

this late date. I can still only repeat here what I said before, as follows:—

"My idea is, briefly, that the first depression occurred as the storm passed on its westward track, followed by the usual shift of wind to the northward. Along this branch of its trajectory its severity was probably not quite so great as it was later, and the force of its southerly winds was masked by the mountains on the island of Upolu; possibly careful observations of the rapidity of motion and the character of the clouds, or of the state of the sea off the harbour, might have indicated a severe storm, but this does not appear from the evidence at hand, though well worth considering. During its recurve the hurricane probably increased in intensity, the barometric depression at the centre deepening, and thus causing the second depression observed at Apia, which was slightly deeper than the first, although the centre itself was really at a greater distance than on the previous day."

Unfortunately, as stated above, we have no very definite data regarding the severity of the storm before it reached Apia, although I must say that a barometric depression of 0.76 inch below the normal in the tropics is very suggestive of the presence of a fully developed vortex, and it seems more than likely that if the harbour had not been well sheltered to the southward the southerly gales would have been quite as severe as those from the north. The only early data that we have, other than the reports from Apia, are contained in the following very brief report from the American schooner *Equator*, Captain Reid:—

"March 14 (Samoa date), lat. 12° 00' S., long. 170° 50' W., wind S., S.W., W., N.W.; thick, squally; hove to.

"March 15 (Samoa date), lat. 13° 00' S., long. 170° 40' W., wind N.W.; fierce gale; squalls; heavy sea."

This, although brief, seems to indicate that it was a fully developed hurricane that was approaching Apia, and I am inclined to the opinion that such was actually the case.

Mr. Blanford will surely admit that it is one recognized peculiarity of tropical hurricanes to reduce their speed of translation whilst they are recurving, and he will thus admit also that such recurvature accounts, in the present case, for the duration of the northerly gale at Apia. So far as the sharpness of recurvature is considered, I would say that the report of the *Equator* is sufficiently vague to allow us to draw a curve with a somewhat less sharp recurvature, and I should myself have done so but for the single fact that a hurricane approaching from a more northerly direction must have sent a northerly or north-easterly swell into the harbour that would have been felt to at least such an extent as to be noted in the log of one or more of the vessels there, but I have looked in vain for any such remark, until after the shift of wind on the afternoon of the 15th.

In conclusion, I take pleasure in expressing my obligation to Mr. Blanford for his interesting and able discussion of the subject, which, even with all the data at hand, has still many perplexing features. The principal object of this preliminary publication was to elicit comment and discussion, and it will be very gratifying if other authorities will give us the benefit of their experience and suggestions.

EVERETT HAYDEN.

Hydrographic Office, Washington, D.C.,  
January 4.

THE distinction to which allusion was made in the passage quoted by Mr. Hayden is not, as he seems to have understood, that between tropical and extra-tropical storms when fully formed, but between the circumstances of their respective formation. These are that, in the former, the cyclonic circulation of the winds is preceded by much irregular action, which sometimes extends over a considerable area. Within this area there are local squalls and shifts of wind, with heavy rainfall, but the action is not for some time definitely concentrated and cyclonic. This preliminary stage does not appear to obtain in the storms of the temperate zone, where the deviating effect of the earth's rotation is so much greater than in low latitudes, and indeed, if we accept the views of Werner Siemens and Prof. Hann, the cyclonic circulation is the cause and forerunner of the storm. I cannot think it probable that a vortex, once fully formed, and travelling towards higher latitudes, should recurve so sharply as to produce a fall of the barometer on two successive days (with a rise in the interval) at the same place, simply by twice passing in its vicinity. To effect this, the

recurvature must, as I apprehend, describe an angle considerably less than a right angle, and of such I know of no example among tropical cyclones. At the same time, my own view was put forward merely as a suggestion, and in no dogmatic spirit.

H. F. B.

### Phoronomy.

ABOUT thirty-five years ago, I had a conversation with the late Dr. Donaldson, a well-known Greek scholar of the time, in which we discussed the appropriateness of the use of the word kinematics, in the sense in which it was then, and is now, employed by writers on mathematical science. Dr. Donaldson's opinion was that it is not the best word which can be employed to represent the science of pure motion, without regard to causation. He said that the word *κινῆω* involved the idea of the cause of motion, and therefore that it ought not to be used when the idea of causation is to be completely set aside. He further gave it as his opinion that the word *φωρέω* is more nearly expressive of the idea of mere going, without any reference to the cause of motion, and therefore that the proper word would be phoronomy, or phoretics.

I was so much impressed by this conversation that, for many years, I headed with the word phoronomy the papers of questions on the subject of pure motion which I was in the habit of preparing for College lectures and private pupils.

I have recently consulted a very eminent Greek scholar, and his opinion is that, on the whole, the word phoronomy is more distinctly expressive of the science of pure motion than the word kinematics. He agreed with Dr. Donaldson that the word kinematics suggests some idea of causation, whereas no such idea is suggested by the word phoronomy.

