's " solid of Least Resistance." would

really be that offering the Least Resistance *when moving in water* has never, that I am aware of, been tested experimentally. Newton himself says, in his letter, " I have not yet made any experiments about the Resistance of the air and water ; " and it does not appear that he ever did make the experiments. I am, myself, inclined to think that Newton's solid would *not* be found to be the shape of Least Resistance, since Newton's argument is based on the " Sine-squared Law," which we know, experimentally, is not true ; and one can only get out of a Mathematical equation what he has previously put into it.

M. Cantor says that Newton, in 1687, " solved the first problem of the Calculus of Variations," the determination of the figure of the solid of Least Resistance. I quote this statement for what it may be worth. (*cf* M. Cantor, *op. cit.*, p. 291.)

But in these Theorems if Newton did not, indeed, strictly determine the shape of the Body of Least Resistance, he certainly indicated the lines on which research should proceed in order to discover it. We have seen that the question must be approached by the use of the Calculus of Variations.

At this point it will be well to indicate how the Resistance of a body of Revolution, whose surface of presentation is regularly curved, *can be calculated.* I have shown in *Resistance of Air* how the Resistance of a cylinder can be calculated ; it is necessary now to show how a " coefficient of shape " can be calculated, which will give us the ratio between the Resistance of the Body and that of its Circumscribing Cylinder. This will tend towards making Hydraulics a *science,* instead of merely a collection of empirical rules, *corrected,* so far as possible, by coefficients derived from experiment.—a method, indeed, little better than what a schoolboy would call " fudging " giving the calculation, what Henri Poincaré called familiarly " *un coup de pouce.*"

Professor Unwin has admitted that " in Hydraulic calculations, it must [why " must " ?] frequently be impossible to assign the true values of coefficients within ten per cent." It will be seen, however, that by the method here employed, we can calculate the Resistances of Bodies of Revolution to

one per cent. of the results obtained by experiment, and sometimes very much less.

I shall make one fundamental assumption, viz., that the Resistance of a Body of Revolution bears a *fixed numerical relation* to that of its enveloping cylinder. It may be thought by some that this is " obvious " ; but many things in Hydromechanics which are, apparently, " obvious," can be shown to be wrong. I therefore prefer to point out that it is *an assumption*, which we shall find is supported by experiment.

In *Resistance of Air* (which I assume the reader to know),

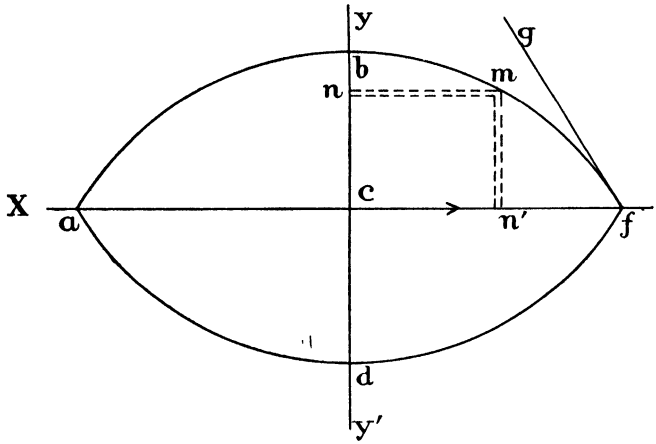


FIG. 14.

I have shown that the Resistance of a Cylinder, moving in the direction of its axis, can be expressed as

$$R = K \cdot \omega \Delta u^2$$

where K is a constant, ω is the sectional area of the cylinder, Δ is the density of the fluid medium, and u is the speed of the body through the fluid. The Resistance of the body to be calculated may be expressed as

$$R_1 = K \omega \Delta u^2 \times C$$

where R_1 = R × coefficient of shape.

The old Scotch proverb says: " Weel soapit is hauf shavit." Having prepared the ground let us attack the problem directly. Let Fig. 14 represent the section of a body

of revolution generated by the revolution of the curve *abf*, and which is moving in the fluid (at rest) in the direction *af* passing through its centre of figure. Let *bd* be the diameter of its greatest section, normal to the direction of Motion. The area of the greatest cross-section, ω, will be $\pi (\overline{bc})^2$.

Let *ds* be an Element, *m*, of the generating curve, and *p.ds* the normal pressure which it experiences in passing through the fluid. Then putting *v* as the speed, *in the direction of the normal*, or the speed with which the particles of the fluid are being displaced; and adopting the usual theory regarding the Pressure and the Resistance of Bodies, we have

$$p = cv^2 \ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

where *c* is a constant depending upon the density of the fluid. And if *u* is the speed in the direction of the axis X, then

$$v = u \cdot \frac{dy}{ds} \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

and (1) becomes

$$p = cu^2 \left(\frac{dy}{ds}\right)^2 \ldots\ldots\ldots\ldots\ldots \quad (3)$$

This pressure may be resolved along the axes X and Y; that part along Y not retarding the body. Putting the part resolved along X, as exerted upon the Element *n* of the greatest section *bd*, the pressure, per unit area, on the Element at *n* may be expressed as

$$p^1 = p \cdot \frac{dy}{ds} = cu^2 \left(\frac{dy}{ds}\right)^2 \ldots\ldots\ldots\ldots \quad (4)$$

But the body being one of Revolution, the area of this Elemental zone, whose radius is *y*, will be $2\pi y.dy$; and the total pressure, P, on this Elemental circular ring will be

$$\mathrm{P} = \mathrm{K} \Delta u^2 \left(\frac{dy}{ds}\right)^2 . 2\pi y . dy,$$

or

$$\mathrm{P} = \mathrm{K} \Delta u^2 . 2\pi \left(\frac{dy}{ds}\right)^2 . y . dy.$$

where K is a constant, and Δ is density, as before.

