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III. On Newton's "*Regula Tertia Philosophandi*." By the Rev. Professor CHALLIS, M.A., F.R.S., F.R.A.S.\*

THE Third Book of Newton's *Principia*, to which exclusively the title "De Mundi Systemate" is attached, contains at its beginning four Rules of Philosophizing, each accompanied by explanations. Of these rules, the first, second, and fourth, with their explanations, have been very generally accepted, and do not require special consideration. The Third Rule, which is enunciated in these terms, "The qualities of bodies which admit neither of increment nor decrement, and which pertain to all bodies on which experiments can be made, are to be considered qualities of universal bodies," is accompanied by special explanatory remarks and definitions respecting the ultimate qualities of bodies. The purpose of this communication is to indicate the necessity of accepting this rule in conducting physical theory, and to discuss the definitions Newton has appended to it.

The object I have in view requires commencing with the premise that all theoretical research for explaining experimental facts is carried on by means of *calculation*, which in essence is *reasoning* conducted by symbols of quantity. The application of reasoning by calculation for acquiring knowledge consists of three distinct processes :—(I.) Making hypotheses (*i. e.* foundations of calculation); (II.) deriving *equations* by means of the hypotheses from the data of proposed questions; (III.) solving the equations, according to previously ascertained rules, for obtaining the answers to the questions. In order to establish the truth and necessity of this method of theoretical inquiry, I shall bring under review the various stages of its application, beginning with the earliest as regards both the mathematics employed and the physical data. As there will be occasion to revert repeatedly to the several parts, I shall designate them respectively as parts (I.), (II.), and (III.).

It is well known that arithmetical calculation, such as that now practised, had its origin in India, where "the device of place," according to which the value of a figure is determined by its place relative to other figures in the same row, was invented. This expedient, which appears to have been unknown to the Greeks and Romans, and without which it was hardly possible that much progress could be made in calculation, was imported into Europe by the Arabians. Whether or not they derived *Algebra* from the same quarter, it is certain that the Arabic name, which properly signifies the oppo-

\* Communicated by the Author.

sition of the two sides of an equation, not only indicates advancement in numerical calculation, but shows also that the Arabians put in practice the above-defined processes (I.), (II.), (III.) for acquiring information by calculation. In fact, the formation of the equation implies, first, the making of an hypothesis (putting  $x$  for the unknown quantity), thence deriving the equation by arithmetical rules from the data of the question, and, thirdly, the possession of some means of solving the equation. The *rules* of calculation are obtained by means of numerical indications of quantity, it being necessary for that purpose to know the relative magnitudes of the quantities. Letters might thence be put in place of numbers in such manner as to generalize all particular instances of the application of the rules; and the result would be general arithmetic, but not algebra. The essential principle of algebra, as now understood, is to reason according to the arithmetical rules without knowing whether  $a$  represents a greater or less quantity than  $b$ . The Arabians, in forming and solving equations, do not appear to have put letters for known or given quantities. This step was first taken by Vieta (in the latter half of the sixteenth century), necessitating the use of the signs of operation  $+$  and  $-$ , and, in logical sequence of the use of such symbols according to ascertained rules, forms of expression which may be called representations of *negative* quantity and *impossible* quantity. From this time the science of general algebra made rapid progress, our countrymen Oughtred, Harris, Wallis, and Newton being conspicuous among its promoters. When by their labours the rules of operating with indices and expanding in series were established, the way was cleared for discovering a new process in the application of calculation for answering questions. This advance was first made by Newton, whether as exhibited in his method of prime and ultimate ratios or that of fluxions. Leibnitz invented the differential calculus, which is the symbolic form of the new calculation which is most suitable for general application. By means of the differential calculus equations are formed the answers to which are no longer values of unknown quantities, but forms of unknown functions of variable quantities.