As a matter of history, the word *cinématique* was introduced by Ampère to represent the purely geometrical science of motion in the abstract, and was anglicized into kinematics by, I think, the late Dr. Whewell.

Sir W. Thomson and Prof. Tait, in the preface to their "Natural Philosophy," adopt the suggestion of Ampère, and employ the word in the same sense. They also employ the word *dynamics* in its true sense as the science which treats of the action of force, whether it maintains relative rest, or produces acceleration of relative motion. They further state that these two corresponding divisions of dynamics are conveniently entitled *statics* and *kinetics*.

Here, then, we have two words, kinematics and kinetics, both derived from the same root-word, employed to represent two entirely different sets of ideas; and there is not the same broad line of demarcation between the words themselves as there is between the sets of ideas which they are intended to connote.

Hence it appears to me that the word phoronomy, the law of going, is the most suitable, as it is the most expressive word, to represent the science of pure motion in the abstract.

At the time of my conversation with Dr. Donaldson, we were neither of us aware that the word had been already invented and employed. Some years after, I found that a treatise was published at Amsterdam in 1716, entitled "Phoronomie, sive de viribus et motibus corporum. Autore Jacobo Hermann, Basil." Hermann, however, uses the word in much the same sense as we now use the word dynamics.

The point has been recalled to my mind by the discovery that the word has been employed in Germany in the sense in which Dr. Donaldson advocated that it should be used.

In the treatise entitled "Allgemeine Mechanik der Punkte und Starren Systeme," by E. Budde, published in 1890 at Berlin, the word phoronomy is adopted, and the author gives his reasons in the following words:—

"Man kann eine Ortsveränderung zunächst rein geometrisch, ohne alle Rücksicht auf ihre Ursachen, betrachten, und das soll in den nächsten Capiteln geschehen. Die Disciplin, welche sich mit dieser Betrachtung befasst, heisst *Phoronomie* oder *Kinematik*. Der Name Kinematik ist seit Resal der gebräuchlichere gewesen; neuerdings aber wird von Reulaux und seinen Schülern die Morphologie der Verknüpfung von Maschinentheilen als 'Kinematik' bezeichnet. Wir wählen deshalb hier den Namen Phoronomie."

With reference to this statement of Budde's, I observe that Ampère, in the "Essai sur la Philosophie des Sciences," particularly mentions trains of machinery, such, for instance as the works of a watch, as coming under the heading kinematics.

I also find that Grassmann, in the "Ausdehnungslehre von

1844," published at Leipzig in 1878, speaks of "Phorometrie" as representing "die reine Bewegungslehre"; and I see that Möbius uses the adjective in an article on the "Phoronomische Deutung des Taylor'schen Theorems."

A change in scientific nomenclature is by no means an unprecedented occurrence.

For instance, notwithstanding the great authority of Lagrange, the phrase "virtual velocity" has been practically superseded by the phrase "virtual work," and in the year 1876 the word "work" was substituted for "virtual velocities" in the regulations, published in the Cambridge University Calendar, for the Mathematical Tripos.

Another instance is the fact that the phrase *vis viva* has been superseded by *kinetic energy*, as a more convenient term in the expression of the principle of energy.

Further, I notice that Prof. Tait, in lectures on "Recent Advances in Physical Science," gives the suggestion that the time-honoured word force is in all probability destined, as science advances, to be relegated to the limbo of departed nomenclature.

For these various reasons, then, I trust that I shall not be regarded as an iconoclast, if I venture to substitute, for the word *kinematics*, the word *phoronomy*.

W. H. BESANT.

St. John's College, Cambridge, February.

### On the Terms "Centrifugal Force" and "Force of Inertia."

THE retention, in the last edition of Mr. Loney's "Elements of Dynamics," 1891, of a paragraph (p. 141) which resuscitates the objections formerly urged by some writers against the use of the term "centrifugal force" seems to call for a protest. It is to be regretted that students of dynamics should find absolutely contradictory statements presented to them respecting the validity of this term. While, however, in one set of text-books we find a perfectly clear definition and consistent use of the phrase "centrifugal force," there does not, on the other hand, appear to be unanimity of ideas amongst the objectors, nor always sufficient clearness in expressing the same.

In the uniform circular motion of a ball rolling on a table against the inner surface of a vertical cylinder, the pressure of the cylinder upon the ball is a centripetal force directed towards the centre of the circle. The contrary pressure of the ball upon the cylinder is the "centrifugal force," which is defined as the reaction to the centripetal in this case, and in every case as the reaction to the normal component of the centrifugal force.

The foregoing definition or usage of the term is adopted without hesitation or apology in the following works, named in order of date:—

Poisson's "Traité de Mécanique," 1833, vol. i. p. 332, or Harte's translation, 1842, p. 256.

Walton's "Mechanical Problems," 1842, pp. 240, 260, 269.