Since we are going to compare the resistances of curved bodies to those of cylinders, whose surfaces of presentation are normal to the line of motion, it will be further necessary to divide by $(dy^1/ds^1)^2$, dy^1/ds^1 being the sine of the angle, *gfa*, which the curve makes with the X axis at *f*; this is the greatest value of dy/ds in the whole curve. If we then multiply by $(ds^1/dy^1)^2$ we shall get, for the total value of the Resistance,

$$R = K \Delta u^2 . 2\pi \left(\frac{ds^1}{dy^1}\right)^2 \int_0^r \left(\frac{dy}{ds}\right)^3 . y . dy \quad \ldots\ldots \quad (\theta)$$

where $r = bc$, the radius of the circle; $\left(\frac{ds^1}{dy^1}\right)$ is a constant, for the same curve, but is different for different curves; K is

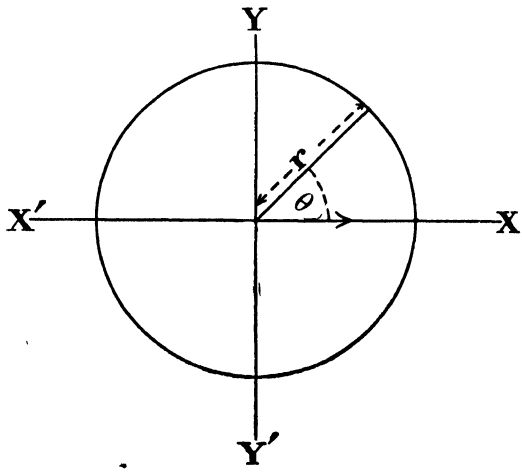


FIG. 15.

the proper coefficient for the cylinder whose radius is *bc*, and whose length is *fa*. The values of K are given in *Resistance of Air*; their reproduction here is unnecessary.

From our assumption that the Resistance of a body of Revolution bears a fixed relation to that of its circumscribing cylinder, it follows that this ratio is

$$2\pi \left(\frac{ds^1}{dy^1}\right)^2 \int_0^r \left(\frac{dy}{ds}\right)^3 . y . dy : \omega.$$

Let us now apply our knowledge by calculating the Resistance of a Sphere. Let Fig. 15 represent the section

of a sphere, moving in the direction indicated by the arrow Applying Polar Co-ordinates, it is clear that

$$x = r\cos\theta \qquad y = r\sin\theta.$$

$$\frac{dx}{dy} = -\tan\theta \qquad dy = r\cos\theta.\, d\theta.$$

But $ds^2 \equiv dx^2 + dy^2.$

$$\frac{ds^2}{dy^2} = 1 + \tan^2\theta = \frac{1}{\cos^2\theta}$$

$$\left(\frac{dy^1}{ds^1}\right)^2 = \sin^2 90° = 1\ ;\ \text{so will be omitted.}$$

$$I = 2\pi \int_{\frac{\pi}{2}}^{0} y \cdot \left(\frac{dy}{ds}\right)^3 \cdot dy$$

$$= 2\pi \int_{\frac{\pi}{2}}^{0} r\sin\theta \cdot \cos^3\theta \cdot r\cos\theta \cdot d\theta$$

$$= 2\pi r^2 \int_{\frac{\pi}{2}}^{0} \cos^4\theta \cdot \sin\theta \cdot d\theta$$

$$= 2\omega \left[\frac{\cos^5\theta}{5}\right]_{\frac{\pi}{2}}^{0} = \omega \cdot \frac{2}{5},$$

whence the Resistance of a sphere is 2/5 of that of its circumscribing cylinder. But the value of K for this cylinder is 0·6412 ; therefore K, for the sphere will be $\frac{32}{125}$ (correct to third place of decimals), and the Resistance of a sphere may be expressed as

$$R_1 = \frac{32}{125}\,\omega\,\Delta\,u^2.$$

This coefficient of shape can (of course) be also calculated by the use of the ordinary equation to a circle, viz., $x^2 + y^2 = r^2$ and be found to be 2/5 ; but it is a trifle tedious, and is not so elegant.

That the "coefficient of shape" of a sphere is actually 2/5 can be seen by reference to Charles Hutton's experiments, recorded in *Mathematical Tracts*, Vol. III, 1812. Very many examples also of the application of this coefficient will be found in *Resistance of Air*, where it is shown that correct results are produced when it is applied to spheres moving at speeds up to 2,000 feet per second.

Let us now approach the question of the calculation of the Shape of Least Resistance. Lewis B. Carll (*Calculus of Variations*, 1885) in referring to this question, in his Prop. VI, says: "It is evident that the Problem does not admit of a solution until some further restrictions are imposed."

What further restrictions are required? A little reflection will show that, for one, the Principle of Least Action must be satisfied. In other words, that the sum total of the quantities of Energy transformed during each and all of the very small intervals of Time, t_0, t_1, t_2, etc., must be a *minimum*. This condition Carll does not refer to.

The problem is now analogous to that of the parabolic path of a projectile, moving in a vacuum, discussed in the last Chapter. When a body is moving *uniformly* in a resisting medium, it is clear that it is being *uniformly accelerated* by some "Effort"* whilst it is also being *uniformly decelerated* by a "Resistance"; the "Effort" and the "Resistance" being equal and opposite to one another. The filaments of the fluid are also being constrained to move along the curved path formed by the figure of the moving body.

The problem, therefore, is to find a path, or a curve which will so constrain the fluid as to cause the least amount of Energy to be transformed in a given time.

As in the last Chapter, we have

$$\int \phi\,(x, y)\,\sqrt{1+(dy/dx)^2}\,.\,dx.$$

In the Problem before us, let us assume that the fluid particles are constrained to move along the curved path

* I here use Rankine's expression.

ADC, Fig. 16 (A and C being the terminal points of the curve), by a body ABC, which is itself moving along the straight path X towards X^1 at a uniform speed v. The path taken by the fluid filaments is to be such that $\int v.ds$ is a *minimum; ds* being an Element of the path ADC.

Putting X for the measure of the accelerating "Effort" (corresponding to g in the problem in the last Chapter), and proceeding as before, we get the expression to be minimized

$$\int_0^{BC} \sqrt{2X(a+x)} \cdot \sqrt{1+(dy/dx)^2} \cdot dx,$$

where a is the distance corresponding to the *initial speed.*

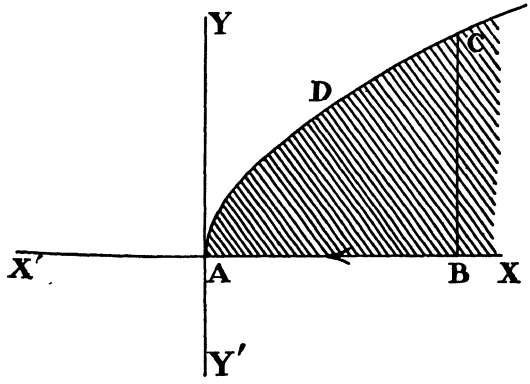


FIG. 16.

From this, ultimately, we get $y=2\sqrt{ax}$.

In this solution, I have neglected Resistance and, therefore, treated the Body as being uniformly accelerated; the speed at any point being expressed by $\sqrt{2X(a+x)}$.