In the meantime discoveries were made by observation and experiment which called for the application of the new calculus. Kepler ascertained by observational astronomy his three famous laws of the planetary motions without being able to assign any reasons for them. Galileo determined experimentally (in opposition to the Aristotelians) the laws of the acceleration of bodies falling towards the earth by the constant

action of gravity, and, what is much more, the parabolic motion of a projectile. The latter fact is indicative of the law that the acceleration in the direction in which gravity acts is the same whatever from other causes may be the movement of the body acted upon. It is here proper to remark that experimental determinations may be classed under two different heads, some being necessary as *foundations* of theoretical research, and others serving to verify and extend theoretically calculated results. Of the former class are Galileo's theorems respecting the acceleration of falling bodies and the parabolic motion of a projectile. All that Newton and Laplace wrote on physical astronomy depends on hypothetically adopting these two theorems. Newton, in his First Book, repeatedly expresses his indebtedness to the second, calling it “Galileo's Theorem.” Kepler's observations belong to the other class; and his name occurs in the Third Book, where Newton specially refers to the law of the planetary distances as a *phenomenon* to be accounted for by the theory of gravitation. The laws of the lunar and planetary motions being determinable since Newton's time by theoretical investigation, there was no more occasion to employ for that purpose such observations as those of Kepler, it being the particular province of theory to demonstrate *laws*, while observations are required for ascertaining the numerical values of the constants which the theoretical formulæ involve. The astronomical observer of the present day simply determines as accurately as he can the celestial places of the sun, moon, and planets, and gives his results to the theoretical computer to be dealt with according to his requirements. Flamsteed, from not understanding what he considered to be Newton's crotchets, attempted to discover the laws of the lunar inequalities solely by observation. There is reason to think that something like Flamsteed's misapprehension of the relation between the respective provinces of theory and experiment exists at the present time.

After these preliminaries, I am prepared to enunciate the parts (I.), (II.), (III.) of *Physical Astronomy*. The differential calculus satisfies the demands of physical astronomy so far as regards the operations which calculation has to perform. Part (I.) consists in making these three hypotheses:—(1) that every particle of matter attracts by the force of gravity every other particle; (2) that the action varies with the distance between the particles according to the law of the inverse square; (3) that the action conforms to the law of Galileo's Theorem. Part (II.) consists in deducing from these hypotheses what is equivalent in modern analysis to forming a

differential equation involving an unknown expression or function of a variable quantity. This was Newton's most important discovery, which his contemporaries, although, as he says, they were acquainted with the law of the inverse square, in vain attempted to make. Part (III.) is composed of determinations of unknown expressions by virtual solutions of differential equations, together with physical inferences drawn from the solutions, and comparisons of these results with facts of observation. In short, in analytical language Newton effected the solution of a differential equation of the second order between two variables, whereby he not only accounted for Kepler's laws, but also demonstrated, *à posteriori*, by the comparison of calculation with observation, the hypothesis of the inverse square and that of Galileo's Theorem.

The course of inquiry by which the science of physical astronomy has been established forms a paradigm to be followed in the more advanced department of physical research embracing the theories of light, heat, force of gravity, electricity, galvanism, and magnetism. Here also Newton has pointed out the way of proceeding, having stated in his Third Rule of philosophizing, and at the end of the Third Book, the primary *hypotheses* on which such research must be founded. These constitute Part (I.) of this new stage of physical philosophy. They consist of two kinds, namely, hypotheses absolutely true, and hypotheses which admit of being proved to be true by comparison of mathematical results derived from them with facts of observation. To both kinds Newton's dictum, "*hypotheses non fingo*" (I do not arbitrarily make hypotheses), applies. The several hypotheses may be stated as follows. The "least parts" (atoms) of bodies have only sensible qualities, as form, magnitude, mobility, and intrinsic inertia. These are absolute and necessary qualities, inasmuch as their existence is immediately conveyed to us by our senses, and apart from them we cannot conceive of matter. Newton adds the quality of "impenetrability" relative to the least parts, which signifies that they admit of no change of form or magnitude. In fact, they would otherwise not be conformable to that part of the enunciation of the Third Rule, which states that the ultimate qualities of bodies cannot be increased and diminished (*intendi et remitti nequeunt*). Moreover he does not allow the possibility (or conceivableness) of one body acting upon another without intervening substance; and in the scholium at the end of the *Principia* he adverts to the agency of a certain "very subtle" medium (the æther), pervading the grosser bodies. On adopting these Newtonian principles for the purpose of extending the boundaries of phy-

sical philosophy, I have further supposed, for the purpose of applying mathematical reasoning, that the atoms are impenetrable *spheres*, that the æther is a perfect fluid of constant intrinsic elasticity, and that it varies in pressure in exact proportion to variations of its density. The æther is thus defined by the equation  $p = kp$ ,  $p$  being the pressure,  $p$  the density, and  $k$  an absolute constant. These three hypotheses are of the kind which have to be verified by comparisons of deductions from them with experimental facts.