Prof. Niven in "Cambridge Senate-house Problems," 1877, p. 78.

Thomson and Tait's "Natural Philosophy," 1879, p. 221.

Garnett's "Elementary Dynamics," 1875, p. 205, and 1882, p. 255.

Routh's "Rigid Dynamics," Part I., 1882, p. 217, and Part II., 1884, p. 15.

Williamson and Tarleton's "Dynamics," 1889, p. 88.

Objections to the term appear in—

Goodwin's "Course of Mathematics," 1849, p. 275.

Parkinson's "Mechanics," 1863, p. 249.

Blakie's "Dynamics," 1887, p. 32.

Rankine, "Encyclopædia Britannica," "Mechanics." 9th edition.

Loney's "Statics and Dynamics," 1891, p. 141.

Other authors might have been cited, but I have referred to such as I happen to possess.

Nearly all these objectors evince the same reluctance to giving the name of "force" to the reactionary effect of the body's inertia in the direction of the normal outwards. Yet, if we admit that "to every force there is an equal and opposite reaction," it is not easy to escape from the conclusion that such a reactionary force exists.

Mr. Loney, however, postulates both forces, but adds:—"Centrifugal force is a very misleading term. It seems to imply that the force belongs to the mass instead of being an external force acting on the mass. A somewhat less misleading

term is centripetal force. We shall avoid the use of either expression; the student who meets with them will understand that either (*sic*) means the force which must act on a mass to give it the acceleration normal to the curve in which it moves."

These are confusing directions to the student, who must be left in complete bewilderment as to any distinction in meaning between "centripetal" and "centrifugal." "Centrifugal," from its derivation, signifies that the force has a tendency to make the body fly away radially from the centre. And such a tendency there is, and such a motion would result if we could make the centrifugal force last after the centripetal has ceased. But in the objections taken the word "tendency" is regarded as though it implied an actual subsequent motion in the direction of the tendency. A beginner is almost certain to fall into the error of imagining that, when the cord is slipped, the stone from a sling will dart away in a direction intermediate between that of the string and its own previous motion in the circle. But the name "centrifugal" is not answerable for this. The idea is due to the unmistakable pull upon his hand of an outward tending force, to which "centrifugal" merely gives the right name. Clearer conceptions show him that the two forces, the action and the reaction, cease at the same instant when the string is cut, and that there is no initial velocity in either direction.

Uniform circular motion is perfectly unique. In the direction of the force there is no motion, in the direction of the motion there is no force. The real *crux* lies in this conception of a constant acceleration with a perpetual zero velocity in the direction of the acceleration. How, says one, can there be a rate of change when the change itself is zero? But the objection is a metaphysical one, and it may be urged with equal force against the whole doctrine of limiting ratios.

Mr. Loney's statement that the centrifugal reaction is not a force belonging to the mass, but "an external force acting on the mass," requires some elucidation. Dr. Parkinson, in the paragraph referred to, has something similar. He says that the term "centrifugal force" "vaguely conveys an impression, as it were, that the particle of itself resisted curvilinear motion and exerted a force *per se* to move in a rectilinear path, which innate tendency was only overcome by the action of some external force." He also grudgingly recommends the student to use the obnoxious phrase "simply as an equivalent for the moving force in the direction of the normal." Here again "centrifugal" is made to signify a tendency towards the centre! Is not the vagueness complained of imported into the subject in some measure by the writers themselves?

Whatever names are employed, the facts are these. The force towards the centre communicates to the body an acceleration in that direction, which acceleration gives rise (we know not how, but we say by the law of inertia) to a force equal and opposite to the force which produced the acceleration. This reaction always appears to emanate from the mass of the moving body, and it has therefore been called "the force of inertia" of the body. Although this view has been combated by Poisson and others, some of the latest authorities are reasserting it. Thus in Thomson and Tait, 1867 and 1879, we find in Article 216: "Matter has an innate power of resisting external influences."

... This the inertia of matter, &c." Again, in Sir Robert Ball's "Experimental Mechanics," 1888, p. 252: "When any agent acts to set a body in motion or to modify its motion in any way, the body reacts on the agent, and this force has been called the kinetic reaction."

I cannot see any objection to designating this reaction "the force of inertia." It is a provisional term, which will serve our purpose until the nature of force is better understood. Poisson's argument against it, derived from our experience of friction, appears to me invalid, and his illustration irrelevant, because the law of resistance is not the same as in the case of inertia. If it had happened that the law governing friction was that the resistance to motion was directly proportional to the acceleration, then if a body were moving with constant velocity upon a rough plane there would be no resistance from friction. The smallest acceleration of velocity would give rise to a correspondingly small amount of friction, a double acceleration would double the resistance from friction, and so on, precisely as with the resistance from inertia.

GEORGE S. CARR.

### A Lecture Experiment in Surface Tension.

HOPING it may be of interest to some of your readers, I venture to send you the following description of a simple ex-