By this argument, accordingly, the path which the fluid filaments will follow when $\int v.ds$ is a *minimum*—that is to say, the path which they will follow when the sum total of all the amounts of Energy transformed during all the successive small intervals of Time, t_0, t_1, t_2, etc., is a minimum; which implies *Least Resistance*—is seen to be of *parabolic form.*

Therefore, the shape of the body of Least Resistance—the body which so constrains the fluid filaments as to cause them to follow this path—*will be paraboloidal.*

The calculation of the Resistance of a Paraboloid is as easy as the calculation of the Resistance of a Sphere. For a paraboloid whose height is equal to the diameter of its base, the " coefficient of shape "—*i.e.*, the ratio it bears to that of the circumscribing cylinder—is 0·256; being very considerably less than that of the sphere. Paraboloids of greater height than one radius offer considerably less Resistance than even this.

Next comes the question: How far does experiment support this theoretic conclusion? I know of no experiments bearing directly on the question. No one has made, as far as I am aware, any regular series of experiments on the Resistances of bodies of Revolution generated by known curves. The bodies tested have always been of (what is called) " stream-line " shape; but since no equation of the " stream-line curve " is ever offered, the Resistances of these bodies are not amenable to calculation.

My calculations of the Resistance of variously shaped bodies of Revolution, generated by known curves, have shown me that the " coefficient of shape " of the Paraboloid is smaller than the coefficient of any other body of Revolution; and that, therefore, the Paraboloid is the Body which offers the Least Resistance to Motion in a fluid medium. I have failed in calculating the Resistance of a Catenoid; but I have no reason to expect its Resistance is less than that of a Paraboloid.

All this, however, is theoretical, and is not direct practical evidence of correctness. But if we examine different objects which are designed to offer the minimum of Resistance to an opposing medium, shapes which have been designed after a good deal of " trial and error," or if we study Nature and examine certain birds and fast-swimming fish, we can obtain a good deal of " collateral " or indirect evidence on this point.

(A) The best shells, or shot, for heavy ordnance, have their heads " struck " as circular ogivals; but near their noses this ogival changes into a portion of a sphere. If

examined carefully it will be seen that the curve makes a fairly close *approximation to a parabola.*

(B) The ends of the best "air-ships" *look* very like paraboloids, though their extremities are, I believe, parts of spheres. I have reason to believe, from what I gather, that these ends are really *parabolic ogivals*—the parabolas being of a somewhat high order.

(C) The heads of torpedoes, again, *look* very like paraboloids. They were originally made with pointed—"circular ogival"—heads, but it was found (quite possibly by accident) that a blunt nose was an improvement, both from the point of view of Resistance and of steering qualities.

(D) The leading edges of aeroplane wings are not made "sharp"—in order to, as it is called, "cut the air"—but

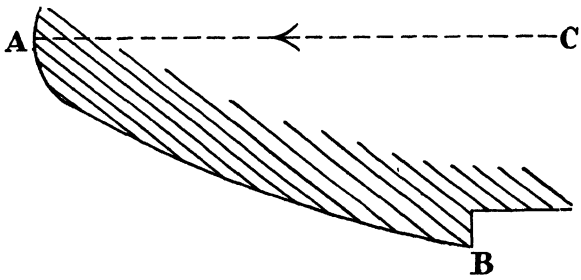


FIG. 17.

blunt. It has been found, *practically*, that they are more efficient if made very blunt; and the best curves for the cross-section of the upper surface of these edges are found to be those which, certainly, bear a strong *resemblance to parabolas.*

This shape gives, I believe, the minimum of *Resistance* to the wing of the aeroplane. If, however, it be desired to obtain a wing with the greatest "*lifting power*"—for rapid climbing, say—then, I suggest that the best cross-section of the leading-edge of the wing should be a cycloid, and not a parabola. A very small amount of speed would be sacrificed, but the "lift" would be materially advantaged. It would be worth trying, practically, whether such is the case.

(E) The longitudinal section of the fore part of the floats of "flying-boats" (which are flat-bottomed) is somewhat as in Fig. 17, where AB very closely *resembles a parabola,*

whose axis is AC. This shape has, we may assume, been found *experimentally* to be the best. Indeed, at an Exhibition of the "Model Engineers," my attention was drawn to a "skimming-boat" built similarly to Fig. 17, the curve AB of which was *actually* and *intentionally* made a parabola! I learned that the maker had tried various curves for AB, and had found that the parabola was the curve which enabled the boat to travel at the greatest speed!

I have been informed by a member of a Model Boat Racing Club that the "step" shown at B had been long used on model boats; though it is still rather an innovation on "full-size" boats, not being more than about sixteen or seventeen years old. It was, I believe, first adopted by Sir John Thornycroft, in his boats called "Hydroplanes"; his celebrated *Maple Leaf*, built on this principle, attained a speed of 48·8 knots. It has been stated in the press that an American hydroplane, *Miss Chicago*, was reported to have made an average speed of 70·951 miles per hour during a six-mile trial!

From the point of view of the Modern Theory of Resistance—the "Fluid Friction" Theory—this "step" should be a hindrance, since it increases the wetted surface. As it has proved to be an advantage it must, I suppose, be placed among the "paradoxes," and the discussion of it discouraged!

I referred to these hydroplanes in my *A.B.C. of Hydrodynamics*, 1912, where I suggested as an explanation of this curious reduction in resistance that, as air was introduced under the hull the boat might, *in a sense*, be said to "float on air." In this explanation, I was no doubt influenced by the fact that at that time it was considered *most important* that there should be a free-channel leading down to this step. At the present day it is found that such an air-channel is unnecessary. I would, therefore, withdraw my explanation of 1912, and substitute for it that of 1914 (*Motion of Liquids*) which is, that it is a case of "recuperation of Energy."

The water in passing the hull is accelerated until it gets to the greatest cross-section, where the acceleration is a *maximum*. It may be said, speaking familiarly, that when

the water has reached this greatest cross-section "the damage is done"; the amount of the Energy transformed "*has now to be paid for.*" If, however, we can by any means *retransform* some of the Kinetic Energy back into Potential Energy, then (on balance) the Kinetic Energy will be reduced; and hence the Resistance will also be reduced.