Bearing in mind Newton's monition not to make hypotheses contrary to “the tenor of experience,” or to deviate from “the analogy of nature,” I have added the above-stated hypotheses on the following grounds. First, since experience has brought to our knowledge no other kind of *masses* of matter than such as are atomically constituted, and since a universal *plenum* under any natural conditions is inconceivable, I suppose the æther to have a uniform atomic constitution—that is, to consist of minute atoms all of the same size and uniformly distributed. Under this constitution it may be conceived to be susceptible of *variations of density* from point to point, and to be capable of pressing in proportion to the density. Again, in the formula  $p = kp$  as applied to the æther, there can be no variation of the factor  $k$ , because, as the existence of the æther is supposed to be the primary condition of physical force, there are no ulterior forces to which a variation of its elasticity can be referred. This view makes it necessary to demonstrate that all the forms of physical force are modes of pressure of the ætherial medium. To show this has been the express object of physical researches (subsequently referred to in this communication) on which I have been engaged during many years. Further, it is to be said that a system of philosophy which rests wholly on the indications of sensation and experience can admit of no other kind of force than *pressure*, because of this force we have immediate *sensible* cognition (as when we press with the hand against any substance); and the same assertion cannot be made respecting any other kind of force. In support of the hypothesis of the spherical form of the atom I might cite another of Newton's sayings, “*natura solet esse simplex*,” the form of the sphere being defined by a single constant. Also the fact that a mass of water remains the same in magnitude and quality after any amount of change of the relative positions of its parts, implies that each ultimate part has the same relation to surrounding parts after as before the disturbance, and that the relation is the same in all directions from the ultimate part. These views may be regarded as justifying the hypothesis of the spherical

form of the atom, but only as a ground for mathematical reasoning whereby the truth of the hypothesis may be tested.

Before proceeding to Part (II.) of the stage of physical inquiry now under consideration, a certain mathematical difficulty has to be stated and cleared up. In physical astronomy, as we have seen, Newton overcame the difficulty of discovering the calculation proper for determining the motion of a single particle acted upon by given forces. Since, according to the foregoing hypotheses, the pressure of the ætherial medium performs an essential function, we have now to discover the *hydrodynamical* principles and calculations applicable to the determination of the motion and pressure of a fluid particle in juxtaposition with other particles, which is a preliminary difficulty of the same kind as that just mentioned. The former process required the formation of a differential equation containing two variables; this requires differential equations containing at least three variables, and is therefore of greater complexity. The discovery and treatment of two such equations, one expressing the principle of constancy of mass, and the other derived from an application of D'Alembert's Principle, were effected by the researches of Euler, Lagrange, and Poisson. On attempting to advance to Part (II.) by employing these equations, I was stopped by finding that Poisson had deduced from them, in the case of a fluid the pressure of which varies proportionally to its density, an equation from which I could strictly infer that the same fluid particle might be at rest and have a maximum velocity at the same moment of time (see *Phil. Mag.* for June 1848, p. 496, and my 'Principles of Pure and Applied Calculation,' p. 195). This was evidently a *reductio ad absurdum* absolutely demanding a reconsideration of the hydrodynamical principles. As those principles on which the two equations are founded are unquestionably true, the only logical inference is that they are *insufficient*. It consequently occurred to me that, besides the equation expressing constancy of mass, one was required for expressing *continuity of the motion*. Accordingly I have proposed to found such an equation on the following principles:— (1) that the course of a given particle is so far continuous that the directions of its motion in successive instants cannot make with each other a finite angle; (2) that the directions of the lines of motion in each elementary particle are at each instant normals to a surface of continuous curvature, which is either of finite or infinitely small extent, the total surface of displacement being conceived to be such that, however it might be composed of discontinuous parts, no two of its contiguous elements are inclined to each other by a finite angle.