Passing the "step," a strong vertical vortex is formed in the hollow behind it; and the sides of this hollow will tend to retard the rotation of the vortex, increasing its Pressure, and thus tending to push the boat forwards. Thus less Energy will *have to be paid for.*

Of recent years the use of the "step" has been pushed

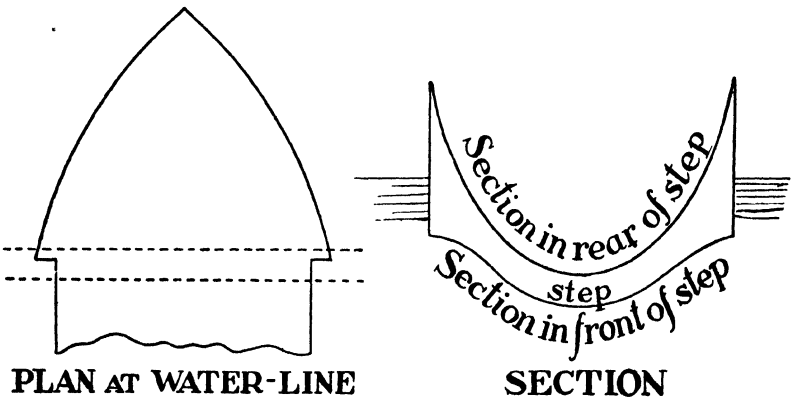


FIG. 18.

further; not only is the step made at the bottom of the boat, but it is also continued up the sides, which are nearly vertical, somewhat as in the rough Diagram, Fig. 18.

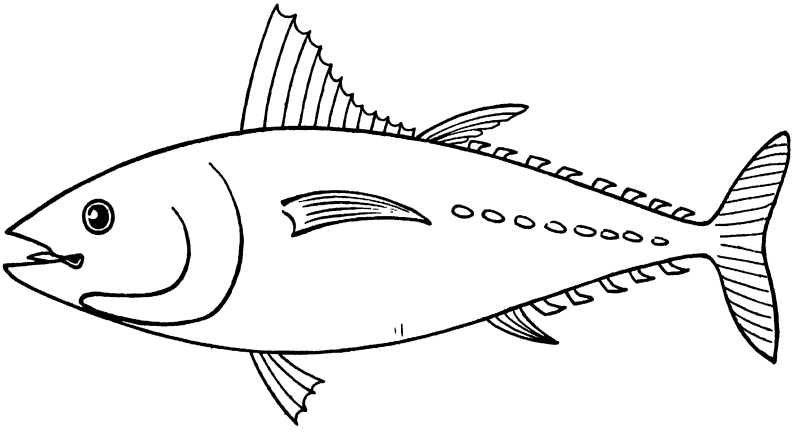
In this case horizontal as well as vertical vortices are formed which are "damped down" behind the step.

I have suggested, as an experiment, to members of a Model Racing Boat Club, applying some roughening material behind the step, with a view to still further damping the vortices. I am not aware if this has been actually tried.

(F) The fishes of the Tunny genus—indeed the whole of the Mackerel family—are probably the fastest swimmers for long distances; numbers of other fishes, the Pike, for

example, can move at extraordinary speeds, but only for short distances.

The Mackerel family, from behind the dorsal fin up to nearly the base of the tail (both above and below) have small " finlets," as shown in the diagram-drawing in Fig. 19. These are movable and supplied with muscles. Now, of what use are these " finlets " ; I presume that they serve *some* purpose, or they would not be there ? I understand that nobody has suggested any use for them, beyond the very vague idea that they help in steadying the fish and so preventing it from rolling. If required for this purpose



Thynnus Thynnus, or Thunnus Thynnus. 10 feet long. Weight up to 1,000 lbs. 6 to 9 "finlets." One of the largest fish in the ocean. The only "warm-blooded" fish.

FIG. 19.

they appear to me to be very unsuitably placed ; with such a very powerful dorsal fin their use for such a purpose would appear to be superfluous. I suggest that their purpose is to damp down the vortices in the water, and by so doing to *recuperate* some of the Kinetic Energy by re-transforming it back into " Pressure." By this means the Resistance to Motion is reduced, and, consequently, the " Work " the fish has to do in swimming.

The Herring, another very fast-swimming fish, appears to reduce Resistance in a slightly different manner. It has

many rather large, and very thin scales. These scales have suitable muscles for enabling them to, what I may call, " open " and " shut." Whilst not pretending that this is the *sole* use, or even possibly the *principal* use of the scales, at least one of their uses is for damping down the vortices, and thus reducing the Resistance to Motion. They would act in a similar manner to the " steps " in skimming boats. In a similar manner the feathers of birds' wings, when opened a little, would also act as " recuperators " of Kinetic Energy.

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CHAPTER XV

SHAPES OF LEAST RESISTANCE (*continued*)

JOHN W. NYSTROM, of Philadelphia, says, in his *Treatise on Parabolic Construction of Ships*, 1863 :

" The *Parabolic construction* of ships was originated by the celebrated Swedish Naval Architect, Chapman,* who published a work on the same in the year 1775. Mr. Chapman came on the fortunate idea that a vessel of the least possible Resistance in water should have the ordinate cross-sections of the displacement *diminishing in a certain progression* from the dead flat.

" In order to *find out that certain progression*, he collected a great number of drawings of ships of known good and bad performances ; on each drawing he transferred the ordinate cross-sections to rectangles of the same breadth as the beam of the vessel, placed the upper edges in the plan of the water-line, by which he found that the under edges of the rectangular sections formed a bottom, the curve of which was a parabola in the ships of the best known performances."

Admiral Chapman himself does not appear to have propounded any special theory on this subject. He merely *assumed* that there must be *some* curve of Least Resistance, and by examining the drawings of the best ships came to the conclusion that the curve was a *Parabola.*

Mr. Nystrom says : " I have laboured very hard to find out some theory by which to sustain Chapman's hypothesis, but have not succeeded ; found it necessary to start on new hypothesis, namely, that *the resistance to a vessel of a given displacement, bounded in a given length, breadth and depth, is proportioned to the square of the sine of the mean angles of incidence and reflection.* By *differentiating and integrating* those angles, and finding their *maxima* and *minima*, will

* This was Fredrik Heinrik af Chapman (Frédéric de Chapman), Vice-Admiral in the Swedish Navy, b. 1721, d. 1808.

result in, that *the square root of the ordinate cross-sections of displacement should be ordinates in a Parabola*, the principle upon which the Parabolic construction is herein worked out.*

"I shall here not furnish the proofs of the formulas, for the reason that when practical men see the poetry of the calculus, they get frightened and lay the book aside; have, therefore, first furnished the conclusive formulas trimmed to a practical shape, ready to receive the given datas, and concluded with some calculus poetry on pages 19, 20 and 21, with which practical men need not trouble themselves.