In proof of principle (1) it suffices to say that a finite change of direction of the motion of an element in an infinitely small interval could only be produced by infinite force, which, by the nature of the inquiry, is excluded. The principle (2) of geometrical continuity, being supposed independent of change of time and place, requires to be expressed as such, like that of constancy of mass, by a differential equation, the mathematical investigation of which I proceed now to give.

If  $u, v, w$  be the velocities at the point  $xyz$  at the time  $t$  in the directions of the axes of  $x, y, z$ , it is known that in the case supposed of surfaces of displacement of continuous curvature,  $u dx + v dy + w dz$  is either integrable of itself or by a factor. Hence if  $\frac{1}{\lambda}$  be the factor, when one is required, and  $(d\psi)$  be put for the exact differential  $\frac{u}{\lambda} dx + \frac{v}{\lambda} dy + \frac{w}{\lambda} dz$ , we shall have  $(d\psi)=0$  for a surface of displacement. The above stated principle of continuity requires that such an equation should apply to each element of the fluid at successive instants. This condition is expressed by the formula  $(d\psi) + \delta(d\psi)=0$ , the sign of variation  $\delta$  having reference to change of position of the element in space and time. Hence as  $(d\psi)=0$ , we shall have also  $\delta(d\psi)=0$ , and, on account of the independence of the signs of operation  $\delta$  and  $d$ ,  $(d \cdot \delta\psi)=0$ , whence by integration  $\delta\psi = \phi(t)\delta t$ . The complete variation  $\delta\psi$  of the function  $\psi$  in the time  $\delta t$  is, by the Calculus of Variations,

$$\frac{d\psi}{dt} \delta t + \frac{d\psi}{dx} \delta x + \frac{d\psi}{dy} \delta y + \frac{d\psi}{dz} \delta z,$$

$\psi$  being a function of  $x, y, z$ , and  $t$ . Since the variations  $\delta x, \delta y, \delta z$  apply to the change of position of the given particle in the small interval  $\delta t$ ,  $\delta x = u\delta t$ ,  $\delta y = v\delta t$ , and  $\delta z = w\delta t$ . Also we have  $\frac{u}{\lambda} = \frac{d\psi}{dx}$ ,  $\frac{v}{\lambda} = \frac{d\psi}{dy}$ ,  $\frac{w}{\lambda} = \frac{d\psi}{dz}$ . Hence by substitution, and, after rejecting the common factor  $\delta t$ , supposing  $\phi(t)$  to be included in  $\frac{d\psi}{dt}$ , we obtain

$$\frac{d\psi}{dt} + \lambda \left( \frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0. \quad . \quad . \quad (A)$$

This is the required equation of continuity. By this equation the mathematical theory of the motion of a fluid is completed, inasmuch as the three differential equations, together with the equations  $u = \lambda \frac{d\psi}{dx}$ ,  $v = \lambda \frac{d\psi}{dy}$ ,  $w = \lambda \frac{d\psi}{dz}$ , and a given relation between the pressure  $p$  and the density  $\rho$ , furnish seven equa-



tions, which suffice for determining the seven unknown quantities  $\psi$ ,  $\lambda$ ,  $u$ ,  $v$ ,  $w$ ,  $p$ ,  $\rho$  as functions of  $x$ ,  $y$ ,  $z$ , and  $t$ . (See another investigation of the same equation in pp. 174 and 175 of the work already cited.)