"A part of the treatise on the Parabolic Construction of ships has appeared in the Journal of the Franklin Institute, where room could not be obtained to treat the subject fully, for which reason it is decided to publish it in a more complete form. The part which appeared in the Journal is herein corrected and improved.

* * * * * * *

"The Seventh Edition of Nystrom's *Pocket Book* contains a more complete set of formulas and tables for the Parabolic construction of ships, which will serve as a handy memorandum after the system is herein acquired.

"The Parabolic system is so complete that it enables us to construct the finest clipper or sailing yacht that ever will float, and it will form a 'Winan's Cigar Steamer,' a scow, a square or prismatic box or a barrel; in fact, the constructor must be very careless if he does not construct a good and proper vessel by the *Parabolic system.*"

Mr. Nystrom pointed out in the *Journal of the Franklin Institute*, Philadelphia, 1863, that: "The construction of ships is yet in an empirical state, with no established rules for laying down the principal lines, but is wholly dependent on skill, experience, and taste of the constructor. In the Parabolic construction herein proposed, positive rules are established for the principal lines, as the load water-line, rail in plan, cross-sections, displacement, sheer, etc., that when the lines are laid down by these rules they *cannot be improved* by taking in or out a little more or less here or there; the rule makes the lines right. This will enable the

* This is not easy to follow, but I quote it as it is.

young constructor to lay down ship-lines as fine as if made by the most experienced ship-builder; still he will not be confined to any particular shape or proportion of the vessel, but can vary it according to his own taste and judgment.

"The advantage of the Parabolic construction is not only in laying down fine lines, but the simple calculations connected with it are of great importance, and enable the constructor, with few figures, to go to work with the greatest certainty of attaining a correct result, without recourse to trial and error, or comparison with other vessels. All the lines in a ship are combinations of the *circle, ellipse, parabola* and *hyperbola.* These lines are derived from the conic sections, where they are curves of the second order, but herein we will employ them to any order whatever. The ordinary formula for a parabola is $y = \sqrt[n]{px}$, in which $y =$ ordinate, $x =$ abscissa, and $p =$ parameter; the letter $n = 2$ in the conic parabola; it is called the *exponent,* and denotes the order of the curve. By assuming different values on the exponent n, different forms of parabolas are obtained. When $n = 0$, the parabola will be the two sides forming the right angle in a triangle; when $n = 1$, the parabola will be the hypothenuse in a right-angled triangle; when $n = 2$, it will be the true parabola in the conic sections; and when $n = \infty$, the parabola will again become the two sides forming the right angle in the triangle. The circle, ellipse and hyperbola follow the same law, which enables us to form theoretically any line in the hull of a ship.

* * * * * * *

". . . the sharpest form a vessel can have, being a conic parabola with the exponent $n = 2$. The higher the exponent is, the fuller will the lines be. . . . The water-lines and the cross-sections are precisely the same kind of lines; both can be laid down by the same ordinates."

It will be seen Mr. Nystrom's method for ship-designing was very complete; though it would be difficult to summarize it, it is not, in itself, very difficult. Knowing, for example, the length of the vessel at the load-line, its greatest immersed cross-section, and its displacement in cubic feet (or in tons of salt water), he gives twenty-six simple equations for finding out everything else we require—including the

"exponents" of the different Parabolas. Very complete tables of the ordinates of all Parabolas with exponents of 2, $2\frac{1}{2}$, 3, $3\frac{1}{2}$, 4, 5, 6, 8, and 10, are given; that is to say in $y = \sqrt[n]{px}$, $n = 2, 2\frac{1}{2}, 3$, etc.

Laying off the abscissas on the X axis, we can then erect the ordinates, from the table, on the Y axis and so obtain the curved line. The sharpest form a vessel can have being a conic Parabola, with the exponent $n = 2$, the higher the exponent is, the fuller will the lines be. "The *exponents* perform the most prominent part, both in the calculations and construction. By proper selection of the exponents any form of vessel can be constructed by the Parabolic method."

Nystrom says that "the yacht *America*, which won the prize at the Exhibition in London in 1851 . . is a perfect model of the Parabolic Construction."* In this yacht the "exponents" are $n = 2$, $n^1 = 2$, and $n^{11} = 2\frac{1}{4}$; the last one being the only departure from the conic Parabola.

To understand this method of ship-designing it would, of course, be necessary to refer to Nystrom's book, and study it there. I regret to say that I know of only one copy of this interesting little work; it is in the "Scott Loan Collection" at the Institute of Naval Architects. It is not in the British Museum.

All the foregoing strongly supports the Theoretical arguments here advanced. Newton was already well aware of the advantages of Parabolic lines for bodies moving in resisting media, for at the end of the Scholium to Prop. 37, Book II, we see a reference to it, which is worth quoting: "that the obliquity of the motions may be taken away, and the parts of the water may give the freest passage to the cylinder, by yielding to it with the most direct and quick motion possible, so that only so much resistance may remain as arises *from the magnitude of the transverse section*, and *which is incapable of diminution*, unless by diminishing the diameter of the cylinder, we must conceive those parts of the fluid whose motions are oblique and useless, *and produce resistance*, to be at rest among themselves at both extremities of the cylinder, and there to cohere and be joined to the cylinder.

* A model of this yacht is in the Science Museum at South Kensington.

Let ABCD [Fig. 20] be a rectangle, and let AE and BE be two parabolic arcs, described with the axis AB, and with a latus rectum that is to the space HG, which must be described by the cylinder in falling in order to acquire the velocity with which it moves, as HG to ½AB. Let CF and DF be two other parabolic arcs, described with the axis CD, and a latus rectum quadruple of the former; and by the convolution of the figure about the axis EF let there be generated a solid, whose middle part ABCD is the cylinder we are here speaking of, and the extreme parts ABE and CDF contain the parts of the fluid, at rest among themselves, and concreted into two hard bodies, adhering to the cylinder at each end like a head and tail. Then if the solid EACFDB move in the direction of the length of its axis FE towards the parts beyond E, the resistance will be the same which

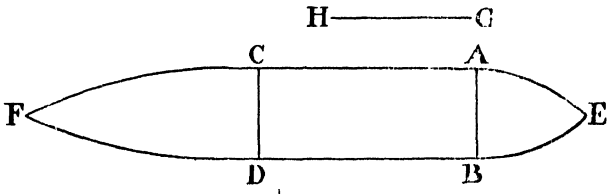

FIG. 20.

we have determined in this proposition, nearly*; that is, it will have the same ratio to the force with which the whole motion of the cylinder may be destroyed or generated, in the time that it is describing the length 4AC with that motion uniformly continued, as the density of the fluid has to the density of the cylinder, nearly."*