As the equation (A) serves for determining  $\lambda$  as a function of  $x$ ,  $y$ ,  $z$ , and  $t$ , and as I have not been able to discover any fault in the reasoning by which it was reached, I regard it as giving proof of the reality of motion for which  $udx + vdy + wdz$  is integrable by a factor. In short, reasons may be adduced

for concluding that the factor  $\frac{1}{\lambda}$  is applicable to cases in which the motion is not distinctively that of a fluid, but such as a fluid is capable of if conceived to be composed of infinitely small parts that are solid—such, for instance, as uniform rectilinear motions parallel to a given plane and varying as some function of the distance from the plane, or uniform motions of revolution of infinitely thin cylindrical shells with velocities varying as some function of the distance from a common axis, or steady spiral motions composed of these two kinds. So far as the motion partakes of that which pertains to a solid, it must be determinable from the data of the problem by separate treatment, and has to be eliminated. For the remaining motion  $udx + vdy + wdz$  is integrable of itself, because this analytical circumstance specially indicates that the motion is such as pertains to a fluid. Hence the reasoning (in the Phil. Mag. for March 1851, p. 232, and June 1873, p. 436, and in the 'Principles of Applied Calculation,' p. 186) from which I have inferred that the motion is rectilinear when  $udx + vdy + wdz$  is an exact differential, and have thence attempted to get rid of the difficulty resulting from the before-mentioned *reductio ad absurdum*, cannot be maintained. I have recently ascertained that the origin of the difficulty admits of being simply stated as follows. In the reasoning by which the rate of propagation in a fluid defined by the equation  $p = a^2\rho$  can be shown to be such as to lead to the above-mentioned absurdity for a particular form of the arbitrary function, it is assumed that the lines of motion are all *parallel* to a fixed plane, or that the surfaces of displacement are planes. (See Phil. Mag. for Jan. 1851, example I., p. 34, and 'Principles &c.' example I., p. 193.) Now in that case, as is evident, the motion is not such as is peculiar to a fluid, but of the kind for which, as above said,  $udx + vdy + wdz$  is integrable by a factor, whereas in the reasoning that differential expression is assumed to be integrable of itself. This contradiction accounts for the *reductio ad absurdum*.

In order to clear up the difficulty, I have supposed that *rec-*

*tilinear* propagation takes place along an *axis*, and that the contiguous motion is such as only a fluid is capable of. To express these conditions, it suffices, after taking the straight line of propagation for the axis of  $z$ , to assume that

$$(d \cdot f\phi) = udx + vdy + wdz,$$

$f$  being a function of  $x$  and  $y$  only, and  $\phi$  a function of  $z$  and  $t$  only. For, on these suppositions,

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz};$$

so that if the function  $f$  be such that  $f=1$ ,  $\frac{df}{dx}=0$ , and  $\frac{df}{dy}=0$ , where  $x=0$ , and  $y=0$ , the axis of  $z$  will evidently be an axis of the motion. On reasoning from these antecedents no contradiction is met with like that which occurred in the previous method; and the reasoning is proved to be legitimate by actually conducting to a definite form, expressed in series, of the function  $\phi$ , and also to a series of definite form for  $f$  in case the motion be a function of the distance from the axis for any given value of  $z$ . The series for  $\phi$  is the harmonic series *assumed* by Helmholtz and other physicists in the mathematical theory of music. As the motion indicated by this series is not supposed to be dependent on a particular mode of disturbance, it evidently should be derived mathematically from the initial hydrodynamical definitions. This is what is done by the above-stated course of reasoning. The other factor  $f$  differs very little from unity in aerial vibrations; but in a medium of great elasticity, such as we have supposed the æther to be, it is of special significance, serving to account for *transverse* vibrations in the undulatory theory of light, and the distinction between common and polarized light. As experience shows that polarization is a quality of light depending on the constitution of the medium by which the luminous vibrations are transmitted, being producible under a great variety of extraneous conditions, it is an important confirmation of the present reasoning that it is capable of deducing transverse vibrations from the original definition of the æther.

By the same course of reasoning, the velocity of propagation is found to be  $\kappa a$ ,  $\kappa$  being a determinable numerical constant. Having concluded that my attempt to calculate the value of  $\kappa$ , contained in the Phil. Mag. for February 1853, was erroneous, I made another in the Phil. Mag. for May 1865, p. 329, which I have introduced into the ‘Principles’ &c. prop. xiv. pp. 214–224. The theoretical value of the

velocity of sound thence derived is 1109·3 feet per second, which exceeds by 17·5 feet the value obtained experimentally by Dr. Schröder (Phil. Mag. for July 1865, p. 47).

It will here be proper to make some remarks on methods that had been previously employed for determining theoretically the velocity of sound. The reasoning adopted by Newton, and afterwards by Laplace, fails to give the true theoretical value, because it is vitiated by involving the contradiction which I have indicated above. Laplace, assuming that from hydrodynamical considerations no value different from the quantity  $a$  was deducible, proposed to account for the difference between observation and theory by the effect of development of heat and cold produced by the condensations and rarefactions of the aerial vibrations. He accordingly sought, quite logically, to establish this explanation on a theory of heat. I do not suppose that any physicist now accepts that theory, inasmuch as the explanation of the above-mentioned difference is made to rest on some gratuitous hypothesis which is not supported by any reference to an antecedent theory of heat. For instance, when it is said that the acceleration of the rate of propagation is due to slow dispersion of the developed heat and cold, no evidence is adduced to show that this is a *vera causa* in unconfined air. From a theory of heat which I have founded on Newton's *à priori* hypotheses, I have inferred that the dispersion is so rapid as to produce little or no effect on the rate of propagation; and this result accords with the value of  $\kappa$  deduced mathematically, as said above, from the properties of a fluid defined by the equation  $p = a^2 \rho$ . (See the discussion of this question, under the head of "Mathematical Principles of Physics," in pp. 472-474 of the before-cited work.)

After the foregoing rectification of the principles of hydrodynamics, the way is clear for proceeding from Part (I.) of general physics to the consideration of Parts (II.) and (III.). The former of these two parts consists of formations of equations applicable to the several theories of light, heat, gravitation, electricity, galvanism, and magnetism; and the latter comprises deductions from the solutions of the equations and comparisons of the results with experiment. Respecting the investigations belonging to these two divisions, I can only refer to my own mathematico-physical productions, because, as my contemporaries have in no instance adopted the Newtonian hypotheses of Part (I.), they are logically debarred from entering upon Parts (II.) and (III.). In a communication like the present it would not be possible to advert in detail to the many problems I have undertaken to discuss under these

two heads; so that I can do no more than direct the reader's attention to the investigations contained in my ‘Principles of Pure and Applied Mathematics,’ published in 1869, and in two smaller works, one entitled ‘Essay on the Mathematical Principles of Physics,’ and the other ‘Remarks on the Cambridge Mathematical Studies and their Relation to Modern Physical Science,’ published respectively in 1873 and 1875. These investigations are founded, for the most part, on numerous communications made to the Philosophical Magazine. I am well aware that, as was likely to happen in so extensive an undertaking, there are many imperfections and errors in these productions (some of which I have rectified in later communications to the Phil. Mag); but since the course which they follow rests on Newton's authority, and, as I think I have shown in this communication, is pointed out by the antecedents of physical philosophy, I cannot but be of opinion that these works, or one embodying like views, will eventually have to be regarded as holding the same place relative to general physics as Newton's *Principia* holds with respect to physical astronomy.

I shall here introduce a few results obtained under Parts (II.) and (III.), selecting such as may serve to justify the opinion above expressed as to the necessity of the course of philosophy I have been advocating.

(1) The distinction between common light and polarized light is indicated by means of the function  $f$  already defined, which for common light is a function of the distance  $r$  from the axis of propagation independently of any special mode of disturbing the æther, and for polarized light a function of  $x$  and  $y$  depending on arbitrary disturbance. These results agree with experimental facts.

(2) The dynamical action of ætherial vibrations on a small sphere, when terms of the second order are taken into account, is capable of producing, according to differences of circumstances, attraction or repulsion of the sphere, and thus of accounting for atomic repulsion, molecular attraction, and the attraction of gravity.

(3) It may be demonstrated that no particle of the æther, supposed to be of unlimited dimensions, can be transferred across a plane fixed in space so as permanently to alter the quantities of fluid on the two sides of the plane. Consequently the motions are either vibratory or in re-entering currents. To the former motions the phenomena of light, heat, molecular adhesion, and gravitation are referable, and to the latter the phenomena of electricity, galvanism, and magnetism.

(4) On the same principles, the existence of a mechanical equivalent of heat is accounted for (see 'Principles &c.' pp. 469 and 481).