We may say that if a solid be of the shape ACFDBE, since it will uniformly accelerate the fluid, it will be a *shape of Least Resistance.*

William Emerson (*The Principles of Mechanics and the General Laws of Motion*, 1758), gives, "the construction of the fore part of a vessel, that will move through the water *with the least possible resistance.*" He shows how to draw the curves by the use of a table which he gives; but there is no indication offered how the figures in this table were

* This word "nearly" suggests that Newton had tried the experiment.

arrived at. Since, however, there is only one table of ordinates, ship-designing by Emerson's method would be a little crude, and also restricted, since the table only admits of one curve being laid down. The curve of the keel being the same as that of the curve at the water-line, the prow of the vessel would necessarily be nearly a point; and the design of all the vessels would be *exactly similar.*

I only refer to this mode of construction because the curve given by Emerson—except quite close to the bow of the vessel—*is also a parabola.* There is no comparison between Emerson's rough method of construction and Nystrom's complete and *practical method* of drawing the lines of a vessel of any given length and beam, or maximum cross-section. By Nystrom's method one can design any kind of elegant vessel, with any special details one may require.

M. L. Geoffroy (*l'Aéronaute,* Janvier, 1879), appears to have had very definite ideas on this subject, for he says: "In order that the motion [of the fluid] should be *uniformly accelerated* [*i.e.,* that the Principle of Least Action, or of Least Resistance, shall be satisfied] it is necessary that the prow of the moving body [*mobile*] should be formed by the use of generating curves *exercising a constant action* on the filaments or threads [*files*] of Molecules, which necessitates a variation of the curvature proportional to the path travelled [*au chemin parcouru*] by the Molecules *during equal intervals of time,* that is to say, *a Parabolic curve.*"

All the foregoing, doubtless, falls very short of practical *proof,* although it all points to the fact that a paraboloid of revolution will be the shape of the fore-part of the solid of Least Resistance. I say "fore-part," since it will be evident from what has been said previously, that the "after-part" of the body may be so designed as to, what I have called, "recuperate" some of the Energy transformed; by this I mean that some part of it may be re-transformed back from Kinetic Energy to Potential Energy, or Pressure, so that *on the balance,* the amount of the Energy transformed into the Kinetic form—which is what we have to pay for—and hence the Resistance, will be reduced.

In a correspondence that I had with one of our most eminent Mathematicians on this question of the shape of the

Least Resistance, he said that, of course, if I *assumed* "uniform acceleration," the generating curve of the solid would be a Parabola. I certainly *do* assume it; since, for the Energy transformed to be a *minimum*, it is necessary for this Energy to be transformed *uniformly*. This construction agrees with Newton's solid, just referred to, with paraboloidal ends, illustrated as Fig. 20 (page 203).

If the Principle of Least Action holds for a solid moving uniformly through a fluid, it appears logical to suppose that it will equally hold if the fluid is moving uniformly through the solid. In other words, either when the fluid moves *outside the solid* or when it moves inside it. Or, again, put in another way, the flow of a liquid round a thin circular plate should be very *similar* to the flow of the same liquid through a similar circular aperture in the thin wall of a tank.

The careful reader may, however, suggest that there is, apparently, what looks like an essential difference between the two cases, since in the case of the thin plate, the fluid, as was pointed out on page 144, is passing through an imaginary annular "opening" which is, as it were, "too small" for it. How it gets over this difficulty was, however, explained in Chapter XI.

In the case of the liquid flowing through the circular opening in the *thin* wall of a tank, it may be considered as if flowing into an imaginary cylinder which proves to be "too large" for it—which, in fact, it *cannot fill*. As is well known, the jet *contracts*, forming what is called the *vena contracta*. This contraction was, I believe, first pointed out by Newton in the *Principia* (Book II, Prop. 36, Case 1.).

How the out-flowing fluid "contracts"; how, in fact, it *actually flows* through the opening, has not, that I am aware, ever been investigated or explained at all satisfactorily. The subject is *extremely* interesting and important, but we cannot enter into this question here, beyond saying that the liquid "screws itself" through the aperture. It would take too long to explain this motion here, and I therefore reserve it for another occasion.

From Prop. 36, Book II, of the *Principia*, it would appear that Newton first conjectured that the fluid flowed "full

bore" from the orifice—or, *very nearly full bore.* This was, apparently, Newton's earlier view when the first Edition of the *Principia* was published. In the Second Edition, however, this was much modified, in the Scholium and Corollaries that follow. Having tried the experiment he found that the jet was very considerably contracted at a short distance from the aperture; he tells us that the diameters were in the ratio of 1 : 0·83 or 0·84.

Newton gave an explanation of this contraction, which is, however, now well known to be far from satisfactory.

It may be of interest to point out that the equation to the generating curve of Newton's "Cataract" would be y^4x = constant; the formula for a Hyperbola of a high order. Now, although water does not, as a matter of fact, flow out as Newton assumed in his original Prop. 36, there can, I think, be no doubt that if the fluid be suitably *constrained,* so as to make it flow like this, the shape of the constraining channel will be a Shape of Least Resistance. Experiment appears to confirm this view; the discharge when so constrained being much increased.

So far, in this discussion, we have only considered that kind of acceleration which is "spurt" (longitudinal) acceleration; but the reasoning should equally apply to what Karl Pearson calls "shunt" (transverse) acceleration. For example, let us suppose that an engineer wishes to connect two steam-pipes, or water-pipes at an angle, in such a manner that there shall be *the least loss of Energy*—a point which is not ordinarily thought of as much as it deserves; or suppose an irrigation engineer wishes to turn a canal through a certain angle with the *least loss of Head;* it is clear, from what has been said previously, that he must so arrange that the "shunt acceleration" shall be uniform. In other words, the connection must be made by the use of a parabolic curve.

I am, as I said, not aware that this has been properly recognized up to the present. The only case that I know of in England is connected with railway curves. To quote a somewhat recent article, Professor S. W. Perrott, *Some Problems of Railway Curves* (Trans., Liverpool Eng. Soc., 1917), speaking of "Transition Curves between Circular

Curves and Straights," says: "The sudden change from motion in a straight to that in a curved path has always been objectionable, from the point of view of safety, of resistance to hauling and also of comfort to passengers in trains." He gives the rate of acceleration as

$$\frac{v^2}{r} \div \frac{K}{v} = \frac{v^3}{Kr}$$

where v = velocity, r = radius of curvature, and K = length of car; and he says that "the Cubic Parabola has been very largely used in setting out Transition Curves."