(5) It may plainly be demanded of this advanced department of physical philosophy to give reasons for the qualities of the three hypotheses on which, as before stated, the science of physical astronomy rests. It affords, in fact, the following reasons:—(1) The universality of gravitation is a direct consequence of its being a mode of action of a universal medium, the æther. (2) The gravitation law of the inverse square is mathematically derived from the dynamical action of ætherial undulations of large magnitude on an atom taken to be a small sphere. The most complete investigation of this law that I have succeeded in giving on hydrodynamical principles, I consider to be that contained in arts. 31–38 of a communication on Attractive and Repulsive Forces, in the Phil. Mag. of September 1872. (3) In art. 39 of the same communication, an argument embracing the *squares* of the velocities of the æther is adduced, from which the *coexistence* of the translatory effects of all forces referable to the action of ætherial vibrations is inferred. Hence, after accounting for universal gravitation and the law of the inverse square, Galileo's Theorem, in its most general acceptation, is an immediate consequence of the above result.

If it should be inquired whether the hypotheses of general physics, like those of physical astronomy, admit of being derived from ulterior conditions, I should decidedly say they are not so derivable. For as the philosophy of general physics consists of fundamental principles perfectly intelligible from sensation and experience, and of mathematical deductions therefrom, no further research is either needed or possible, the results being reached by means whereby alone complete human knowledge of nature is attainable. These physical principles must therefore be referred to causes that are "not mechanical" (to use Newton's expression), and may consequently exist by the immediate Will of the Author of the Universe. It may further be remarked that in this scheme of philosophy no place is left for the exercise of the *imagination*; and accordingly Newton has laid down the rule (not enough attended to by some modern physicists), "*somnia temere confingenda non sunt.*"

It is right that in connexion with the foregoing views I should advert to the many excellent treatises on physics which have been published in recent times both in England and on the Continent. These, one and all, are devoted to establishing physical facts, and expressing the laws by which they are

governed by mathematical formulæ. This necessary preliminary department of physical philosophy, which has been handled with admirable skill and exactness in those treatises, evidently differs in its character from Newton's mathematical *Principles* of Natural Philosophy. French writers, with their usual attention to logical accuracy, name that department *La Physique* (Physics), and carefully distinguish between ascertaining laws by such means, and referring them to *a priori* principles. The deduction of laws from experiment and observation, and expressing them by mathematical formulæ, is often called *theory*; and such it is in a subordinate sense. But theory in its most exact and complete sense consists in giving *reasons* for experimental laws by means of mathematical reasoning founded on intelligible primordial *principles*. This is the department I have taken up, in accordance with Newton's example and anticipations, being of opinion that his *Principia* has established the presumption that laws, just because they are *laws*, are proper subjects of human research and demonstration.

My physical investigations may be classed under two heads:—those relating to the principles and processes of Hydrodynamics, which I commenced in the Philosophical Magazine as far back as the year 1829, and have carried on, not always successfully, up to the present time; and those which treat theoretically of the laws of the physical forces on the basis of the Newtonian hypotheses, beginning with a Mathematical Theory of Heat contained in the Philosophical Magazine for March 1859. Among this class of researches I beg to call attention more particularly to the article on Newton's “Foundation of all Philosophy,” in the Philosophical Magazine for October 1863, p. 280. As I intend the present communication to be a kind of *résumé* of the portions of my productions which I consider to be most conducive to the progress of theoretical physics, and as probably I may be unable to make any further efforts of the same kind, I take this opportunity for expressing the opinion, which I have long entertained, that injustice has been done to Newton's scientific fame by the persistent neglect, and even opposition, with which modern physicists have treated the parts of his Book III. which have reference to the future of theoretical philosophy, and may be considered to justify his naming that Book *Mundi Systema*, those parts being, according to my judgment, among the most remarkable proofs of the greatness of his genius.

Cambridge, November 17, 1879.

*Postscript*, November 26, 1879.—The theoretical rate of  
*Phil. Mag.* S. 5. Vol. 9. No. 53. Jan. 1880. D

propagation of sound, which, as stated in the foregoing communication, was obtained in the *Philosophical Magazine* for May 1865 (p. 329), and introduced into the 'Principles, &c.,' prop. xiv., pp. 214-224, was derived from an exact integral of the equation

$$\frac{d^2 f}{dr^2} + \frac{df}{r dr} + 4ef = \frac{f}{4r^2},$$

namely,

$$f = (4\pi r \sqrt{e})^{-\frac{1}{2}} \cos \left( 2r \sqrt{e} - \frac{\pi}{4} \right).$$

I argued that the term  $\frac{f}{4r^2}$  might be omitted for very large values of  $r$ , as being incomparably less than the other terms by reason of the denominator  $4r^2$ . It has, however, very recently occurred to me that under the same circumstances the term  $\frac{df}{r dr}$  is incomparably less than either  $\frac{d^2 f}{dr^2}$  or  $4ef$ , as might readily be inferred from the given expression for  $f$ . The integral I employed was therefore an approximation, for large values of  $r$ , to that of  $\frac{d^2 f}{dr^2} + 4ef = 0$ , but not, as I supposed, to that of

$$\frac{d^2 f}{dr^2} + \frac{df}{r dr} + 4ef = 0.$$

In fact it may readily be shown that the latter equation is *exactly* satisfied by the solution,

$$f = \frac{c}{r} \cos \left( \pi r \sqrt{e} - \frac{\pi}{4} \right),$$

for the special values of  $r$  which make the cosine vanish, and for all other values with increasing approximation as  $r$  is greater, and that these special values are separated by the constant interval  $\frac{1}{\sqrt{e}}$ . These results confirm those obtained

by the very different investigation I gave in the Number of the *Philosophical Magazine* for February 1853, which I was induced to relinquish only under the above-stated misapprehension as to the applicability of the former expression for  $f$ . After full consideration I have not been able to discover any fault in the reasoning of that investigation. The rate of propagation it gives is  $a \left( 1 + \frac{4}{\pi^2} \right)^{\frac{1}{2}}$ , which, if we take the value of  $a$  to be 916,322 feet, as adopted by Sir John Herschel, we obtain for the velocity of sound 1086.25 feet, which is only

3·17 feet less than the experimental value 1089·42 feet deduced by the same author from a large number of observations. For these reasons I consider the theoretical value of the velocity of sound to be

$$a\left(1 + \frac{4}{\pi^2}\right)^{\frac{1}{2}},$$

as deduced exclusively from hydrodynamical principles, such as I have defined them to be in the foregoing communication.

J. CHALLIS.

IV. *On a Suggestion as to the Constitution of Chlorine, offered by the Dynamical Theory of Gases.* By A. W. RÜCKER, M.A., Professor of Physics in the Yorkshire College, Leeds\*.

IF a gas of density  $\delta$  consists of molecules each of which possesses  $m$  degrees of freedom, and if also the intermolecular forces are negligible, the specific heats at constant pressure ( $c_p$ ) and at constant volume ( $c_v$ ) are connected by the two well-known equations

$$(c_p - c_v)\delta = \cdot 0694, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{c_p}{c_v} = 1 + \frac{2}{m + e}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where  $e$  is a quantity which depends upon the potential energy of a molecule. Hence, if  $c_p$  is given by experiment,  $c_v$  can be calculated from the first of these equations; and then  $m + e$  is known from the second.

The accuracy of the value of  $m + e$  thus deduced will depend upon that of  $c_p$ , and on the legitimacy of the application of the two equations to the gas or vapour under consideration.

With respect to the first of these points, it may be remarked that E. Wiedemann has recently (*Pogg. Ann.* Bd. clvii. p. 1, 1876, and *Wied. Ann.* Bd. ii. p. 195, 1877) determined the specific heats at constant pressure of 14 out of the 35 gases and vapours studied by Regnault. The difference between the results of the two investigators amounts in two cases only (ethylene and ammonia) to 6 per cent.; in three cases it is about 5 per cent., and in all the others less. Thus even on the assumption that the later experiments are absolutely correct, it follows that Regnault's numbers may be trusted to 6 per cent. His results, however, can only be taken as true for the particular temperatures at which the experiments were made, as Wiedemann shows that in all but the most perfect

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