Such curves would, I suggest, be better if made *entirely parabolic*, and not only *transitionally parabolic*.

In a number (about September, 1921) of *Le Miroir des Sports*, there is a description of a new motor racing track, built near Brescia, in Italy. On this track there are two long straight courses which are joined by "the famous Parabolic curve of a length of about 600 metres" [*la fameuse courbe parabolique*]. The Article further says that the parabolic curve allows the turn to be taken at the greatest speeds attainable by vehicles—*voitures* and *voiturettes*—from which the pilots are able to obtain the *maximum efficiency* [*le rendement maximum*].

At this point, a mere practical difficulty—that of drawing a Parabola tangent at *any two points*—may perhaps present itself. An engineer might possibly say that drawing a parabola between two points is easy enough, if you know its principal axis; Molesworth's and similar works show various ways of doing this; but to draw the parabola when you *do not* know where the principal axis is, is not quite the same thing. As this construction is not usually taught—it is not in *Molesworth*, though it ought to be—it is probable that comparatively few practical men know how to do it. A simple method may, therefore, not be out of place if described here. The solution depends upon a very interesting property of the Parabola and its tangents.

Let AG and BG (Fig. 21) be tangents to a Parabola, at A and B, intersecting one another in G. Then any intermediate tangent, such as CD, will cut the tangents AG and

BG, so that BC : CG :: GD : DA. Or, drawing DO, parallel to GB, and CO parallel to GA, we have

$$\frac{BO}{OA} = \frac{BC}{CG} = \frac{GD}{DA}.$$

As regards the practical application of this property, M. Brianchon (*Des Courbes de Raccordement*, Jour. de l'École Polytechnique, Cahier XIX, 1823) showed how this can be done, in a very simple manner; a manner so simple that any ordinary workman can appreciate it, and carry it out, since it requires no knowledge of Mathematics.

Let us imagine that B and A (Fig. 22) are the ends of straight pipes, or of straight canals, which it is desired to

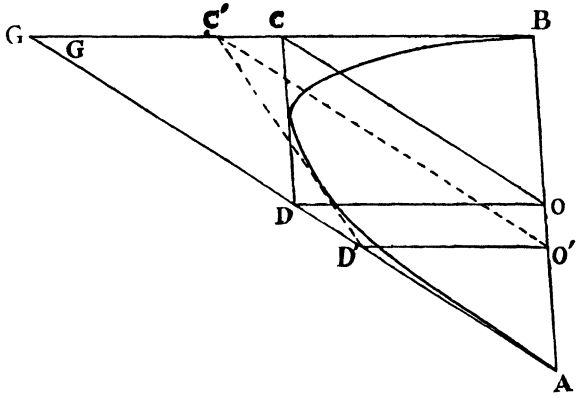

FIG. 21

join so that there shall be the least loss of Energy, or "Head," whilst the liquid is flowing from B to A along the connecting channel. Prolong the straight lines until they meet in G. Take B and A as the first tangent points. Divide BG and GA into the same (any) number of equal parts, say 5; number the divisions as shown in Fig. 22. Join the corresponding numbers: these lines will be the tangents to the required Parabola, as shown.*

* If the reader is specially inquisitive and wishes to find out the axis of this parabola, it can easily be done by taking advantage of another very interesting property of parabolas. Draw AB and bisect it in C. Join CG, which will be a line *parallel* to the principal axis of the parabola. From this the focus, directrix and origin of the parabola can easily be found. The reader is recommended to work this out for himself.

By making the connecting pipe follow this parabola, there will be the *least loss of Energy :* and there will, consequently, be the greatest flow of water ; similarly, by tracing a canal in this manner, there will be the *least loss of Head.* Further than this, the water in the canal will not be always endeavouring to destroy its channel, as it invariably does in canals where the curves are circular. The reason for this is that the Parabolic curve is the one which the fastest flowing filaments of a stream do, in fact, describe round the bends, provided always that the channel is not too restricted. If

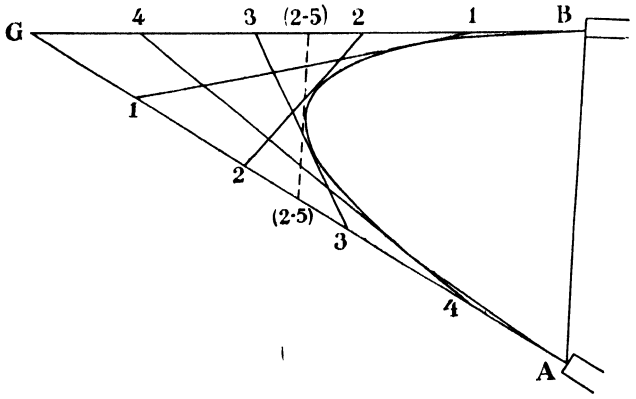

FIG. 22.

the channel should not be suitably traced, the stream endeavours to rectify the error. This can easily be seen by observing the fine line of foam which marks the line of its fastest flowing filaments.

In parenthesis, I may say that this line marks the highest point in any cross-section of the stream, as I explained in *Motion of Liquids:* in the diagram, however, I rather " toned down " this hump in the stream ; it should, really, come to a sharp point, caused by the intersection of the two Parabolas.

Some time after writing *Motion of Liquids,* I came across Jack London's *Smoke Bellew,* in which the author describes what he names the " Box Canyon," as follows : " On either side arose perpendicular walls of rock. The river narrowed

to a fraction of its width, and roared through this gloomy passage in a madness of motion that heaped the water in the centre into a ridge fully *eight feet higher* than at its rocky sides. This ridge, in turn, was crested with stiff upstanding waves that curled over, yet remained each in its unvarying place. The Canyon was well feared, for it had collected its toll of dead from the passing gold-rushers."

The "eight feet," I have italicized, seemed to me a large figure. Since, however, Jack London gives the length of the ridge, and the time taken in travelling along it, there was no difficulty in calculating what the height might be theoretically—which was sixteen feet—if the water was supposed to be at rest at the edge of the Canyon. Making allowance for the boat moving slightly faster than the stream, I feel convinced that this Canyon was actually described from observation, though I have never been able to find out the real name, or the exact position of this Canyon. That torrents, especially, *do* heap up the water in their centres is, I believe, well known and recognized now.

This brings my Chapters to a close: not that I have exhausted the subject, for I have done little more than touch the fringe of it, but I trust that I have said enough to cause the serious reader to think. As to the practical application, it is unnecessary to point out that, though most modern undertakings are so well designed at the present day that any *very great* improvement would be difficult to make, still there is, I think, room for many slight improvements in the design of the channels conveying steam, or water, say; and an increase of one per cent. in efficiency—the Energy being saved by the improved design of a channel—is not to be despised: and even a saving of only one quarter of one per cent. on each of, say, 100,000 channels, would total up to a very large amount.

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INDEX

ABSOLUTE, Motion, 53, 56, 58; Space, 52, 53; Time, 19, 53.
Acceleration, 39; rate of, 40.
Action, 84; and Reaction, 87; Sir J. J. Thomson's view, 162; as used by Relativists, 172.
Added Mass, pendulum experiments, 150.
Armstrong, on Education, 4.
Attributes and applicate quantities, 32.

BALL, W. W. ROUSE, on Force, 92.
Bayma, on Action and Reaction, 88; Force, 94; transfer of velocity, 89.
Boscovich, on Infinity, 19.
Brianchon, on drawing Parabolas, 208.

CAMPBELL, N., on Relativity, 58, 61, 63.
Carus, on Space and Time, 53, 54, 55.
Continuity, the conception of, in Fluids, 128.
Cox, on Momentum, 69; Newton's Third Law, 80; Vectors and Scalars, 26.
Creswell, D., on Geometrical and Analytical Methods, 10.
Curves, transition, 206.

D'ALEMBERT'S Paradox, 11.
Direction, Vectorial, 30.
Dynamics and Energetics compared, 8, 96, 110.

EDUCATION, on, Armstrong, 4; curse of, Gorst, H. E., 6; Huxley, 2; Swinburne, 4.
Effort, or Active Accident, 103; and Work, 105.
Energetics, historical note on, 108.
Emerson, Shape of Least Resistance, 203.
Energy, its nature, 3; its conservation, 46; Kinetic and Potential, 47; Measurement of, 48; Recuperation of, 196; Transformation of, 91; and Work, 100.
Ether and Light, 56, 62; its Rigidity, 123.
Euler, on Least Action, 175.

FLOW, theory of, 115, 118; model explaining, 117; past circular plate, 143.
Fluid Motion, through solid, 205; Resistance and Similitude, 132; do. Newton's Theory of, 130, 141.
Flying-boats, shape and step, 194.
Force, Dimensions of, 73; interpretation of idea, 92; Definition of, 65; Definition, Impressed Force, 93; Bayma, on, 94; as Action and Reaction, 78.

GALILEO, on Infinite divisibility, 20.
Gandillot, on DuBuat's Paradox, 165.
Glazebrook, on Newton's Second Law, 72.
Gramont, Experiments on Air Resistance, 164.

HEGEL, on Matter, 21.
Hicks, on teaching Mechanics, 2.
Huxley, on Education, 2.

IMPACT, 86.
Inertia, 81, 82.

LAGRANGE, on Fluid Motion, 127.
Least Action, problem involving, 180; Leibnitz on, 177; Mach, E., on, 177; Poincaré, H., on, 173.
Least Resistance, 75, 84, 92.
Leibnitz, on Action, 171.
Length and Distance, 24.
Lewis and Tolman, on Relativity, 55.
Locke, on Conceptions, 64.

MACAULEY, W. H., on teaching Mechanics, 3.
Mach, E., on Mass, 22; on Least Action, and the path of a projectile, 177.
Mass, and Matter, 20; and Speed, added Mass, 23.
Maupertius, on Least Action, 170.

Maxwell, on Vectors, 31 ; addition and subtraction of Vectors, 34 ; Acceleration, 40 ; Absolute and Relative, 52 53.
Momentum (quantity of Motion), 43 ; conservation of, 90 ; transference of, 89.
More, L. T., on Relativity, 59.
Motion, absolute, 53, 56, 58.

NEWTON, on Ether, 123 ; on Force, 67, 76, 77 ; on Parabolic form of Least Resistance, 202 ; Theory of Fluid Resistance, 130, 141, 151, 153 ; Space and Time, 19 ; Second Law (*also see* Playfair), 70 ; Second Law, Restated, 75, 84, 92 ; Third Law, 78 ; and Stokes' Law, 131, 134.
Nystrom, on Parabolic ship construction, 199.

PARABOLAS, methods of setting out, 207.
Parabolic ship construction, 199.
Paradox, d'Alembert's, 11.
Pearson, K., on Mass and Matter, 22 ; Accelerations, 40 ; Newton's Second Law, 74.
Perry, on teaching Mechanics, 1.
Planck, on Theoretical Physics, 13.
Playfair, on Newton's Second Law, 76.
Poincaré, H., on Energy, 100 ; on Least Action, 173.
Power (derived unit), 49 ; in Energetics, 107.
Pressure (derived unit), 44, 104.

QUANTA, Heat generated in, 116.
Quantities in Mechanics, 15.
Quotients and ratios, 14.

RANKINE, on Energy, 98, 101, 103 ; Effort and Work, 105–107.
Rate, conception of, 73.
Rayleigh, on Fluid Motions, 126 ; Rayleigh's Function, 135.
Recuperation of Energy, 196.
Relative Motion, 157.
Resistance and Principle of Relativity, 160.
Resistances deduced from Newton's Theory, 141.
Richardson and Landis, on Ratios and Quotients, 14 ; Vectors and Scalars, 27.
Rigidity, its meaning in Mechanics, 119 ; its relation to Viscosity, 121.
Russell, B., on Force, 84 ; on Least Action, 177 ; on Relativity, 50.

SHAPE of Least Resistance, Emerson's, 203 ; Newton's, 183.
Shear, its relation to Viscosity, 113.
Skin Friction, Theory of, 148.
Space, absolute, 52, 53 ; Conceptual and Perceptual, 16.
Speed, in Energetics and Dynamics, 39.
Spencer, on Matter, 21.
Spurt (change of speed), 42.
Stallo, on Force, 93.
Stokes, on Fluid Friction, 114.
Stress, condition of, 45, 79, 83.

TAIT, P. G., on Force, 66 ; on Shear, 113 ; on Rigidity and Viscosity, 120.
Thibault's experiments on Air, 164.
Time, absolute, 19, 53 ; Conceptual and Perceptual, 18.
Transition curves, form of, 206.
Tunny, its finlets, 197.

UNITS of Dynamics, 13.

VECTOR (point and mass), 42.
Vectors, addition and subtraction of, 34.
Velocity (derived unit), 37.

WEIGHT, as "Force," 94.
Work, its nature, 35 ; and Energy, 46.

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